S. 455.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND THE ARTS:

ILLUSTRATED WITH ENGRAVINGS.

BY WILLIAM NICHOLSON.

VOL. IV.

JOURNAL

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AND THE ARTS:

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The King having procured fome Merino fheep of the firft character for the finenefs of their wool, and having caufed a number of judicious trials to be made, by which it is afcertained, during the courfe of above feven years, that the quality does not degenerate, but maintains all its original finenefs in this country; and the value of the wool being now known to our manufacturers, His Majefty has been pleafed to give away to different perfons, for experiments, more than one hundred rams and fome ewes: and ftill further to extend this valuable improvement in our ftaple commodity, he is now pleafed to permit fome ewes and rams to be fold at moderate prices. The management of this bufinefs is committed to the Right Honourable Sir Jofeph Banks, to whom letters may be addreffed.

Water flows out of a ciftern through the conical pipe of Venturi.   Into the throat, or fmalleft part of this pipe, is inferted an air tube, proceeding from a veffel in a more elevated fituation.   The confequence of this arrangement is that the air in the upper veffel is rarefied, and water rifes into that veffel through a vertical pipe.   As foon as the veffel is nearly filled, its contents are delivered by an apparatus, which is contrived to perform the requifite alternations without attendance.

Short enumeration of the facts.   General laws of the theory of Refractions, near the place of contact and admixture of fluids differing in denfity.   Experiments to confirm the theory, by including fyrup and water and alcohol in the fame veffel; by means of which double images and inverfion of objects are produced.   Other fluids.   Hot water carefully placed upon cold.   Heated air in the vicinity of red hot iron.   Obfervations on the ordinary effects of folar heat.   Experiments and obfervations with ether, alcohol, and water, to fhew how far evaporation is concerned in thefe effects.

The fulphate of lime of Montolier is coloured by a red oxide of iron, which, by treatment with charcoal, affords a fulphuret of iron, in which the metal is lefs oxided, and which, by folution in acids, and precipitation by a pruffiate, affords not Pruffian blue, but a green depofit of a different nature; which fulphuret preferves its blue colour in pot-afh even at the heat of fufion.   A
                                                                                    fimilar

fimilar combination is produced from fulphuret of iron prepared in the direct way. The co-louring matter of lapis-lazuli is found by every experiment to be this blue fulphuret, and nothing elfe.

Account of the effects of piles confifting of zinc, gold, filver, iron, copper, lead, tin, and mer-cury, feverally combined with each other in pairs. Explofion of the gafes. Similar piles were found to act moderately well in atmofpheric air; better in oxygen gas; and not at all in azote. The refpirable gafes were diminifhed.

Board of Longitude of France. Prize of Aftronomy.---Tranflation of a Note of Cit. G. Cuvier, relative to Foffil Subjects of Natural Hiftory.---Hydraulic Engine operating by Mercury.---Phi-lofophical Tranfactions of the Royal Society of London for the Year 1800. Part the Firft.---Royal Society of Copenhagen.---Decompofition of the fixed Alkalis.---Separation of Butter.---Beet Sugar and ardent Spirits.---Fourcroy's Synoptic Tables of Chemiftry.---Note from a Cor-refpondent refpecting the Bolognian Phofphorus.---Extract of a Letter from Mr. William Henry, dated Sept. 25, 1800, to correct an Inference in his Paper on Galvanifm.

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D E C E M B E R 1800.

Engravings of the following Objects: 1. Conftruction of a Wheel to reprefent the Planetary Motions. And, 2. Method of graduating the Hydrometer, fo as to indicate the Strength of Spirit by Infpection at any Temperature.

Whether the primitive colours be compofed of three original tints or more? cannot be determined by mixture of powders, &c. That light of one colour is not all of the fame refrangibility, is inferred from the extent of each colour on the fpectrum. Probability that red, yellow and blue are the primitive colours, becaufe any two of thefe adjacent to each other will compofe the intermediate tinge; though thefe fuppofed originals cannot be fo compofed of the others, Theoretic obfervations, experimental teft of this doctrine by coloured glaffes. Ocular fpectra. Hypothefis of three colours will folve the phænomena of chromatics.

Sulphuric acid excites the pile very little, but becomes active by the addition of water. Liquid fulphurets are equal in power to water. Nitric and fulphuric acids act in vacuo. Obfervations, &c. Enquiries to determine what happens in the pile itfelf. Examination of the power of different Galvanic combinations.

JANUARY 1801.

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FEBRUARY 1801.

Engravings of the following Objects: 1. An improved Hydraulic Engine, by Mr. Close. 2. The Duke of Bridgewater's underground inclined Plane. 3. Figures illustrative of Galvanic Discoveries. And, 4 Mr. Home's Exhibition of the Structure of that singular Animal the Ornithorhynchus paradoxus.

The method consists in boring and then driving a wooden pipe into the cavity. Through this pipe the borer is made to act and increase the depth. Another pipe is driven in so as to sink the first still lower. By a continuation of this process the length of pipe is carried to very great depths, if necessary, and water is conducted from the lower springs to the surface.

VI. Some

M A R C H 1801.

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ARTICLE I.

On the Antiquity of the Art of Etching on Glafs. By Mr. FRED. ACCUM.

LOOKING over a parcel of old foreign newfpapers, I was not a little furprifed to obferve that the art of etching on glafs had been known and practifed amongft our anceftors upwards of a century ago.

In order that every one may judge for himfelf, whether this difcovery belongs exclufively to Scheele, the Swedifh chemift, I fhall tranfcribe an advertifement inferted in an old German periodical publication *, which is likewife copied into Beckman's Collections towards a Hiftory of Inventions. The original advertifement, literally tranflated, runs thus: " *The difcovery of a powerful acid, by means of which every imaginable kind of figures* " *may eafily be etched upon glafs.*

" Take fpiritus nitri per deftillationem, put it into a retort, and apply a ftrong heat. " When it has paffed over into the receiver, throw into it fome powdered *green Bohemian* " *emerald* (which, when heated, fhines in the dark), otherwife called *hefphorus*. This " being done, place the receiver, containing the mixture, on a heated fand-bath, for about " four-and-twenty hours, and it will be fit for the purpofe. To ufe this corrofive acid,

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* Breflawer's Collections for the year 1725: January. This receipt is likewife inferted by Krunitz in his Economical Encyclopedy; XI. p. 678.

" take a pane of glafs of any kind, clean it well, and free it from greafe, by wafhing with a
" ley, and when dry, trace out upon it, with fulphur and varnifh, whatever you choofe.
" Put a border of bees'-wax round it, about one inch high, and then pour the corrofive
" acid, prepared as before directed, carefully over the whole furface of the glafs, and let it
" ftand undifturbed for fome time, the longer the better. The glafs will become corroded,
" and all you had traced before, will now appear as if raifed, or elevated, above the furface
" of the glafs, in a very diftinct and pleafing manner."

This advertifement is figned *I. G. Wigand*, and dated from *Goldingen*, in Courland. In
a fubjoined note, the author of it confeffes that he is not the inventor of the corrofive acid,
but that he had the receipt from *M. Pauli*, a famous phyfician, refident at Drefden, and
lately deceafed. He further afferts, that the late doctor, during his life-time, had etched
on glafs feveral curious crefts, names, and figures, in a manner no lefs beautiful than fur-
prifing.

In a fupplement to the before mentioned publication, the fame author very earneftly en-
deavours to convince the public, that the art of etching on glafs he had made publifhed fome
time before, was very different from the art of engraving on glafs by means of emery,
flint, red-hot iron, or other fharp pointed inftruments of the hardeft kind. Nor is it that
art, fays he, by which Mathefius, the famous apothecary, operated, who formed elevated,
or engraved figures, feftoons, and other ornaments on glaffes, drinking cups, &c. for all this
I know very well is done by means of diamond pencils. But the method I have made public,
feems to be, in fact, the very fame admirable and fecret art which was formerly practifed
by a fkilful artift of Nuremberg, who worked in that art folely for the emperor Charles II.

On purfuing this fubject a little further, there remains, no doubt, but that this old artift
of Nuremberg, to whom our author alludes, is the perfon of whom Doppelmair * gives
the following account : " Amongft the artifts of Nuremberg muft be enumerated *Henry*
" *Swanhard*, a fkilful engraver in fteel and ftone, who found out by the glafs of his
" fpectacles, upon which fome corrofive liquid had fallen, a new art of etching upon glafs,
" which difcovery he made in the year 1670." Wagenfeil †, on the contrary, relates, that
the inventor derives more merit from his difcovery, than accident entitled him to claim.
" Swanhard," fays he, " by the acutenefs of his genius, proved what was hitherto deemed
" impoffible. He was the firft who ever compofed a corrofive, fo active as to diffolve the
" hardeft cryftal glafs, which hitherto refifted the force of the moft powerful fpirits of the
" apothecaries. By means of this corrofive, Swanhard delineated and etched on glafs
" figures of men, fome naked and fome dreffed, and all kinds of animals, flowers, plants,
" trees, and fhrubs, in a manner perfectly natural, and perfectly eafy." The fame is men-
tioned in Schneider's Biography of the Artifts of Germany, 1740, p. 37.

How the above artift prepared his corrofive is not mentioned by any of the above
authors. It is, therefore, very probable, that he kept his art a fecret, as it feems that the

* Doppelmair's Hiftory of the Artifts of Nuremberg, 1734, p. 16.
† Wagenfeili Comment. de Civitate Norimbergenfi, Altdorf, 1697. 4to. p. 154.

receipt

receipt for that purpoſe was made public, for the firſt time, in January, 1725*, by the before mentioned M. Pauli. How he came to know it, I have not been able to learn.

That the inventor employed his corroſive to a purpoſe different from that for which it is uſed at preſent, is obvious from the effect it produced upon the glaſs. At preſent the glaſs is covered either with wax, iſinglaſs, or a varniſh, and thoſe figures which are intended to be etched, are traced through this covering, by means of a pointed inſtrument; but our ancient artiſt covered his figures, when traced, with a varniſh and ſulphur, and then applied the acid to corrode the glaſs around them, ſo that the figures which remained ſmooth and clear appeared, when the varniſh was removed, as if raiſed from a dim or dark ground. He adopted this method probably in order to render his invention more palpably different from the art of cutting the figures on the glaſs, as if engraven, long known and practiſed before this diſcovery was made.

That the Bohemian emerald, or heſphorus, made uſe of in this receipt, was our fluate of lime, can hardly be doubted. In old German authors who treat on the working of mines, this ſtone is claſſed amongſt the fluſſe (fluxes) becauſe it was an uſual ſubſtitute to accelerate the fuſion of certain ores. *Agricola*, who changed the German names of quartz, zinc, biſmuth, cobald, &c. into quartzum, zincum, &c. changed fluſs into fluor, which became afterwards very common †. If a paſſage of the ancients can be quoted that ſeems to allude to fluate of lime, it is that of Theophraſtus, where he ſays, that there are certain ſtones which, when added to ſilver, copper, and iron ores, become fluid. For Cronſtadt was the firſt ſyſtematic writer (if I am not miſtaken) who mentioned fluate of lime as a particular genus.

That the ſtone called emerald, or heſphorus, was really our fluor ſpar, becomes obvious from what Kirchmair, profeſſor at Wittemberg, in Germany, remarks, who was probably the firſt who made known what kind of ſtone it was, which had the peculiar property of ſhining in the dark after having been heated‡, and which by the ancients was ſought for

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* Breſlawer's Collections, l. c.

† Lapides ſunt gemmarum ſimiles, ſed minus duri, fluores liceat mihi verbum an e verbo exprimere, noſtri metallici appellant, nec meo judicio, inepte; ſiquidem ignis calore, ut glacies ſolis, liqueſcunt et fluunt. Varii autem et jucundi colores eis inſident.

<div align="right">Bergmannii Opuscul, p. 466.</div>

‡ Ante annos pacuos admodum inventus mineralis alicujus viſu et proprietatibus in quibuſdam ſimilis ſmaragdo, ab artificibus duobus mihi per amicè notis uſus eſt. Conficiendi phoſphori et repræſentandi modus levis atque brevis ille. Recipe q. v. mineralis viridis ſmaragdum pene referentis; contare in pulverem, aqua madeſac communi, pulvis inſtar ut fiat. Pencillo poſtea in lamina cuprea, magnitudine vel majoris plani alicujus manubrio inſtructa literas, quaſcunque voles, in lamina deſcribe craſſiuſculas. Ardentibus ſuper impone prunis vaſculo exceptis. Phœnomenon ſpectabis in obſcuro amæniſſimum ſine ſume et odore lucens. Sed ut verum fatear, nec uſum, niſi curioſi animi explendi, artificium hoc, neque diuturnitatem habet. Pectus ergo avidum ſciendi meliora fatiari nequit ſive Heſphorus vocetur ſive Veſperugo.

G. C. Kirchmaieri De Phoſphoris et natura lucis, nec non de igne, commentatio epiſtolica. Wittembergae, 1680, p. 7.

<div align="center">B 2</div>

<div align="right">with</div>

with fo much avidity. Neither need I to mention that the nitrous acid made ufe of in the receipt was equally capable of difengaging the fluoric acid, though we ufually make ufe of the fulphuric.

There remains, therefore, little doubt, that the art of etching on glafs does not entirely belong to the moderns, or to deny that the ancients were altogether unacquainted with it, would be doing them an injuftice. It feems rather, that this art belongs to the difcoverics which were made in thofe times, in which men were little inclined to tranfmit an account of their inventions to pofterity, and thus this art muft have been forgotten or loft. Scheele * difcovered the fluoric acid, and re-invented the art of etching on glafs in the year 1771. But had Swanhard been able to purfue properly what either accident, or ingenuity prefented to him, he might have enriched the arts, with a difcovery which acquired great reputation to this Swedifh philofopher one hundred years after.

<div align="right">FREDERIC ACCUM.</div>

II.

A Defcription of a new Inftrument called the Blaft Ventilator. Invented by J. W. BOSWELL.

IT would be fuperfluous to offer any arguments to the cultivators of natural philofophy, refpecting the importance of any difcovery in that fcience. It is fufficient to announce it to thofe who fo well know, that no operation of nature is unimportant; and that although the firft perfon to whom a fact may occur, may not be able to point out all the advantages which may arife from it, yet others of more difcernment coming after him, may improve upon his ideas, and perhaps ufe it for purpofes of much higher importance.

What Dr. Hales has written of the great confequence of the fubject of ventilation to health, and the prefervation of the food of man, renders it unneceffary to expatiate on the importance of any new difcovery in this art; and to his excellent treatife on this matter, I beg leave to refer thofe who have any doubts on the advantages arifing from it.

The method of forcing air in any required direction, by a fall of water paffing from a fmaller into a larger tube, in an engine called the † Water Blaft, is now well known, and frequently applied in mines and furnaces with great efficacy.

Some confiderations on this machine led me to conjecture long fince, that a current of air might be made to act inftead of a fall of water, in an apparatus conftructed on principles fomewhat fimilar to the above, and thus applied fo as to draw off foul air or fmoke from any places required, in fituations where the other could not be ufed. It was only lately that I have been able to think of any method of putting this matter to the teft of ex-

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* Tranfactions of the Swedifh Academy for the year 1771. C. xxxiii. p. 122.

† Dr. Lewis, in his very ufeful work on the Arts, has laid down the beft conftruction of this engine, from actual experiment.

<div align="right">periment,</div>

periment, which would not be attended with too much inconvenience. This I have effected by the instrument of which I now send the draft and description; and I found the result of the experiments made with it, fully to answer my expectations.

In the draft the first figure represents the external appearance of this instrument, and the second shews it in section. Plate 1. From A to B in fig. 2, there is a free open communication in the larger tubes. The part C E D is an open truncated cone, ending in a tube E F, one third of the diameter of that which surrounds it, and which reaches into the larger tube the length of two of the diameters of the latter.

There it a scale with the draft for fig. 1 and 2, which marks the proportion of the small instrument I had first made. But as the size of any other must vary according to the uses for which it is intended, I have only set down the number of proportional parts, and have not mentioned their value, which may be encreased at pleasure.

In using this instrument, the base C D of the cone must be turned towards the wind, which when it blows even moderately, will then cause a very sensible current of air to pass up from A to B through the larger tubes. The small instrument, which I tried at a time when the wind was low, when placed as above directed, opposite the opening of the upper sash of a window let down about two inches, acted with such power, as to draw the flame of a candle considerably into the tube A, held horizontally at some distance from it, and made the tube B, by which I supported it, so hot in a short time, that it was painful to hold it.

This experiment I repeated before many gentlemen always with the same result; and which at least entitles the instrument to a trial on a larger scale for useful purposes, in which I have no doubt of its succeeding.

The uses to which this instrument may be applied, are of no small importance, even so far as they have occurred to me; others may yet be found out which I have not thought of, those which I have thought of are as follows:

1st. It will do extremely well for extracting the unwholesome air from mines, when it is made of a size sufficiently large.

2d. It will also be particularly convenient in ships for changing the air in them, rendered unfit for respiration by the breath and perspiration of a numerous crew, by the effluvia from provisions, bilge water, &c.; and if its efficacy may be judged of in this respect, from experiments made on a small scale, it would with a moderate wind entirely change the air in a large vessel in the course of two or three hours, even when made of no great size.

3d. It will considerably encrease the force of the draft in a blast furnace, if placed on the top of its chimney as hereafter directed, for chimneys in general.

4th. It will serve as a ventilator for corn, stores, magazines, hospitals, and crowded rooms.

5th. It is particularly well calculated to prevent or cure the smoking of chimnies, (which inconvenience frequently renders houses so uncomfortable, as to induce the owners to quit

them;

them; to say nothing of the injurious effects of smoke received into delicate lungs. One cause especially of smoking chimneys it removes, for which no effectual cure has been before discovered.

Dr. Franklin, to whom the world is so much indebted for his many useful discoveries in natural philosophy, and the arts, has given two very valuable papers on the subject of smoking chimnies, in the Transactions of the Philadelphia Society. In these he has recited nine causes, which occasion chimneys to smoke, amongst which that of high winds blowing the smoke down the funnel is a chief one: for all which he has proposed effectual remedies; except for this last mentioned alone, for which, he acknowledges that there was no certain remedy discovered; we may therefore look on such a remedy as an object the more valuable, as it compleats what the Doctor began: such a remedy is the instrument described in this paper, which by removing this evil leaves no defect incident to chimneys that cannot be obviated; and it is so effectual for this purpose, that from the very principles of its construction, the stronger the wind blows, the better will the chimney draw, thus changing what was formerly an evil into an actual benefit.

The manner in which the instrument should be used for this purpose, is one with which we are already familiar. It should be placed on the top of the chimney, in the same manner in which the contrivance, (intended for the same purpose, but very ineffectual) called the Boar's Head, is usually put up: and should like it be supported on a pivot, so as to turn round with its back to the wind, that the blast might fully enter its conical opening.

To prevent the external air from being drawn up between the turning tube and the fixed one, when placed on the chimney, some soft leather, or canvas, should be fastened round the outside of the lower end of the turning tube, which surrounds the fixed tube; this leather or canvas, which should hang down two or three inches below the interval between the tubes, will act as a species of valve to prevent the outer air from entering, and will thus cause the whole effect of the instrument to operate in increasing the draft of the chimney.

There is another method of effecting this last purpose, more certain in its mode of acting, but which from its requiring a much greater expence, cannot be recommended for general use; this is represented in fig. 3. which figure is on a smaller scale than the other two, and is only intended to shew the best method of fixing the instrument on a chimney: it is drawn in section to exhibit the internal structure. In it A represents the fixed part to be bedded in mortar on the top of the chimney in the usual manner. B represents the moveable part, which turns with the wind, so that the conical opening G shall be always opposite to the blast. E is the upright rod on whose point B turns round; it is fastened below to the two bars D D, which are fixed across in the lower immoveable tube A, and passes through a collar in the bar H, which is fastened at its extremities to the moveable tube B. By this means the rod preserves its perpendicular position; and the part of the moveable tube, which surrounds the upper part of the fixed one, will revolve round it very close without coming in contact in any part.

C C

C C is an external tube, clofely foldered or cemented to the fixed tube A, fo that, with its upper part, it may form a deep groove of four or five inches, in which the lower extremity of the moveable tube may freely revolve: this groove fhould be filled with mercury, high enough to cover the bottom of the moveable tube about half an inch; which contrivance will effectually prevent the outer air from being drawn in between the tubes, without impeding the motion of the upper one in the leaft. F F is a fmall projection faftened to the moveable tube, to prevent the rain or duft from falling into the groove.

The Blaft Ventilator would be particularly ferviceable to fhip chimnies above all others. Any one who has ever been at fea, muft have obferved the difficulty of preventing fhip-chimnies from fmoking when the wind rifes to any height, and what an advantage any contrivance would be that would prevent this inconvenience; and that this inftrument would be quite effectual for this purpofe, (there is every reafon to believe from the experiments already made) and even make thofe chimnies draw better as the wind blew harder.

The inftrument reprefented in the two firft figures would be the fitteft for this purpofe, any of the men on deck could eafily fhift it about as the wind changed; and as this way of ufing it might be made for a few fhillings, the expence will be no object to prevent fhip-owners from giving it a fair trial.

It is fcarcely neceffary to mention, that it would do equally well for the fame purpofe in canal paffage-boats, which are often as much incommoded by fmoke as fhips are; a mode of conveyance to which in fome fituations, elegance is added to eafe and cheapnefs, and which well deferves farther extenfion on the Englifh canals.

The manner in which the wind operates in this apparatus, to caufe a current of air through the upright tube, is moft probably the following:

When the conical opening C D, Fig. 2, is turned to the wind, the portion of the current of air which enters this opening, will be compreffed againft the fides of the cone, as it paffes on, more and more, till it comes to E; and after it has paffed F, the extremity of the fmall tube, it will begin to expand itfelf; but as it moves forward while thus expanding, the union of the two motions will caufe it to form the figure of a cone or conoid, fomewhat fimilar to that by which it entered, which is marked by the dotted lines terminating at G and H.

This conical blaft ftriking obliquely the air before it in the tube B, will caufe it to move forward only, its lateral motion being prevented by the fides of the tube; and the preffure of the atmofphere forcing the air forward at A, to replace what has been removed at B by the conical blaft, will of courfe caufe the current of air from A through B already mentioned.

In fituations where coals may be had cheap, fteam may be made to operate on the blaft ventilator inftead of wind, for many ufeful purpofes, by a contrivance fomewhat fimilar to the eolipile; with fome alteration as to its form, but preferving the fame principle; particularly fteam might be ufed with this inftrument fo as to force air on a blaft furnace, and

would

would do beſt for ſmelting iron, where ſome degree of dampneſs in the blaſt is rather beneficial.

To deſcribe the apparatus minutely for this purpoſe, would ſwell this paper to too great a bulk; but of its poſſibility I could eaſily convince any one, and would engage to conſtruct it, in ſuch a ſituation as before mentioned, at an expence comparatively trifling to any other mode of cauſing a blaſt, where a fall of water could not be procured.

Even where fuel is not very cheap, yet ſteam may be applied to the purpoſe of ventilation with this inſtrument, in the manner hinted at here, to great advantage, in thoſe ſituations where labour is difficult to be procured, or inconvenient to be uſed; or where habits and prejudice operate ſtrongly againſt working manual ventilators. This laſt is too frequently the caſe aboard men of war, and other ſhips, though in no other place is ventilation ſo neceſſary to preſerve health, and ſecure proviſions from decay.

An apparatus for ventilating ſhips in this manner might eaſily be fixed beſide the ſhip's boiler, to be uſed in calms, when the wind would not operate on the inſtrument before deſcribed, and which at other times might be applied to boil water for various uſes; and the ſhip's boiler itſelf might be ſo adjuſted as to ſerve for this purpoſe, without preventing or impeding its uſe in preparing proviſions.

To conclude, I will juſt mention a matter this ſubject has ſuggeſted of ſome importance, though ſomewhat differing from what has already been treated of.

From obſerving the effects of the conical opening in the blaſt ventilator, it occurs, that walls might be ſo conſtructed about a windmill, as to operate in a ſimilar manner, and that thus the wind might be conducted with increaſed force, to operate in any direction required on a fixed wind wheel, though it blew from various directions; and that a windmill thus conſtructed, would have the additional advantage of being capable to be regulated or ſtopped at pleaſure, in a manner ſomewhat ſimilar to a water-mill. This mode of conſtructing thoſe mills would prevent many fatal accidents; and would not coſt ſo much as might at firſt ſeem, as the granaries, and other buildings annexed to them, might be ſo arranged, as in a great meaſure to anſwer the purpoſe of the walls propoſed to be built *.

III.

Experiments on Whinſtone and Lava. By Sir JAMES HALL, *Bart. F. R. S. & F. A. S. Edin* †.

T HE experiments deſcribed in this paper were ſuggeſted to me many years ago, when employed in ſtudying the *geological* ſyſtem of the late Dr. Hutton, by the following plauſible objection, to which it ſeems liable.

* On the ſubject of this paper, ſee Phil. Journ. I. II. Art. Venturi. N.
† Read before the Royal Society of Edinburgh, and communicated by the author. An abridged account of the contents of this paper was given in our Journal II, 285.

Granite,

Granite, porphyry, and basaltes, are supposed, by Dr. Hutton, to have flowed in a state of perfect fusion into their present position; but their internal structure, being universally rough and stony, appears to contradict this hypothesis; for the result of the fusion of earthy substances, hitherto observed in our experiments, either is glass, or possesses, in some degree, the vitreous character.

This objection, however, loses much of its force, when we attend to the peculiar circumstances under which, according to this theory, the action of heat was exerted. These substances, when in fusion, and long after their congelation, are supposed to have occupied a subterraneous position far below what was then the surface of the earth; and Dr. Hutton has ascribed to the modification of heat, occasioned by the pressure of the superincumbent mass, many important phenomena of the mineral kingdom, which he has thus reconciled to his system.

One necessary consequence of the position of these bodies, seems, however, to have been overlooked by Dr. Hutton himself; I mean, that, after their fusion, they must have cooled very slowly; and it appeared to me probable, on that account, that, during their congelation, a crystallization had taken place, with more or less regularity, producing the stony and crystallized structure, common to all unstratified substances, from the large grained granite, to the fine grained and almost homogeneous basalt. This conjecture derived additional probability from an accident similar to those formerly observed by Mr. Keir, which had just happened at Leith: a large glass-house pot, filled with green bottle glass in fusion, having cooled slowly, its contents had lost every character of glass, and had completely assumed the stony structure.

These views made part of a paper which I had the honour of laying before this Society in 1790*; and about the same time I determined to submit my opinions to the test of experiment. I communicated this intention to all my friends, and in particular to Dr. Hutton; from him, however, I received but little encouragement. He was impressed with the idea, that the heat to which the mineral kingdom has been exposed was of such intensity, as to lie far beyond the reach of our imitation, and that the operations of nature were performed on so great a scale, compared to that of our experiments, that no inference could properly be drawn from the one to the other. He has since expressed the same sentiments in one of his late publications, (*Theory of the Earth, vol. I. p.* 251.), where he censures those who " judge of the great operations of the mineral kingdom, from having " kindled a fire, and looked into the bottom of a little crucible."

But, notwithstanding my veneration for Dr. Hutton, I could not help differing from him on this occasion: for, granting that these substances, when in fusion, were acted upon by a heat of ever so great intensity, it is certain, nevertheless, that many of them must have congealed in moderate temperatures, since many are easily fusible in our furnaces; for it

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* Particular reasons induced me not to publish this paper at full length; but, wishing to preserve a record of some opinions peculiar to myself which it contained, I introduced a short abstract of it into the History of the Transactions.

is impoſſible that a ſubſtance ſhould congeal at a higher point than that at which it may afterwards be melted. If, then, theſe phenomena depend upon the circumſtances of congelation, the imitation of the natural proceſs is an object which may be purſued with rational expectation of ſucceſs: and could we ſucceed in a few examples on a ſmall ſcale, and with eaſily fuſible ſubſtances, we ſhould be entitled to extend the theory, by analogy, to ſuch as, by their bulk, or by the refractory nature of their compoſition, could not be ſubjected to our experiments. It is thus that the aſtronomer, by obſerving the effects of gravitation on a little pendulum, is enabled to eſtimate the influence of that principle on the heavenly bodies, and thus to extend the range of accurate ſcience to the extreme limits of the ſolar ſyſtem.

Encouraged by this reaſoning, I began my projected ſeries of experiments in the courſe of the ſame year (1790), with very promiſing appearances of ſucceſs. I found that I could command the reſult which had occurred accidentally at the glaſs-houſe; for, by means of flow cooling, I converted bottle glaſs, after fuſion, into a ſtony ſubſtance, which again, by the application of ſtrong heat, and ſubſequent rapid cooling, I reſtored to the ſtate of perfect glaſs. This operation I performed repeatedly with the ſame ſpecimen, ſo as to aſcertain that the character of the reſult was ſtony or vitreous, according to the mode of its cooling.

Some peculiar circumſtances interrupted the proſecution of theſe experiments till laſt winter, when I determined to reſume them. Deliberating on the ſubſtance moſt proper to ſubmit to experiment on this occaſion, I was decided by the advice of Dr. Hope *, well known by his diſcovery of the earth of ſtrontites, to give the preference to whinſtone.

The term whinſtone, as uſed in moſt parts of Scotland, denotes a numerous claſs of ſtones, diſtinguiſhed in other countries by the names of baſaltes, trap, wacken, grünſtein and porphyry. As they are, in my opinion, mere varieties of the ſame claſs, I conceive that they ought to be connected by ſome common name, and have made uſe of this, already familiar to us, and which ſeems liable to no objection, ſince it is not confined to any particular ſpecies †.

The following experiments were performed with various kinds of whinſtone, and have likewiſe been extended to lava. To inveſtigate the relation between theſe two claſſes of ſubſtances, ſeems, in the preſent ſtate of geology, an object of conſiderable importance; for they reſemble each other in ſo many reſpects, that we are naturally led to aſcribe the

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* In the courſe of laſt winter, when I firſt thought of reſuming my experiments, I propoſed to this gentleman, that, in imitation of a practice, common in the Academy of Sciences of Paris, we ſhould perform them in company. To this propoſal he cheerfully agreed; but, before any experiments had been begun, he found himſelf ſo much occupied by profeſſional duties, that he could not beſtow upon the ſubject the time which it neceſſarily required; and we gave up the idea of working in company.

† In characteriſing the particular ſpecimens, I have adopted, with ſcarcely any variation, deſcriptions drawn up by Dr. Kennedy, whoſe name I ſhall have occaſion frequently to mention in the courſe of this paper. In the employment of terms, we have profited by the advice of Mr. Deriabin, a gentleman well verſed in the language of the Wernerian ſchool.

formation

formation of both to the fame caufe, and to believe that whinftone, as well as lava, has been expofed to the action of heat. In the courfe of the paper, I fhall mention feveral accidental refults, which, if confidered feparately, might feem unworthy of notice, but which, by affording the means of comparifon between the two claffes, are of great fervice in the general inveftigation.

The whinftone firft employed was taken from a quarry * near the Dean, on the water of Leith, in the neighbourhood of Edinburgh. This ftone is an aggregate of black and greenifh-black hornblend, intimately mixed with a pale reddifh-brown matter, which has fome refemblance to felfpar, but is far more fufible. Both fubftances are imperfectly and confufedly cryftallized in minute grains. The hornblend is in the greateft proportion; and its fracture appears to be ftriated, though in fome parts foliated; that of the reddifh-brown matter is foliated. The fracture of the ftone *en maffe* is uneven, and it abounds in fmall facettes, which have fome degree of luftre. It may be fcratched, though with difficulty, by a knife, and gives an earthy fmell when breathed on. It frequently contains fmall fpecks of pyrites.

On the 17th of January 1798, I introduced a black lead crucible, filled with fragments of this ftone, into the great reverberating furnace at Mr. Barker's iron foundery. In about a quarter of an hour, I found that the fubftance had entered into fufion, and was agitated by a ftrong ebullition. I removed the crucible, and allowed it to cool rapidly. The refult was a black glafs, with a tolerably clean fracture, interrupted however by fome fpecks.

In fubfequent experiments, I endeavoured, by flow cooling after fufion, to prevent the whinftone from becoming vitreous, and to compel it to refume its original character by cryftallization. In this I fo far fucceeded as to obtain a fubftance, which was not glafs, though it did not poffefs the properties of whinftone. The production of this intermediate fubftance, which much refembled the liver of an animal, is accompanied with fome curious particulars, which I fhall enumerate and explain in another part of this paper. On fome occafions, too, I obtained a vitreous mafs in which were a multitude of little fpheres, having a dull or earthy fracture.

At laft, on the 27th of January, I fucceeded completely in the object I had in view. A crucible, containing a quantity of whinftone, melted in the manner above defcribed, being removed from the reverberatory, and conveyed rapidly to a large open fire, was immediately furrounded with burning coals, and the fire, after being maintained feveral hours, was allowed to go out. The crucible, when cold, was broken, and was found to contain a fubftance, differing in all refpects from glafs, and in texture completely refembling whinftone. Its fracture was rough, ftony and cryftalline; and a number of fhining facettes were interfperfed through the whole mafs†. The cryftallization was ftill more apparent in

cavities

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* Called Bell's Mills Quarry.

† It contained a number of fmall globules like lead fhot, which were found to confift of regulus of iron, reduced from the oxide in the whinftone, by means of the carbonic matter of the black lead crucible.

By

cavities produced by air bubbles, the internal furface of which was lined with diftinct cryftals *.

Having fhewn this refult to feveral of my friends, Dr. Hope regretted that the fubftance, previoufly to its artificial cryftallization, had not been reduced to the ftate of folid glafs; fince the adverfaries of the fyftem might alledge, that, during the action of heat, the original cryftallized texture of the ftone had never been completely deftroyed. Being convinced of the propriety of this obfervation, I determined, in future, to reduce the ftone firft to glafs, and to perform the cryftallization after a fecond fufion.

For this purpofe, with the affiftance of Dr. Kennedy, to whofe co-operation I am greatly indebted for the fuccefs of all the following experiments, I reduced a quantity of the fame whinftone to moft perfect black glafs. A crucible, filled with fragments of this glafs, being then expofed to a heat, which, from previous trials, was judged to be more than fufficient to reduce its contents to fufion, the fire was very gradually lowered till all was cold. I thus expected to obtain a refult fimilar to that laft mentioned, but found, to my great furprife, that the fragments had never been in complete fufion, fince they ftill, in a great meafure, retained their original fhape. This extraordinary fact, which afterwards led to the difcovery of fome curious properties of whinftone, will be fully accounted for in a fubfequent part of the paper.

Another portion of the fame glafs being perfectly melted by a very ftrong heat, the temperature was reduced to about 28 of Wedgwood, and was maintained at that pitch during fix hours. The refult was a perfectly folid mafs, cryftallized to a certain depth from the outfide, though ftill vitreous in the heart. In another experiment, performed like the laft in all refpects, except that the heat was maintained at 28 during twelve hours, I obtained a mafs entirely cryftalline and ftony throughout, with facettes appearing in the folid parts, and fmall cryftals fhooting into fome of the cavities.

Soon after I had communicated thefe refults to Dr. Hope, he performed, with complete fuccefs, an experiment fimilar to the firft, in which I had obtained a cryftallized fubftance, by the gradual cooling of the melted ftone. The fame was likewife, foon afterwards, performed by Mr. Bofwell of Auchinleck.

My experiments, already defcribed, were confined to one fpecies of whinftone; but have fince been extended to fix other varieties. They were all firft reduced to glafs by the application of a ftrong heat, and fubfequent rapid cooling. After a fecond fufion they

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By this reduction, as a portion of its iron was withdrawn from the mafs, and as the relative proportion of the component elements was thus varied, the nature of the original fubftance could not fail to be changed. I determined, on this account, to lay afide black lead crucibles in future. The propriety of this has fince been fully proved; for the firft refult is found to differ effentially from thofe obtained in all my fubfequent experiments, which were performed with Heffian crucibles, or with others, which, like them, contain no fenfible quantity of carbonic matter.

* I fhowed this refult at a meeting of the Society on 5th of February.

were cryftallized, by being kept long in a ftationary temperature, between 28 and 30. This laft operation was beft performed in a long and narrow muffle, wholly furrounded with burning coals, according to a practice long followed by Dr. Kennedy, by which the heat could be maintained with fo great fteadinefs as to render the refult almoft certain.

The fufibilities were determined in an open muffle, in which a fragment of the fubftance under trial was placed contiguous to a pyrometer piece. As foon as the fragment, in confequence of the gradual rife of heat, had fo far foftened as to yield to the touch of a bent iron rod, the pyrometer was removed and meafured. The fufibilities, thus obtained, in degrees of Wedgwood's fcale *, have been ftated in a table, to which I would be underftood always to refer. I have diftinguifhed the cryftallized fubftances, obtained from the glaffes, by the name of *cryftallite*, a term fuggefted by Dr. Hope. It may be obferved in this table, that the original whins foften in a range from 38 to 55; the glaffes from 15 to 24, and the artificial cryftallites from 32 to 45.

No. 1. *Whin of Bell's Mills Quarry.*

This ftone was the fubject of all the foregoing experiments, which were frequently repeated with fuccefs on a large fcale.

In trying the fufibility of the glafs obtained from it, a curious circumftance occurred, which accounts for the unexpected refults already mentioned. I had placed in the muffle a long and flender fragment of this glafs, with its extremities refting on two fupports of clay, and its middle unfupported. Having then increafed the temperature by flow degrees, I expected to difcover the loweft point of emollefcence, by obferving when the fragment funk by its own weight. The muffle having attained a moderate heat, I obferved the glafs to lofe its fhape a little. Wifhing to fee it completely melted, the fame heat was continued, but no further change took place. The heat was then raifed feveral degrees, but without effect. At laft, being urged ftill further, the glafs funk down completely between its fupports. The pyrometer being then withdrawn, denoted a temperature above 30.

It occurred to me, that, on this occafion, the glafs, by the firft application of heat, had foftened, and then had cryftallized, fo as to become hard again; that, in cryftallizing, it had acquired fuch infufibility as to yield to no heat under 30. I immediately confirmed this conjecture by the following experiment.

A piece of the fame glafs, placed in a cup of clay, was introduced into the muffle, heated to 21. In one minute it became quite foft, fo as to yield readily to the preffure of an iron rod. After a fecond minute had elapfed, the fragment, being touched by the rod, was

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* The meafurement of the temperatures may be relied upon as accurate; they were determined by two fets of pieces, one purchafed by me during the lifetime of the late Mr. Wedgwood, and the other likewife made by him, belonging to Dr. Kennedy. The two fets correfpond exactly; and Dr. Kennedy's had, at his requeft, been carefully examined by the prefent Mr. Wedgwood, who found them true by his father's original ftandard.

found to be quite hard, though the temperature had remained stationary. The substance, thus hardened, had undergone a change throughout; it had lost the vitreous character; when broken, it exhibited a fracture like that of porcelain, with little lustre; and its colour was changed from black to dark brown. Being exposed to heat, it was found ro be fusible only at 31; that is, it was less fusible than the glass by 13 or 14 degrees.

Numerous and varied experiments have since proved, in the clearest manner, that, in any temperature, from 21 to 28 inclusive, the glass of this whin passes from a soft, or liquid state, to a solid one, in consequence of crystallization; which is differently performed at different points of this range. In the lower points, as at 23, it is rapid and imperfect; in higher points, flower and more complete, every intermediate temperature affording an intermediate result. I likewise found, that crystallization takes place, not only when the heat is stationary, but likewise when rising or sinking, provided its progress through the range just mentioned is not too rapid. Thus, if the heat of the substance, after fusion, exceeds one minute in passing from 21 to 23, or from 23 to 21, the mass will infallibly crystallize, and lose its vitreous character.

These facts enabled me to account for the production of the substance resembling the liver of an animal, which I obtained in my first attempts to crystallize the melted stone. Not being then aware of the temperature proper for complete crystallization, I had allowed it to be passed over rapidly by the descending heat, and I had begun the flow cooling in those lower points, at which the formation of this intermediate substance takes place.

By the same means I was enabled to explain the other unexpected result, which I obtained in endeavouring to convert the glass of this stone into crystallite. The fire applied to the crucible, containing fragments of the glass, had been raised very flowly, which I know to have been the case by some circumstances of the experiment. The glass had softened by the first application of heat, but had crystallized again as the heat gradually rose; so that the substance consolidated, while still so viscid as to retain the original shape of the fragments; at the same time it acquired such infusibility as to resist the application of higher degrees of heat during the rest of the process.

No. 2. *Whin of the Rock of Edinburgh Castle.*

This is a basalt of a blackish blue colour. Its grain is fine, and its fracture uneven, partaking of the splintery. It is in general homogeneous, although, in some pieces, a very few minute crystals of hornblend are perceptible. It has some lustre, from a number of small shining facettes; has an earthy smell when breathed on; and gives fire flightly with steel.

The pure glass which this whin yielded, by rapid cooling after a moderate heat, was crystallized in three experiments, and produced masses greatly resembling the original. In one of these, formed on a large scale in the glass-house, the resemblance is so strong, both as to colour and texture, that it would be difficult, or perhaps impossible, to distinguish them,

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but for a few minute air bubbles visible in the artificial cryftallite. The glafs is lefs fufible than that of No. 1. and feems not to poffefs the property of producing the liver cryftallite.

No. 3. *Whin of the Bafaltic Columns on Arthur's Seat, near Edinburgh.*

Its bafis is a bafalt of a dark grey colour, and uneven fracture. It contains numerous laminar cryftals of felfpar, which feem to be almoft colourlefs, and have confiderable luftre and tranfparency. It alfo contains fome black hornblend. It has an earthy fmell when breathed on, and gives fparks flightly with fteel.

In the temperature of 100, or upwards, the whole was changed to pure black glafs; but in a more moderate heat, (about 60), the felfpar remained unchanged, while the hornblend difappeared, and formed a glafs along with the bafis of the ftone. Both kinds of glafs yielded highly characterifed cryftallites; that laft mentioned, having its felfpars entire, produced a fubftance like porphyry, in which the white felfpars were embedded in a black cryftalline bafis. The cryftals formed in this bafis are fo complete in one example, that they are feen projecting into the cavities, and ftanding erect on the external furface, fo as to make it fparkle all over. Thefe black cryftals feem to be hornblend of new formation. We have found, by fome late experiments, that they are confiderably more refractory than the cryftallite in which they lie, and are equally infufible with fome fpecies of natural hornblend.

No. 4. *Whin from the Neighbourhood of Duddingftone Loch.*

It has for its bafis a black bafalt of an uneven fracture. In it are embedded augit in numerous cryftals, felfpar in a fmaller proportion, and difperfed grains of olivin. The felfpar feems to be greenifh-white, with confiderable luftre and tranfparency. The ftone gives fire with fteel, and has a flight earthy fmell when breathed on. Its glafs yields a fine grained cryftallite, like that of No. 1.

No. 5. *Whin of Salifbury Craig near Edinburgh.*

This fpecies is an aggregate of black hornblend, and of a greenifh-white matter, both in minute grains. The greenifh-white matter refembles felfpar, but is much more fufible. The general characters are nearly the fame with thofe of the fpecimen already defcribed, No. 1. It has confiderable luftre, chiefly from the hornblend; an earthy fmell when breathed on; and gives fome fparks with fteel. Its glafs yielded a highly facetted cryftallite, approaching to the ftructure of the original whinftone, No. 4.

No. 6. *Whin from the Water of Leith.*

It is found in great blocks in the bed of the river, and has been brought there no doubt from a mafs of the fame kind in the mountains above. In confifts of black hornblend, and of a whitifh matter refembling felfpar, as in No. 1. and No. 5. Thefe two fubftances are nearly in equal proportion, and are confufedly and imperfectly cryftallized in minute maffes. If the whitifh fubftance were felfpar, this ftone, as well as that laft mentioned, would

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be the grünftein of Werner; but this white fubftance is far more fufible than felfpar, and melts at a lower heat than the hornblend, with which it is mixed. It has an earthy fmell when breathed on, and may be fcratched with difficulty by a knife.

In fufion and cryftallization it refembled the other whins. A fragment fimilar to this in all refpects, which I found in the neighbourhood of Edinburgh, manifefted fo ftrong a difpofition to cryftallize, that, though cooled in the open air after fufion, it was found ftony in the heart, with a vitreous outfide. When cryftallized, however, with every precaution, it yielded no remarkable refult.

No. 7. *Whin of the Balfaltic Columns of Staffa.*

I received this fpecimen from a gentleman who broke it from the original rock. It is bafalt of a bluifh-black colour. It is fine grained and homogeneous; and its fracture is uneven. It has a fmall degree of luftre, from a number of minute fhining points perceptible in a ftrong light. It gives an earthy fmell when breathed on, and may be fcratched with difficulty by a knife. It yielded a perfect and very hard glafs, which, in a regulated heat, produced a uniform ftony cryftallite, greatly refembling the original.

It has thus been fhown, that all the whins employed affume, after fufion, a ftony character, in confequence of flow cooling; and the fuccefs of thefe experiments, with fo many varieties, entitles us to afcribe the fame property to the whole clafs. The arguments, therefore, againft the fubterraneous fufion of whinftone, derived from its ftony character, feem now to be fully refuted.

Experiments on Lava.

In the inveftigation of Dr. Hutton's fyftem, great advantage may be expected from an examination of lavas. They have undoubtedly flowed on the furface by means of heat; and whinftone, according to his hypothefis, having flowed in the bowels of the earth by the influence of the fame agent, the two claffes ought to poffefs many properties in common, by which the hiftory of both may be illuftrated.

I have been enabled to inftitute a comparifon between them, by means of a cabinet of volcanic productions which I collected in 1785, in company with Dr. J. Home of this Society, on Vefuvious, Ætna, and the Lipari Ifles. On this occafion we were greatly affifted by the celebrated M. Dolomieu*, who accompanied us in part of our expedition. This author complains, in his writings, that travellers, in collecting volcanic productions, have brought away only the fuperficial fcoria of lavas, which nearly refemble each other in all

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* Though I differ widely from this gentleman in many of his theoretical opinions, I cannot too ftrongly exprefs my admiration of his merit as a natural hiftorian. His defcriptions of countries, as well as of minerals, prefent the moft lively reprefentations to the mind of the reader, which, in the numerous inftances I have witneffed, are perfectly correct.

                                                                                    cafes,

cafes, and convey no idea of the real character of the lava, which can only be feen in the interior parts of the currents. In forming our collection we fcrupuloufly avoided this error, and chofe fuch fpecimens only as were the moft compact and free from the fcorified appearance of the furface.

When thefe folid lavas are compared with our whinftones, the refemblance between the two claffes is not only ftriking at firft fight, but bears the clofeft examination. They both confift of a ftony bafis, which frequently contains detached cryftals of various fubftances, fuch as white felfpar and black hornblend. The analogy between the two claffes feems to hold through all their varieties; and I am confident that there is not a lava of Mount Ætna to which a counterpart may not be produced rom the whinftones of Scotland.

This refemblance in external character is accompanied with an agreement no lefs complete in chemical properties. But before I mention the experiments which tend to prove this agreement, it will be neceffary firft to examine the opinion of two very celebrated authors concerning lavas. M. Dolomieu and Mr. Kirwan, though they differ widely in many refpects, agree in believing, that lavas have never been acted upon by heat of fufficient intenfity to produce complete fufion; and endeavour, each by an hypothefis peculiar to himfelf, to account for their fluidity. The opinion of thefe gentlemen is of fuch importance in the prefent queftion, and the arguments they have ufed are fo extraordinary, that I muft beg leave to quote their words at full length.

M. Dolomieu ftates his opinion in the following paffage, *(Ifles Ponces,* p. 7.): " Il eft " effentiel de conftater, par beaucoup d'exemples et d'obfervations, quelques vérités que " j'ai annoncées il y a plufieurs années, favoir, que le feu des volcans ne dénature pas ordi- " nairement les pierres qu'il a mifes en état de fufion; qu'il ne les altere pas au point de " ne pouvoir les reconnoître, de ne pas diftinguer quelle a pu être la bafe des laves; que " ce feu agit différemment que le feu de nos fourneaux, tel que nous l'employons dans la " chimie et dans les arts; qu'il produit dans les laves une fluidité qui n'a aucun rapport " avec la fluidité vitreufe, que nous opérons, lorfque nous traitons à grand feu les mêmes " matières qui leur fervent de bafe, et lorfque nous voulons rendre aux laves elles mêmes " leur fluidité. Celui des volcans n'a point d'intenfité; il ne peut pas même vitrifier les " fubftances les plus fufibles, tels que les fchorls, qui fe trouvent comme parties confti- " tuantes dans l'intérieur des laves; il produit la fluidité par une efpece de diffolution par " une fimple dilation, qui permet aux parties de gliffer les unes fur les autres, et peutêtre " encore par le concours d'une autre matière qui fert de véhicule à la fluidité."

Mr. Kirwan cenfures this fuppofition as ftrange and inconceivable; but in my opinion, that which he has brought forward is not lefs fo. In the *Elements of Mineralogy, fecond edition,* vol. I. p. 396. he fays: "Now, there are but three forts of fufion with which we are ac- " quainted: that which produces porcelain; that which produces enamels and femi-vitri- " fications; and that which produces glafs. By infpecting lava we fhall find that very " little of it has been in any of thefe ftates; fince therefore it has flowed, it is plain it has " derived its liquifaction not from the fufion of its own materials, but from that of fome

" foreign fubftance mixed it. This fact is fo plain, that it has even ftruck M. Dolomieu,
" in the midft of his prepoffeffions in favour of fome ftrange inconceivable power, which
" he attributes to volcanic fire, of melting earthy fubftances, without effecting an alteration
" in their fenfible qualities. " I hope," fays he, " to prove, that lavas contain, in their
" interior, a combuftible matter, which burns and confumes in the fame manner as other
" inflammables," *Ifles Ponces*, 10. Yet he neglects telling us what this matter is; though
" it plainly appears to be no other than fulphur and bitumen, of which an immenfe quan-
" tity is found in all volcanos, which liquefies in a low degree of heat, and caufes all the
" ftony fubftances to flow that are immerfed in it."

(To be continued.)

IV.

On the Hydraulic Machine erected at the Mines of Schemnitz in Hungary.

To Mr. NICHOLSON.

SIR,

THE early infertion of the extract which I fent you from the Micrographia of Robert Hooke in your laft Number, encourages me to requeft you will give fome account, with your opinion, of a machine for raifing water at the mines in Hungary, upon the principle of the fountain of Hiero. It is flightly mentioned in many elementary books, but I cannot point out the work in which it was originally publifhed, nor fay thing more than that I believe it has not been accurately defcribed in any Englifh work.

I am, &c.

R. B.

London, Feb. 20, 1800.

The account was firft given to the Royal Academy at Paris by their correfpondent, M. Jars, and inferted in their Memoirs for the year 1768. The machine was executed in the year 1755, and ufed to raife the water of a fhaft named *Amalie*, in the mines at Schemnitz. Fig. 1, Plate 2, is copied from the engraving of Jars. It is placed at the depth of forty toifes beneath the furface of the ground above. The wooden trough A is a kind of refervoir at the end of a channel which conveys the water by which the machine is worked. The pipes are not drawn in the proportion of their lengths, but contracted to the fpace of the defign. B B conveys the water from it into the refervoir D, which is twenty-two toifes lower than A. This pipe defcends very nearly to the bottom of D, as is fhewn by the dotted line, with the intention that the included air fhall be forced to

afcend

afcend and pafs through the pipes H into another refervoir I ftill lower. The pipes N, through which the fubterraneous waters are raifed, are fixteen toifes in height. They begin very near the bottom of the veffel I, in order that the air from that refervoir may not efcape until all the water has been raifed.

The pipes B and N are four and a half (French) inches in diameter within; but thofe H for the air are only three inches in diameter at iffuing from the refervoir D, and two and a half when they enter the refervoir I, and this alfo is the diameter of the pipe P.

The wooden trough L is the termination of a trough or channel from another engine, which raifes the waters from a yet greater depth. From this proceeds a pipe with a cock K, in order that the water from L may either be admitted into I, or intercepted at pleafure.

The refervoir D, which, as well as I, is made of an alloy of copper and tin, is of double the capacity of I. It is eight feet and a half high, and five feet diameter infide meafure, and its thicknefs is two inches. The lower refervoir I is four feet in diameter, and fix feet and a half high infide meafure, and its thicknefs one inch and a half. Each of thefe veffels was caft in three pieces, as the drawing fhews, which are joined by flanches and fcrews, a ring of lead and another of leather being placed between, to fecure the joint, and prevent the tranfmiffion of any fluid. M. Jars obferves, that the pipes would have been better if connected in the fame manner; but the practice is to drive the ends of the pipes into hollow cylinders of very dry wood, iron bound with three hoops. Thefe anfwer very well, and are of confiderable durability.

The cocks C, E, K are fcrewed in their places by caps or covers faftened down with fcrews.

When the machine is to be put into action, all the cocks are clofed, and as the refervoir A is always full, the pipe B is alfo full, as far as the cock C. The refervoir L is alfo conftantly full of water from the mine which is to be raifed fixteen toifes to O. For this purpofe the cock K is opened, and the water flows into I, the air being fuffered to efcape from that veffel by turning the cock M. The veffel is known to be full by the emiffion of water through P; at which inftant both the cocks K and M are clofed. Immediately afterwards the cocks C and G are opened, and the water from A entering B, compreffes the included air, and forces it through the pipes H, where it preffes on the furface of the water contained in the inferior refervoir I, and forces that fluid to afcend through N to O, or the Adit through which it is difcharged. This water being raifed, and the refervoir I being empty, the cocks C and G are fhut, and E and F is then opened, the firft to fuffer the contents of D to flow off, and the fecond to accelerate the difcharge, by admitting the external air. Both thefe cocks are again clofed as foon as the evacuation is completed. During this laft operation, another man below opens the cocks K and M, by which the included air iffues with great force through P, and the water from L again fills the refervoir I as at firft. The operations of clofing K and M, and opening C and G, being therefore again repeated, the contents of I are again forced up to, and thus the engine may be kept continually at work.

Each

Each alternation employs about three minutes, and raifes between twenty-nine and thirty cubic feet of water, which, in the courfe of the natural day, amounts to twelve or thirteen thoufand cubic feet of water, raifed by the fall of about double the quantity through a fomewhat greater height. Two men * are required to attend it; but M. Jars feems to think, that on account of the fimplicity and cheapnefs of its conftruction, and the little wear and tear, together with the facility with which it may be made to work and ftop for very fhort periods of time, it would be of great value in fuch places as afford the requifite fall of fuperior water, and do not require a higher fingle lift than fifteen or twenty toifes (or fathoms.)

The fall of twenty-two toifes given to the fuperior water is not neceffary for raifing the column of fixteen, though it is profitable, by increafing the velocity of afcent. But it was at firft intended to force the lower water as high as twenty-one toifes, which could not be performed with the apparatus then fet up. For at the very firft introduction of water into the veffel D, it burft with an effect which fufficiently fhewed the power of fuch a column of water compreffing a mafs of air which, by its elafticity, was difpofed to give a a vibratory or jerking motion to the agent.

When the machine is near the end of its action, that is to fay, when nearly the whole of the water has been raifed out of the refervoir I, if the cock M be opened to give vent to the compreffed air, and a hat, or miner's bonnet, be prefented to the aperture P, the aqueous vapours difperfed through the compreffed air, and perhaps alfo, fays M. J. part of thofe of the external air, are condenfed on the bonnet, in the form of very white and compact ice, very much refembling hail, and not eafily feparated from the bonnet: it foon melts, which is not to be wondered at, as the temperature of the place itfelf is not cold. Meffrs. du Hamel and Jars remained in Hungary from January to July, 1758, and obferved the fame phenomenon at all feafons; but as they had not a thermometer, they could not make a number of experiments, which might have been of value in the inveftigation of the fubject.

It is obfervable, that the air iffues out with fuch impetuofity, that the workman could not hold the bonnet at the diftance of a few inches from the aperture, as he does in this experiment, if he were not fupported behind. The ice is much more compact if the cock be only in part opened.

On the above, which is tranflated from the Memoir of M. Jars, with very little abridgement, I have fcarcely any additional remark to make. The utility of this engine, under the circumftances here ftated, is fufficiently clear, and its applications might become more extenfive, if the improvements fuggefted in the preceding note were added. In particular, where a ftream flows with the poffibility of carrying off part of the water by a deep

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* It feems very eafy to connect the levers of the cocks above and below, fo as to require only one man to work the whole fet: and, indeed, there would be little difficulty in making the machine work itfelf fafely without any attendant except to fet it off at firft, or ftop it when requifite.---N.

fhaft

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shaft or drain, it would be practicable by the fall of part to raise the remainder to the top of an house or building for domestic or other purposes. In this case it would be requisite to place the first vessel D at the bottom of the shaft, and the second I at the level of the stream, and the operation of this inverted engine would force water nearly as high above that level as the depth of the shaft might be below it.

If the ascending and descending columns were of equal length, they would be in equilibrio, and no more water would rise than just to fill the lower tube. The velocity of ascent, supposing sufficient water way, and other circumstances, such as the inflexions of the pipes, to be properly disposed, will depend on the excess of length in the descending column, and this excess is capable of a maximum relative to the quantity raised to a given height, or as engineers call it, the effect. The longer the lift, the less the quantity raised, supposing the upper reservoir to be of a given magnitude, the densities of air being inversely as the pressures, or length of the pressing column, and the quantity raised being equal to that of the air in its compressed state. Whence it follows, that the whole quantity raised will be less the fewer the lifts.

I do not understand the hypothesis upon which this machine was constructed. The reservoirs have their capacity as 2 to 1. But the air subjected to the re-action of a column of 16 toises, besides the common pressure, would sustain the pressure of four atmospheres, and therefore be condensed into one-fourth the space. If there be no fallacy in this plain remark, it must follow, that the lower vessel I was only half emptied when the stream ceased to be afforded at O, and, consequently, the effect was only half what is here stated.

That air condensed to one-fourth should take up and dissolve more water than in its ordinary rare state, and afterwards deposit it when it recovered its original dimensions, is consentaneous with other well known facts: and the modern theorists will easily apply the doctrine of latent heat, or the increased capacity of expanded air, to account for the phenomenon of its robbing the water not only of the heat which maintained its state of elastic solution, but even that which would have been requisite to keep up the state of common dense fluidity. W. N.

V.

An Account of the Pearl Fishery in the Gulph of Manar, in March and April 1797. By HENRY J. LE BECK, Esq.

THE diving stone is a piece of coarse granite, a foot long, six inches thick, and of a pyramidical shape, rounded at the top and bottom. A large hair rope is put through a hole in the top. Some of the divers use another kind of stone shaped like a half moon, to bind.

bind round their belly, so that their feet may be free. At present these are articles of trade at *Condatchey*. The most common, or pyramidical stone, generally weighs about thirty pounds. If a boat has more than five of them, the crew are either corporally punished or fined.

The diving, both at *Ceylon* and at *Tutucorin*, is not attended with so many difficulties as authors imagine. The divers, consisting of different casts and religions, (though chiefly of *Parrawer* * and *Musselmans*,) neither make their bodies smooth with oil, nor do they stop their ears, mouths, or noses with any thing, to prevent the entrance of salt water. They are ignorant of the utility of diving bells, bladders, and double flexible pipes. According to the injunctions of the shark conjurer they use no food while at work, nor till they return on shore, and have bathed themselves in fresh water. These *Indians*, accustomed to dive from their earliest infancy, fearlessly descend to the bottom in a depth of, from five to ten fathoms in search of treasures. By two cords a diving stone and a net are connected with the boat. The diver putting the toes of his right foot on the hair rope of the diving stone, and those of his left on the net, seizes the two cords with one hand, and shutting his nostrils with the other, plunges into the water. On reaching the bottom, he hangs the net round his neck, and collects into it the pearl shells as fast as possible, during the time he finds himself able to remain under water, which usually is about two minutes. He then resumes his former posture, and making a signal, by pulling the cords, he is immediately lifted into the boat. On emerging from the sea, he discharges a quantity of water from his mouth and nose, and those who have not been long enured to diving frequently discharge some blood; but this does not prevent them from diving again in their turn. When the first five divers come up and are respiring, the other five are going down with the same stones. Each brings up about one hundred oysters in his net, and if not interrupted by any accident, may make fifty trips in a forenoon. They and the boat's crew get generally from the owner, instead of money, a fourth of the quantity which they bring on shore; but some are paid in cash, according to agreement.

The most skilful divers come from *Collish*, on the coast of *Malabar*; some of them are so much exercised in the art, as to be able to perform it without the assistance of the usual weight; and for a handsome reward will remain under water for the space of seven minutes; this I saw performed by a *Caffry* boy, belonging to a citizen at *Karical*, who had often frequented the fisheries of these banks. Though Dr. Halley deems this impossible, daily experience convinces us, that by long practice any man may bring himself to remain under water above a couple of minutes. How much the inhabitants of the South Sea islands distinguish themselves in diving we learn from several accounts; and who will not be surprised at the wonderful *Sicilian* diver Nicholas, furnamed the Fish † ?

* Fishermen of the Catholic religion.
† According to Kircher, he fell a victim amongst the *Polypes* in the gulph of *Charybdis*, on his plunging, for the second time, in its dangerous whirlpool, both to satisfy the curiosity of his king, Frederic, and his inclination for wealth. I will not pretend to determine, how far this account has been exaggerated.

- Every

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Every one of the divers, and even the moſt expert, entertain a great dread of the ſharks, and will not, on any account, deſcend until the conjurer has performed his ceremonies. This prejudice is ſo deeply rooted in their minds, that the government was obliged to keep two ſuch conjurers always in their pay, to remove the fears of their divers. Thirteen of theſe men were now at the fiſhery from *Ceylon* and the coaſt, to profit by the ſuperſtitious folly of theſe deluded people. They are called in *Tamul*, *Pillăl Kadtăr*, which ſignifies one who binds the ſharks and prevents them from doing miſchief.

The manner of enchanting conſiſts in a number of prayers learned by heart, that nobody, probably not even the conjurer himſelf, underſtands, which he, ſtanding on the ſhore, continues muttering and grumbling from ſun riſe until the boats return; during this period, they are obliged to abſtain from food and ſleep, otherwiſe their prayers would have no avail; they are, however, allowed to drink, which privilege they indulge in a high degree, and are frequently ſo giddy, as to be rendered very unfit for devotion. Some of the conjurers accompany the divers in their boats, which pleaſes them very much, as they have their protectors near at hand. Nevertheleſs, I was told, that in one of the preceding fiſheries, a diver loſt his leg by a ſhark, and when the head conjurer was called to an account for the accident, he replied that an old witch had juſt come from the coaſt, who, from envy and malice, had cauſed this diſaſter, by a counter-conjuration, which made fruitleſs his ſkill, and of which he was informed too late; but he afterwards ſhewed his ſuperiority by enchanting the poor ſharks ſo effectually, that though they appeared in the midſt of the divers, they were unable to open their mouths. During my ſtay at *Condatchey*, no accident of this kind happened. If a ſhark is ſeen, the divers immediately make a ſignal, which, on perceiving, all the boats return inſtantly. A diver who trod upon a hammer oyſter, and was ſomewhat wounded, thought he was bit by a ſhark, conſequently made the uſual ſignal, which cauſed many boats to return; for which miſtake he was afterwards puniſhed.

The owners of the boats * ſometimes ſell their oyſters, and at other times open them on their own account. In the latter caſe ſome put them on mats in a ſquare, furrounded with a fence; others dig holes of almoſt a foot deep, and throw them in till the animal dies; after which they open the ſhells and take out the pearls with more eaſe. Even theſe ſquares and holes are ſold by auction after the fiſhery is finiſhed, as pearls often remain there, mixed with the ſand.

In ſpite of every care, tricks in picking out the pearls from the oyſters can hardly be prevented. In this the natives are extremely dexterous. The following is one mode they put in practice to effect their purpoſe: when a boat owner employs a number of hired people to collect pearls, he places over them an inſpector of his own, in whom he can confide; theſe hirelings previouſly agree that one of them ſhall play the part of a thief, and bear the puniſhment, to give his comrades an opportunity of pilfering. If one of the gang happens

* Theſe are the individuals which farm one or more boats from the renter; and though they are in poſſeſſion of them only during the fiſhery, they are commonly called the owners of the boats.

to meet with a large pearl, he makes a fign to his accomplice, who inftantly conveys away one of fmall value, purpofely, in fuch a manner as to attract notice. On this the infpector and the reft of the men take the pearl from him : he is then punifhed and turned out of their company. In the mean time, while he is making a dreadful uproar, the real thief fecures the valuable pearl, and afterwards the booty is fhared with him who fuffered for them all. Befides tricks like thefe the boat owners and purchafers often lofe many of the beft pearls, while the dony is returning from the bank ; for, as long as the animal is alive and untouched, the fhells are frequently open near an inch; and if any of them contain a large pearl, it is eafily difcovered and taken out by means of a fmall piece of ftiff grafs or bit of ftick, without hurting the pearl fifh. In this practice they are extremely expert. Some of them were difcovered whilft I was there, and received their due punifhment.

Gmelin afks if the animal of the *mytilus margaritiferus* is an *afcidia?* See Linn. Syft. Nat. tom. I. p. vi. 3350. This induces me to believe that it has never yet been accurately defcribed; it does not refemble the *afcidia* of Linnæus, and may, perhaps, form a new genus. It is faftened to the upper and lower fhells by two white flat pieces of mufcular fubftance, which are called by *Houttuin* * ears, and extend about two inches from the thick part of the body, growing gradually thinner. The extremity of each *ear* lies loofe, and is furrounded by a double brown fringed line. Thefe lie almoft the third of an inch from the outer part of the fhell, and are continually moved by the animal. Next to thefe, above and below, are fituated two other double fringed moveable fubftances, like the branchiæ of a fifh. Thefe *ears* and *fringes* are joined to a cylindrical piece of flefh; of the fize of a man's thumb, which is harder and of a more mufcular nature than the reft of the body. It lies about the centre of the fhells, and is firmly attached to the middle of each. This, in fact, is that part of the pearl fifh which ferves to open and fhut the fhells. Where this column is faftened, we find on the flefh deep impreffions, and on the fhell various nodes of round or oblong forms, like imperfect pearls. Between this part, and the hinge *(cardo)*, lies the principal body of the animal, feparated from the reft, and fhaped like a bag. The mouth is near the hinge of the fhell, enveloped in a veil, and has a double flap or lip on each fide ; from thence we obferve the throat, (œfophagus) defcending like a thread to the ftomach. Clofe to the mouth there is a carved brownifh tongue, half an inch in length, with an obtufe point; on the concave fide of this defcends a furrow, which the animal opens and fhuts, and probably ufes to convey food to its mouth †. Near its middle are

* Vide Houtt. Nat. Hift. Vol. I. p. xv. p. 381, feq.

† The depth at which the pearl fifh generally is to be found, hindered me from paying any attention to the locomotive power, which I have not the leaft doubt it poffeffes, ufing for this purpofe its tongue. This conjecture is ftrengthened by the accurate obfervations made on *mufcles* by the celebrated Reaumur, in which he found that this body ferves them as a leg or arm, to move from one place to another. Though the divers are very ignorant with regard to the œconomy of the pearl fifh, this changing of habitation has been long fince obferved by them. They alledge, that it alters its abode when difturbed by an enemy or in fearch of food. In the former cafe they fay it commonly defcends from the fummit of the bank to its declivity.

two bluish spots, which seem to be the eyes. In a pretty deep hole, near the base of the tongue, lies the beard *(byssus)*, fastened by two fleshy roots, and consisting of almost one hundred fibres, each an inch long, of a dark green colour, with a metallic lustre; they are undivided, parallel, and flattened. In general the *byssus* is more than three quarters of an inch, without the cleft *(rima)*; but if the animal is disturbed, it contracts it considerably. The top of each of these threads terminates in a circular gland or head, like the *stygma* of many plants. With this *byssus* they fasten themselves to rocks, corals, and other solid bodies; by it the young pearl fish cling to the old ones, and with it the animal procures its food, by extending and contracting it at pleasure. Small shell fish, on which they partly live, are often found clinging to the former. The stomach lies close to the root of the beard, and has, on its lower side, a protracted obtuse point. Above the stomach are two small red bodies, like lungs; and from the stomach goes a long channel or gut, which takes a circuit round the muscular column above-mentioned, and ends in the anus, which lies opposite to the mouth, and is covered with a small thin leaf, like a flap. Though the natives pretend to distinguish the sexes, by the appearance of the shell, I could not find any genitalia. The large flat ones they call males, and those that are thick, concave, and vaulted, they call females, or *pedoo-chippy;* but, on a close inspection, I could not observe any visible sexual difference.

It is remarkable that some of these animals are as red as blood, and that the inside of the shell has the same colour, with the usual pearly lustre, though my servants found a reddish pearl in an oyster of this colour; yet such an event is very rare. The divers attribute this redness to the sickness of the pearl fish; though it is most probable that they had it from their first existence. In the shade they will live twenty-four hours after being taken out of the water. This animal is eaten by the lower class of *Indians,* either fresh in their curries, or cured by drying; in which state they are exported to the coast; though I do not think them by any means palatable.

Within a mother of pearl shell I found thirteen *murices nudati* (vide Chemnitz's New System, Cabt. vol. XI. tab. 192, f. 1851 and 1852), the largest of which was three quarters of an inch long; but as many of them were putrid, and the pearl fish itself dead, I could not ascertain whether they had crept in as enemies, or were drawn in by the animal itself. At any rate turtles and crabs are inimical to the animals, and a small living crab was found in one of them.

The pearls are only in the softer part of the animal, and never in that firm muscular column above-mentioned. We find them in general near the earth, and on both sides of the mouth. The natives entertain the same foolish opinion concerning the formation of the pearl which the ancients did. They suppose them formed from dew-drops in connection with sun-beams. A *Bráhmen* informed me that it was recorded in one of his *Sanscrit* books, that the pearls are formed in the month of *May* at the appearance of the *Sooatee* star (one of their twenty-seven constellations) when the oysters come up to the surface of the

water, to catch the drops of rain. One of the moſt celebrated conchologiſts *, ſuppoſes that the pearl is formed by the oyſter in order to defend itſelf from the attacks of the *pho-lades* and *boreworms*. But we may be aſſured that in this ſuppoſition he is miſtaken, for although theſe animals often penetrate the outer layers of the pearl ſhell, and there occaſion hollow nodes, yet, on examination, it will be found, that they are never able to pierce the firm layer, with which the inſide of the ſhell is lined. How can the pearls be formed as a defence againſt exterior worms, when, even on ſhells that contain them, no worm-holes are to be ſeen ? It is, therefore, more probable theſe worms take up their habitations in the nodes, in order to protect themſelves from the attacks of an enemy, than that they are capable of preying on an animal, ſo well defended as the pearl-fiſh is. It is unneceſſary to repeat the various opinions and hypotheſes of other modern authors ; it is much eaſier to criticiſe them, than to ſubſtitute in their place a more rational theory. That of Reaumur, mentioned in the memoirs of the *French* Academy for 1712, is the moſt probable, viz. that the pearls are formed like bezoars and other ſtones in different animals, and are apparently the effects of a diſeaſe. In ſhort it is very evident, that the pearl is formed by an extravaſation of a glutinous juice either within the body, or on the ſurface of the animal : the former caſe is the moſt common. Between one and two hundred pearls have been found within one oyſter. Such extravaſations may be cauſed by heterogeneous bodies, ſuch as ſand, coming in with the food, which the animal, to prevent diſagreeable friction, covers with its glutinous matter, and which, as it is ſucceſſively ſecreted, forms many regular lamellæ, in the manner of the coats of an onion, or like different ſtrata of bezoars, only much thinner ; this is probable, for if we cut through the centre of a pearl, we often find a foreign particle, which ought to be conſidered as the nucleus, or primary cauſe of its formation. The looſe pearls, may originally have been produced within the body, and on their encreaſe may have ſeparated and fallen into the cavity of the ſhell. Thoſe compact ones, fixed to the ſhells, ſeem to be produced by ſimilar extravaſation, occaſioned by the friction of ſome roughneſs on the inſide of the ſhell. Theſe and the pearl-like nodes have a different aſpect from the pearls, and are of a darker and bluer colour. In one of the former I found a pretty large, true oval pearl, of a very clear water; while the node itſelf was of a dark blueiſh colour. The yellow or gold coloured pearl, is the moſt eſteemed by the natives ; ſome have a bright, red, luſtre ; others are grey or blackiſh, without any ſhining appearance, and of no value. Sometimes when the grey lamella of a pearl is taken off, under it is found a beautiful genuine one, but it oftener happens that after having ſeparated the firſt coat you find a worthleſs impure pearl. I tried ſeveral of them, taking one lamella off after another, and found clear and impure by turns, and in an impure pearl I met with one of a clear water, though in the centre of all I found a foreign particle. The largeſt and moſt perfect pearl which I ſaw during my ſtay at *Condatchey*, was about the ſize of a ſmall piſtol bullet, though I have been told ſince my departure, many others of the

* The Rev. Mr. Chemnitz at *Copenhagen.*

fame fize have been found. The fpotted and irregular ones are fold cheap, and are chiefly ufed by the native phyficians as an ingredient in their medicines.

We may judge with greater or lefler probability by the appearance of the pearl-fhell, whether they contain pearls or not. Thofe that have a thick calcareous cruft upon them, to which *ferpulæ* (fea tubes) *Tubuli marini irregulariter intorti, Crifta-gali Chamar lazuras, Lepas tintinabulum, Madreporee, Millipore, Cellipore, Gorgontæ, Spongiæ,* and other zoo-phytes are faftened, have arrived at their full growth, and commonly contain the beft pearls; but thofe that appear fmooth, contain either none, or fmall ones only.

Were a naturalift to make an excurfion for a few months to *Manâr,* the fmall ifland near *Jafna* and the adjacent coaft, he would difcover many natural curiofities, ftill buried in obfcurity, or that have never been accurately defcribed.

Indeed no place in the *Eaft Indies* abounds more with rare fhells, than thefe: for there they remain undifturbed, by being fheltered from turbulent feas, and the fury of the furf. I will juft name a few of them; viz. *Tellina foliaca* Lynn *, *Tell, Spenglerii, Arca culcu-lata* †, *Arca Noæ, folen anatinus* Linn. *Oftrea Ifognomum, Terebullum, albidum, ftriatum, Turbo fcalaris* ‡ *Bula volva* Linn §, *Vexillum ingritarum, &c.* Amongft the beautiful conc fhells: *conus thalaffiarchus Anglicanus cullatus* ‖, *amadis thaffiarchus, con. generalcis* Linn. *c. capitaneus* **, *c. miles* ††, *c. ftercus mufcarum* ‡‡. *c. reteaureum, c. glaucus* §§, *c. cereola, regia corona murus lapedius, canda erminea focietas cordium.* There are many other befides thofe already mentioned, equally valuable and curious.

The great fuccefs of the Rev. Doctor John in conchology when at *Tutucorin,* and affifted by G. Angelbeck, with a boat and divers: and the capital collections made by his agents, whom he afterwards fent there with the neceffary inftructions and apparatus, may be feen in Chemnitz's elegant cabinet of fhells in 4to. (with illuminated plates), and how many new fpecies of Zoophytes he difcovered, we learn from another German work by Efper at *Er-langen,* the third volume of which is nearly finifhed.

* The golden tong. † Mounkfcape. ‡ Royal ftaircafe.
§ Weaver's fhuttle. ‖ Red *Englifh* admiral. ** Green ftamper.
†† Garter ftamper. ‡‡ Great fand ftamper. §§ Capf. Gottw.

VI.

Proceſs for extracting Sugar from the Beet, communicated in a Letter from Mr. ACHARD to Citizen VAN MONS*.

IT is the beta vulgaris L. which is proper for making of ſugar, though all the varieties are not equally ſo. That of which the internal part is white: the ſkin of a pale red, and the root of a long ſpindle ſhape, is the beſt. All the varieties of the beet yield ſugar, but this muſt be choſen when it is to be manufactured to advantage ; beſides which, it is on the culture principally the quantity of ſugar produced by the root depends.

It is aſcertained from the operations of extracting ſugar from the beet root, which I have performed under the inſpection of a committee, nominated by the King of Pruſſia, that the beſt method is as follows. The root not peeled, but in the ſtate in which it is dug out of the ground, with no other preparation than that of carefully ſeparating the leaves and the heart, is to be boiled in water till it is ſoft enough to be pierced with a ſtraw. A ſhort time of ebullition is ſufficient to give it this degree of ſoftneſs, which is well known to the confectioners, who give it to ſome fruits before they preſerve them. The beet when cold is divided and reduced into ſlices by means of the machine uſed by farmers to ſlice potatoes for cattle. A deſcription of this machine is to be found in a publication of Buſch, entitled : *Ueberſicht der fortſchritte in wiſſenſchaften, kunſten, manufacturen und handwerken von Oſtern 1796, bis Oſtern 1797. Erfurt* 1798 : and the engraving which renders the ſubject clearer is copied in the firſt plate of my work. This method of dividing the root is the beſt I have yet found. Two men with this machine can cut nearly 100 pounds into very fine ſlices in three minutes. To extract the juice from the roots when ſliced, they are ſubjected to the action of a preſs which ſhould act with force ſufficient to extract at firſt as much juice as poſſible. The pulp which remains in the preſs ſtill contains a conſiderable portion of ſugar, which is worth extracting. To effect this it is diluted in a ſufficient quantity of water for twelve hours, after which the fluid part is drawn out by preſſure. The ſaccharine matter, after this ſecond extraction, is ſtill ſufficiently abundant to afford by fermentation, brandy or vinegar, with profit.

The liquids thus obtained are afterwards mixed and ſtrained through a flannel, and reduced by conſtant boiling to about two thirds. It is then paſſed a ſecond time through a woollen cloth, or a piece of cloth, ſuch as is uſed in ſugar refineries; after which the liquor is boiled in a ſmaller veſſel than the firſt till it is reduced to one half. The liquor is again boiled in a ſtill ſmaller veſſel which gives it the conſiſtency of a liquid ſyrup. It is neceſſary to remark, that by endeavouring to give to the ſyrup a too ſtrong conſiſtency there will be danger of ſpoiling the whole.

* Decade. An. VIII. p. 15. No. 15. p. 324.

This

This fyrup poured into fhallow earthen veffels, which prefent a large furface to the air, is to be placed in a ftove at the heat of 20 or 30 degrees, or if agreeable at 30 or 40 of Reaumur, in order to cryftalize it. During this infenfible condenfation of the fyrup the cryftalline incruftation, which is formed at the furface, fhould be occafionally broken, in order that by favouring the evaporation the product of cryftals may be haftened. As foon as it is obferved that, inftead of the cryftalline incruftation, a thick gummy pellicle, not granulated, is formed on the furface of the fyrup, it is a fign that the matter does no longer cryftalize, but begins to dry, and the evaporation fhould then be ftopped. This refidue forms a mixture more or lefs thick, of a cryftalline fubftance and a fluid vifcid matter. In order to feparate the cryftalized fugar from the gummy extract, they are put together into a fack of wet cloth tied tight, and then by means of a prefs, gradually applied, the fluid part is to be paffed through the cloth, and the fugar remains in the bag. This fugar after drying is a yellow mufcovado, compofed of regular cryftals, which when pulverized form a white powder of which the tafte is very good, being fweet and clear, and may be applied to a number of ufes for which refined fugar is employed. By the operation of refining, fugar may be made from this mufcovado of whatever quality is agreeable, and by repeating it the fineft fugar may be had. The wafte in this manufacture, that is to fay the refidual pulp, the fyrup or mucilage which paffes through the cloth when fubjected to the prefs, the fyrup in which the fugar has cryftalized, the wafhings, &c. &c. all thefe are ftill very ufeful, and a confiderable quantity of rum or brandy may be obtained from them, which may be ufed in making up the fineft compounds. The mufcovado, fuch as is obtained by the firft opperation, cofts about a grofs and a half of Pruffia, without reckoning the watter which may be had by turning the refidues to ufe. When we add this product, and when the manipulations fhall be more perfect, to effect which I fhall employ myfelf this winter, I am perfuaded that our European mufcovado will only coft half the price, or 9 fennins ; and in the countries, where fuel is dearer, at 1 grofs *. The manufacture of fpirits from the wafte of the fugar is of great importance, as by this means a great faving of corn will be made, and the manufacturing of beet fugar, which delivers Europe from a deftructive monopoly becomes ftill more interefting. I am at prefent employed in the attempt to difcover a method of pouring the juice of the roots when fufficiently condenfed into moulds or forms, in order that it may acquire the figure of a fugar loaf, and afterwards by claying become very white at a fingle operation. I have already found feveral methods of obtaining this object very fpeedily. This new manipulation will facilitate the art of fugar-making, and diminifh the price ftill further.

* The pound in ufe at Berlin, being 476, 28 grammes, and the value of the grofs, or of 12 fennins, being 17 centimes, French money, it follows that the hectogramme does not amount to more than about 5 or 6 centimes, (or the pound, poids de marc, from 28 to 50 centimes). Note of the French editor.- -(about 2½ d. per lb. avoirdup.)

VII.

VII.

Letter to the EDITOR *from Mr.* WILLIAM HENRY, *on his Difcovery of a new and improved Method of preparing the Pruffiate of Pot-afh; and on the difputed Queftion, Whether Pruffiate of Pot-afh be decompofed by Muriate of Barytes?*

Manchefter, March 23, 1800.

SIR,

IN confequence of a paper, inferted in your Philofophical Journal, I have been favoured with the following letter from Mr. Kirwan. As the references, which it contains, to authorities on a difputed point, may be interefting to others as well as to myfelf, I take the liberty of fending you a copy of it for publication; premifing, that I have ftated to Mr. K. my intention of making you this communication, and that he has not exprefled any objection.

" *To Mr.* WILLIAM HENRY.

" DEAR SIR,

" On looking over Mr. Nicholfon's valuable Philofophical Journal, which, though of
" ancient date, I had not until lately leifure to infpect, I find, in the 29th No. Vol. ii.
" p. 170, that you feem to regret I had not mentioned on what grounds I denied the pre-
" cipitation of barytic earth from muriatic acid by the Pruffian alcali, and then fuggeft that the
" relation of the Pruffian alcali to this earth had not been an object of attention to Klaproth
" or Pelletier, &c. I hope, therefore, it will be agreeable to you that I fhould ftate thofe
" grounds. That Pruffian alcali, properly prepared, does not precipitate barytic folutions,
" was firft proved by that accurate chemift, Mr. Meyer, of Stetin, as you may fee in 2
" Crell's Chemical Annals for 1786, p. 142, 143. As I fuppofe you have that work, I
" fhall not trouble you with the detail of his experiments. You may, alfo, fee that both
" Pelletier and Klaproth did treat of this fact, Pelletier affirming the precipitation of this
" earth by the Pruffian alcali (21 Journal des Mines, p. 45); and Klaproth exprefsly deny-
" ing it, and afferting that Pelletier was miftaken. (See 2 Klaproth, p. 91.)

" I am, dear Sir,

" With high regard and refpect, &c.

" R. KIRWAN."

" *Dublin, Jan.* 20.

Unfortunately it is not in my power to confult any one of the works, to which Mr. K. has kindly referred me; and highly as I refpect the authorities, to which he has appealed, yet I did not feel difpofed to acknowledge myfelf in an error, till convinced by a circum-

ftantial

ftantial detail of the experiment of Meffrs. Meyer and Klaproth. I refolved, therefore, to appeal to the decifion of experiments conducted with all poffible accuracy and attention.

The queftion in difpute is, whether or not a double elective affinity be exerted between pruffiate of pot-afh and muriate of barytes ? If fuch an affinity really fubfifts, and produces, on the admixture of the two folutions, two new compounds, one of thefe (the pruffiate of barytes) muft neceffarily, from its known infolubility, appear under the form of a pre-cipitate. Now, on mixing the two folutions, a precipitation generally takes place ; but this, it is affirmed by certain chemifts, is owing to the impurity of the pruffiate of pot-afh, and is occafioned by the decompofition of fulphate of pot-afh by muriate of barytes. The truth of this explanation may be afcertained, either by an attentive examination of the pre-cipitate, or by employing a pruffiate of pot-afh perfectly free from fuch impurity.

The former of thefe tefts, as I have ftated in your Journal, II. 171, I employed feveral months ago. Having found that pruffiated barytes is foluble in diluted muriatic acid, which exerts no action on the fulphate of that earth, I digefted a precipitate, thrown down from muriated barytes by pruffiated pot-afh, with diluted muriatic acid. Part of the pre-cipitate was taken up ; and the folution betrayed, to the proper tefts, decifive characters of its containing pruffiated barytes. Since the receipt of Mr. Kirwan's letter, I have repeated this experiment feveral times ; occafionally with the refult, which has juft now been ftated ; while, at other times, none of the precipitate was diffolved by the acid. Thefe con-tradictory refults will be explained by what follows ; but not being able to devife the caufe of them, at the time they occurred, I had recourfe to the remaining expedient, viz. the employment of a pruffiate of pot-afh, perfectly free from fulphuric falts.

The difficulty of obtaining a pruffiate of the required purity in the common mode, is hardly conceivable, except by thofe who have made the attempt. In your Journal, II. 171. I have afferted that both carbonate and fulphate of pot-afh are decompofed by pruffiated barytes; and it has fince occurred to me, that an excellent mode of preparing pruffiate of pot-afh might be founded on thefe facts. For the fmall portion of fulphate of pot-afh, contained in all carbonates, may be expected to be decompofed by the pruffiated barytes, provided this laft be employed in fufficient quantity. On making the experiment, the re-fult anfwered my expectation ; and I fhall, therefore, give the procefs, which I have fince frequently repeated with the fame fuccefs, under the form of a practical rule.

1. Calcine any quantity of carbonate of barytes in a ftrong fire, a fufficient length of time to expel its carbonic acid. Diffolve the pure barytes in boiling water, and add by degrees pure Pruffian blue in powder, till it ceafes to be difcoloured. Filtre the folution through paper, and if it fhould become muddy during cooling, owing to the depofit of oxyde of iron, filtre it again. After having ftood a few hours, fmall yellowifh cryftals will ap-pear, which are the pruffiated barytes. From the remaining folution a further quantity of cryftals may be obtained by evaporation.

2. To a folution of carbonate of pot-afh, gently heated, add by degrees the pruffiate of barytes in powder, till the folution no longer reftores the colour of reddened litmus paper.

Rather

Rather more of the pruſſiate of barytes ſhould be employed than is required to decompoſe the carbonate. After digeſting the mixture about half an hour, filtre the liquor, which, when gently evaporated, will ſhoot into beautiful cryſtals of pruſſiate of pot-aſh. Theſe cryſtals contained, in one inſtance, 24 per cent. of oxyde of iron; but of this oxyde a conſiderable part may be ſeparated, by digeſting the ſolution, before evaporating it, with a little acetic acid (radical vinegar.) The acetic has this advantage over all other acids, that its combination with pot-aſh affords a ſalt incapable of cryſtallizing; and which cannot, therefore, mix with the cryſtals of pruſſiated pot-aſh.

The carbonate of barytes, which is thus re-generated, may be reſerved; and may be again fitted by calcination for the ſame proceſs.

With the view of deciding the queſtion in diſpute, I added to a ſolution of pruſſiate of pot-aſh prepared, in the above manner, a ſolution of muriated barytes, No precipitation enſued; nor was the tranſparency of the mixture in the leaſt degree diſturbed. This experiment, therefore, in addition to thoſe before related, convinced me that I had been deceived; and that Meſſrs. Meyer and Klaproth were correct in denying the precipitation. Happening, however, to examine the jar, about half an hour afterwards, I obſerved that a number of ſmall cryſtals had formed on its inner ſurface; and theſe, on allowing the mixture to ſtand a few hours, increaſed conſiderably in number. I next mixed the two ſolutions in larger quantity, with the ſame reſult. No immediate effect was apparent; but after a few hours, an abundant crop of ſmall cryſtals had formed at the bottom and on the ſides of the jar. Theſe cryſtals had the following character:

1. They diſſolved very ſparingly in cold water, viz. in about the proportion of a quarter of a grain to each ounce.

2. Hot water diſſolved them more readily, but ſtill only in very ſmall proportions.

3. From theſe watery ſolutions ſulphate of pot-aſh, or ſulphuric acid, precipitated ſulphate of barytes; and on adding the ſulphate of iron to another portion of the ſolution, an abundant precipitation of Pruſſian blue occurred.

4. The cryſtals were totally diſſolved by dilute muriatic acid; and the ſolution gave decided marks, to the proper teſts, of containing pruſſiated barytes.

5. When heated ro redneſs in a ſilver veſſel, they grew black and loſt their form. On adding dilute muriatic acid to this coal, an efferveſcence enſued, and the ſolution was muriated barytes.

The foregoing characters are peculiar to the ſalt termed pruſſiate of barytes, the formation of which, under the above circumſtances, proves, beyond all doubt, that a double elective affinity is exerted between muriate of barytes and pruſſiate of pot-aſh. In this reſpect, barytes differs from other earths, and approaches the metals.

The above experiments furniſhed a clue, which led to the explanation of the contradictory properties, obſerved, at different times, in the precipitate from muriated barytes by pruſſiate of pot-aſh. In trials, which I have ſince made, it has uniformly happened, that common pruſſiate of pot-aſh has precipitated muriate of barytes. When the

fupernatant

fupernatant liquor was decanted immediately, the precipitate, after having been wafhed, proved to be fimply fulphate of barytes; but when the two folutions were allowed to ftand together.fome time, before feparating them from the precipitate, this laft had conftantly the properties of a mixture of fulphate of barytes, with prufliate of barytes; and fometimes cryftals of the latter falt were perceivable in the mixture. ·

Before taking my leave of the fubject, allow me to correct an erroneous ftatement, which I have made in your Journal, II. 171. I have there ftated that cauftic barytes does not precipitate prufliate of pot-afh. This experiment I have fince repeated with pure prufliate defcribed above. No immediate precipitation enfued (the circumftance that before mifled me); but after a few hours fmall cryftals were depofited, which had every character of prufliate of barytes. It follows, therefore, that in a table of the elective affinities of Pruffian acid, barytes fhould be placed above pot-afh.

I am, Sir, very refpectfully,

your obedient fervant,

WILLIAM HENRY.

VIII.

On the Genuinenefs and Purity of Drugs and Medicines. By Mr. FRED. ACCUM.

(Continued from page 122. Vol. II.)

EXAMINATION *of* ALKALINE SUBSTANCES.

Prepared Kali. Pharm. Lond. *Salt of Tartar.*

THE falt of tartar of the fhops, if prepared according to the London Pharmacopeía, cannot be obtained pure from pearl-afhes by mere folutions, colature, evaporation, and cryftallization; hence this article generally contains a confiderable portion of *fulphate* and *muriate of potafh*, and is never perfectly free from *filiceous* and *calcareous earth*. In order to difcover its purity, one part ought to be diffolved in eight parts of diftilled water, and then neutralized with pure nitric acid: if the mixture remain perfectly tranfparent when cold, the alkali is free from filiceous earth; whereas on the contrary, if filiceous earth be prefent the mixture will become turbid, the filiceous earth will be feparated, and its quantity may thus be afcertained.

Sulphate of pot-afh, the next article generally found in this falt, is beft difcovered by adding gradually to a fimilar neutralifed folution, a folution of muriate of barytes, or acetite of lead. If fulphuric acid be prefent, a white precipitate will be produced, but no fuch effect will take place if no fulphuric combination be prefent. One hundred parts of the precipitate, occafioned by the admixture of the muriate of barytes, indicate 26 per cent. of ful-

phuric acid, and 100 parts of the acetite of lead indicate 30 per cent. of acid. The prefence of muriatic acid may be afcertained by adding to a like faturated folution of this falt, a few grains of nitrate of filver. If the alkali did not contain any combination of muriatic acid, it will have no effect upon it; but if the minuteft quantity of muriatic acid is prefent, a white precipitate will be produced, which after fome time acquires a bluifh hue; neither nitric nor acetic acid will rediffolve it. Calcareous earth is manifefted in a fimilar manner, by adding drop by drop, a folution of oxalic acid. The quantity of the precipitate thus obtained, if calcareous earth be prefent, gives the proportion of this earth in a given quantity of the falt fubmitted to the experiment.

Pure falt of tartar, which ftands all thefe tefts, is of a fnow whitenefs, inodorous, perfectly dry, and foluble without *any* fediment in an equal quantity of pure cold water, it then contains generally 70 parts of alkali, 23 carbonic acid, and 7 of water.

Water of Kali. Pham. Lond.   *Solution of Salt of Tartar.*

When falt of tartar is expofed in a fhallow veffel to the action of a moift atmofphere, it foon deliquiefces, or attracts fo much water, as is fufficient to render it liquid. This fluid ought to contain at leaft one part of alkali in four. If fo, it feels extremely greafy or flippery between the fingers, on account of it diffolving the epidermis, and converting it into a kind of foap. If the alkali made ufe of has been pure, this fluid is *perfectly colourlefs*, clear, void of fmell, and poffeffes all the properties of a pure falt of tartar. But as the alkali made ufe of in general, by our chemifts, is far from being fo, the article we meet with has generally a yellowifh tinge, a difagreeable, fomewhat urinous, fmell, and foon depofits a confiderble quantity of earthy infoluble matter.

Water of Pure Kali. Ph. L.   *Solution of Cauftic Pot-afb.*

As it is called in our Pharmacopeia, ought to be a folution of cauftic alkali in water, of fuch a ftrength that an exact pint fhall weigh juft 16 oz. troy. When well prepared, it is as limpid as water, and inodorous. It ought not to occafion any precipitate with lime water, neither ought it produce the leaft effervefcence with acids. A redundancy of lime is beft known by blowing into this fluid, through a tobacco pipe, or other tube. If too much lime be prefent, the fluid will then turn turbid. Inftead of obtaining this article in the fhops, we take common foap-leys of the foap manufacturers, diluted with water, hence this article is always yellow, and hence the redundancy of lime, and other heterogeneous falts found in it.

Prepared Natron. Ph. L.   *Mineral Alkali.*

As it is no eafy tafk to obtain mineral alkali *perfectly pure*, we need not expect to find it fo in commerce. This article, if prepared agreeably to the directions of the Royal College of Phyficians, cannot but be contaminated with *fulphate* and *muriate of foda*. But what is ftill worfe, the fraudulent chemifts mix fulphate of foda with it intentionally. In

order

order to difcover the fulphuric and muriate falt, a determinate quantity of the alkali is to be faturated with acetous acid, and then divided into three different glaffes. By adding to the firft gradually a folution of muriate of barytes, the quantity of fulphuric acid may be difcovered, in the fame manner as mentioned before. In the fecond glafs, having let fall a few drops of nitrate of filver, the quantity of muriatic acid may be inveftigated in a fimilar manner ; and by adding gradually a little tartareous acid to the third glafs, an acidulous tar-traite of pot-afh will be feparated in the form of a white powder, 300 parts of which con-tain 107 per cent. of vegetable alkali, as $75\frac{1}{3}$ per cent. pure alkali, free from carbonic acid.

The beft mineral alkali, free from any heterogeneous admixture, cryftallizes in oblong druneated rhomboidal cryftals, as clear as glafs. It is foluble in an equal quantity of cold water, and generally contains 80 parts of alkali, 64 carbonic acid, and 256 of water.

Water of Pure Ammonia. Ph. L. *Water impregnated with Ammoniac.*

The *perfectly cauftic* fpirit of fal ammoniac, as it is ufually called, is one of thofe articles we feldom meet with. It generally contains a confiderable portion of carbonic acid, and is alfo not fufficiently ftrong (particularly for philofophical purpofes). If this liquid is confiderably ftrong, a phial containing exactly 224 grains of diftilled water muft comprife no more than about 216 grains of aqua ammonia.

Water of Ammonia. Ph. I. *Solution of Carbonate of Pot-afh.*

In order to judge of the relative ftrength of this article, which is nothing elfe but a mere folution of carbonate of ammoniac in water ; ftrong ardent fpirit may afford no mean criterion of the ftrength or proper faturation, as well as of the purity of this fluid. If this fluid be fully faturated with carbonate of ammoniac, a quantity of ftrong ardent fpirit, poured flowly down the fides of the glafs, will feparate the alkaline falt, by uniting with the water, which kept it in folution, in confequence of which an opake denfe coagulum will be formed on the furface where the fluids touch each other ; and if on fhak-ing them brifkly together, the whole mixture fhould become converted into a nearly con-fiftent mafs, this article is very good. Whereas, on the contrary, if weak, the effect will be far lefs confiderable, and the coagulum will conftantly remain feparated into two diftinct parts, viz. a folid and a fluid one.

Water of ammonia fully faturated with carbonate of ammoniac has the fpecific gravity 1,150.

Prepared Ammonia. Ph. L. *Carbonate of Ammoniac.*

This falt fhould be totally volatile if laid on an ignited fubftance. It is frequently of-fered for fale by chemifts in the form of a coarfe powder, which is nothing elfe than a mixture of muriate of ammoniac and pot-afh ; this compofition of courfe is not entirely vola-tilifed by heat The carbonate of ammoniac imported from abroad always contain veftiges

of iron, and fometimes a confiderable quantity of lime. In order to difcover the prefence of iron, a little of the falt is to be faturated with acetic acid, a few drops of pruffiate of pot-afh is then to be added, which will impart to the folution a blue tinge if the minuteft quantity of iron be prefent. The prefence of calcareous earth is manifefted by means of oxalic acid, occafioning a white precipitate of carbonate of ammoniac; that containing the leaft quantity of iron has a yellowifh tinge, whereas pure carbonate of ammoniac is of a fnow whitenefs, perfectly dry, fomewhat fonorous, volatile before 212° Fahr. and foluble in 2 parts of diftil-led water at 55° Fahr. Its fpecific gravity is generally 1,4076, and its component parts are 40 ammoniac, 53 carbonic acid, and 7 water.

Salt of Hartfhorn. Ph. L.   *Carbonate of Ammoniac foiled with Animal Oil.*

The genuinenefs of this article (which differs from the former, merely, in being impreg-nated with a fmall quantity of animal oil) may be difcovered in a fimilar manner.

Solution of the above in an Aqueous Fluid. Volatile Liquor of Hartfhorn. Ph. L.

Is in general imitated by mixing a quantity of aqua ammoniae purae with the officinal fpirit of hartfhorn, in order to make it ftronger, and to give it the appearance of a fpirit well faturated with volatile falt. The fpirit thus becomes more pungent, fo as to bear an addition of a confiderable portion of water, without betraying the impofition either by the tafte or fmell. This fraud may be difcovered by adding ftrong ardent fpirit to the fophifticated fpirit; for if no confiderable quantity of cryftallized volatile falt becomes precipitated, the adulteration may be fufpected. It may even be difcovered by the circumftance of the fpirit not effer-vefcing brifkly with acids.

Spirit of hartfhorn carefully diftilled is colourlefs. Its tafte is not very unpleafant (faline not pungent), and its fmell fomewhat empyreumatic. By long keeping it contracts a yellowifh tinge, and acquires a bitter tafte and naufeous fœtid fmell. If fully impregnated with volatile alkali, its fpecific gravity is about 1,500.

IX.

A Memoir on the Ammoniuret of Cobalt, and upon a new Acid contained in the Grey Oxide of that Metal, known by the Name of Zaffre. By LOUIS BRUGNATELLI*.

MY attention has been for fome time directed to the ammoniacal combinations of the metals, or the ammoniurets. They have hitherto been little examined, excepting that of copper, though their characteriftic properties diftinguifh them very prominently from other

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* Tranflated from the Italian manufcript of the author, by Van Mons, into French in the annals de Chemie XXXIII, 114, whence the prefent tranflation is made.

bodies.

bodies. The ammoniuret of cobalt is that to which I directed my first researches *, and I now send you an account of the peculiarities they have offered.

1. I had several times observed that the precipitate, formed by ammonia in the solution of the nitrate of cobalt, was redissolved in that alkali. I collected this precipitate on a filtre, and washed and dried it. Its colour was dark. Upon half an ounce of this precipitate I poured two ounces of liquid ammonia, after which I closed the bottle, and left the mixture at rest; the temperature of the atmosphere being 20° above the zero of Reaumur. At the expiration of 24 hours the alkali had acquired a deep red colour †, and the precipitate was entirely dissolved. I supposed I had formed a perfect ammoniuret; but nevertheless I endeavoured to procure a larger quantity by other means.

I attempted without effect to dissolve smalt, or the blue (glass) of cobalt, in caustic ammonia, even with the assistance of long continued digestion; but solution took place without difficulty with the grey oxide of the same metal, commonly called Zaffre. This substance afforded me an easy means of procuring the ammoniuret in abundance, and to repeat and vary my experiments in several different manners.

3. I evaporated the ammoniuret obtained from zaffre to dryness. The concrete residue which I obtained was composed of two very distinct substances, one of which was of a deep red colour, and the other of a pale yellowish tinge.

4. I poured distilled water upon this residue, agitating the mixture with a spatula of glass. The red portion was totally dissolved, and communicated a beautiful rose-colour to the water. The yellowish matter remained undissolved.

5. The yellowish substance may be obtained by evaporating the liquid ammoniuret, from which it is separated, the moment the fluid is reduced to half its original bulk. The red substance remained dissolved in the last fourth part of the liquid ‡.

6. Ammonia therefore takes from zaffre in its solution two very distinct substances, of which the one soluble in water has a red colour, and the other insoluble in the same liquid is yellowish. This last substance is the pure oxide of cobalt. We think we have discovered a peculiar acid in the former, distinct in its properties from every other known acid.

Concerning the pure Oxide of Cobalt.

7. The yellowish substance which is separated by flow evaporation in the air, or in the sun's light, may be considered as the pure oxide of cobalt. In fact it is insipid, inodorous, insoluble in water, and soluble in the mineral acids. The nitro-muriatic acid forms with it

* I have since examined the ammoniurets of mercury, zink, copper, and arsenic. B.

† Bergman observed only that ammoniae assumed a red colour with cobalt. "Cobaltum," says he, "a niccolo differt quod omnibus acidis et alcali volatile solvatur colore rubro." This circumstance has been repeated by chemists without addition. B.

‡ A yellow matter is separated from the ammoniuret of cobalt, if kept for a time, even in well closed bottles. B.

4.

a yel-

a yellowish folution, which lofes moft of its colour by the addition of a fmall portion of dif-tilled water.

8. This folution may be ufed as a fympathetic ink, like the common muriate of cobalt. Sometimes the acid refufes to diffolve the whole of the oxide; but the folution is com-pleted by the addition of a fmall quantity of water.

9. This folution is precipitated by the pruffiate of pot-afh, of a blueifh green, which does not change. It is not precipitated by the gallic acid, but the folution becomes of a deeper colour. In other refpects this folution has the fame habitudes as the common folution of cobalt in the fame acid.

10. The muriatic acid diffolves the yellow oxide very well, and affumes a very deep green colour, which immediately difappears with the nitrous acid, and alfo by a fmall portion of water. But it is revived again by the addition of a fmall quantity of well concentrated muriatic acid.

Of the pure Ammoniuret of Cobalt.

11. The yellow oxide is totally foluble in ammonia, and forms the pure ammoniuret. Its colour is yellow, and fometimes rofe coloured; the acids do not decompofe it; the muriatic acid difcolours it: pruffiate of pot-afh gives it a grey colour, and afterwards throws down a precipitate of the fame colour. The fulphuret of pot-afh caufes it to affume a deep colour, inclining to black, and throws down fulphuret of cobalt *.

Concerning the Acid obtained from the Ammoniuret of Cobalt, and its Properties.

12. The red fubftance of the preceding experiments was feparated from the yellow oxide. For this purpofe I evaporated the liquid ammoniuret in the fun, and when it was reduced to about one-fourth of its volume, and no longer afforded any yellow precipitate, I filtered it through paper. The fluid was of a deep red colour, like that of cochineal. This liquid emitted no fmell, but its tafte was very fharp. It was expofed to the fun's light till perfectly dry.

13. The remaining mafs was diffolved in diftilled water, which acquired a fine ruby colour. This folution exhibited unequivocal figns of acidity. It depofited on cooling fome fmall brilliant cryftals, which I found to be a combination of the acid with am-monia. This acid appearing to be diftinct from every other, I have thought fit to call it the *cobaltic acid.*

In order to afcertain whether heat was capable of raifing the cobaltic acid, I fubmitted one pound of the ammoniuret of cobalt to diftillation in a retort. When three-fourths of the liquid had paffed into the receiver, I ftopped the diftillation. The retort contained

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* The fulphuret of cobalt, dried in the air, refembles zaffre in its colour. When rubbed on paper, it affumes the metallic brilliancy, as do moft of the other metallic fulphurets. It emits a fulphureous fmell when heated, and takes fire when thrown on ignited coals. B.

a liquid

a liquid rendered turbid by the precipitated yellow oxide. I decanted the liquid after the precipitate had settled, and evaporated it to drynefs. The residue was yellowifh, and partly foluble in water, to which it communicated a yellow tinge. This folution exhibited all the acid characters of the red liquid, No. 15.

15. In another experiment, I put one pound of the ammoniuret of cobalt into a retort, and urged the diftillation to drynefs. The fixed matter which remained on the bottom of the retort was blue at the furface, but internally yellow. After fome hours the blue colour totally difappeared, and was changed into red.

16. A remarkable difference between the refidue of the evaporation by the fun's light, and that by the fire, was that the latter tranfmits the cobaltic acid nearly colourlefs ; fo that the cold folution is nearly as limpid as water. I have befides remarked, that by this laft procefs the acid contains little or no cobalt of ammonia.

17. I fhall here prefent the principal characters which diftinguifh this new acid. They are

1. Its form is concrete, and it is not volatilized by fire.

2. In fome inftances it is red (13); in fome pale yellow (14); and in fome colourlefs (16).

3. It is without fmell.

4. It has a fharp, and not unpleafant tafte.

5. It ftrongly reddens the tincture of turnfole.

6. It is perfectly foluble in water.

7. It decompofes all the fulphurets of alcali, and precipitates the fulphur.

8. It precipitates the ammoniuret of copper of a light green, and that of zink of a clear white.

9. It precipitates the fulphuret of copper of the fame colour as the ammoniuret of that metal.

10. It precipitates the nitrate of filver white.

11. The nitro muriate of tin the fame.

12. The nitrate of mercury of a light ftraw colour.

13. The acetite of lead white.

14. It does not fenfibly affect the folutions of gold and platina.

15. It precipitates lime water in a white coagulum, infoluble in water, and in excefs of acid.

16. It precipitates the acetites and muriates of barytes.

17. It is feparable by alcohol from its folution in water.

18. When employed as a fympathetic ink, it does not give a green or blue colour, like the folutions of cobalt ; but gives a brown and afterwards a black colour to paper when rather ftrongly heated, as happens with other acids.

19. It affords, with new made tincture of galls, a yellow abundant precipitate.

20. With

20. With the faturated folution of foda, it affords an irregular tranfparent falt foluble in water, and not deliquiefcent.

21. With pot-afh it affords a cryftallizable falt in fquare cryftals, which are tranfparent and permanent in the air.

22. With ammoniac it forms a falt foluble in its acid.

23. With barytes it forms an opake falt, cryftallizable with difficulty.

Supplement.

The prefence of an acid in zaffre, induced me to fufpect that it might probably be arfenical : but this doubt foon vanifhed, by comparing the characters of each.

1. The arfenical acid does not precipitate the folutions of filver, as does the cobaltic acid. 2. The arfenical acid precipitates lime water; this arfeniate is re-diffolved by the acid, as well as by a new quantity of lime water. The contrary happens with the acid of cobalt. 3. The arfenical acid does not decompofe the muriate and acetite of barytes, as does the cobaltic acid. 4. The arfenical acid is foluble in alcohol, which precipitates the cobaltic in a concrete form.

It remained to be proved, whether the acid obtained from zaffre exifted ready formed in that oxide, or whether it was produced by the action of the ammonia.

As the acid of cobalt is very foluble in water, I boiled fix pounds of zaffre in eight pounds of water for a quarter of an hour, and filtered the liquid while yet warm. That which paffed was tranfparent and colourlefs; but had a perceptible tafte. I evaporated the liquid, taking care to cover the veffel with a piece of filk. When it was reduced to one half, it became turbid, but the matter which was feparated was not perceptibly coloured. I continued the evaporation till no more than one-third of the liquid remained, and then removed it from the fire. It depofited a very white matter, which the contact of the air changed to a beautiful rofe colour. I feparated this matter, and collected it on the filtre.

The filtered fluid was of a bright yellow colour, and perfectly tranfparent. It had a very decided acid tafte; reddened the tincture of turnfole, fpeedily decompofed lime water, the falts of barytes and of filver, was precipitated by alcohol, &c. In a word, its habitudes were in every refpect the fame as thofe of the cobaltic acid obtained by the foregoing proceffes.

The red depofition which remained on the filtre had no tafte, and coloured the muriatic acid of a very fine green. It was the pure oxide of cobalt. This oxide is largely foluble in its own acid, and is precipitated in proportion as the acid is concentrated.

The acid which the ammonia feparated from the zaffre, exifts therefore ready formed in that fubftance. Its radical ftill remains to be pofitively afcertained. In the mean time I have thought it proper to defignate it by the name of the *Cobaltic Acid.*

X. *The*

X.

The Apparatus of Woulfe improved, fo as not to require luting.   By Cit. GIRARD.[*]

AS the ingenious apparatus of Woulfe, which has been fo highly ufeful in modern chemiftry, always requires a confiderable time for its arrangement in thofe operations in which it is ufed, the attention of philofophers has been long directed to the means of fimplifying it.   Various forms have been contrived to conftruct this apparatus, hermetically clofed, without the affiftance of luting; but they are in general too difficult in the execution.   That which I now offer to the Inftitute, may be put together or difmounted in a few feconds, and appears applicable in all the circumftances of experiment.

It confifts fimply in caufing the veffels to be made at the glafs houfe, with the neck A, to receive the interior tube A B (Pl. II. Fig. 2.) which is welded in the neck itfelf by means of a projection in the fide of the glafs.   The tube on the oppofite fide is firft drawn to a point, or thin tube as ufual; but then inftead of breaking this tube near the bottle, to make it agree with the fubfequent fittings, it is left entire and bended, fo that it terminates in a curve, which may be introduced into the tube A B.   It is unneceffary to remark, that the tube A B, and the neck into which it enters, fhould have the fame curvature, which is very eafily effected by bending both on the fame cylinder of earth or wood.

It may be eafily underftood, that the neck of the firft bottle being introduced into the tube of the fecond, beyond which it projects, and both tubes entirely immerfed in the water, no gas can efcape from the firft veffel, but by paffing through its neck, and through the liquid it contains.

Fig. 3. reprefents another apparatus of Woulfe's, which will fupply the place of the other in cafe of diftance from a glafs houfe.   It is compofed of large necked bottles and tubes.   Take a fomewhat thick tube, of fufficient bore to receive another.   The upper part of this muft be enlarged by heating and blowing, and it may afterwards be ground and made to fit in the form of a ftopper.

Another tube fhould be then taken, which can enter the firft fo loofely that it can be moved with eafe.   The extremity A of this tube is then ground in the fame manner, in order that it may fit another bottle, after which the ufual curve A B C is given to it.

Laftly, it is introduced into the tube E F, fig. 4. and the prominent part D is bended upwards.   The veffel may then be confidered as hermetically clofed, as foon as the lower aperture of the large tube is immerfed in the included water.

The curve of the tubes and necks of the bottles is only intended to direct the bubbles of gas which efcape through the neck, that they may not again enter the tube out of which they are required to pafs.   This curvature is not required, except in fmall apparatus.

[*] Annales de Chimie, xxxii. 283.

When a projection of an inch, or more, can be given to the neck beyond the lower opening of the tube, the bubbles muft neceffarily efcape without that precaution.

Fig 5. exhibits another conftruction of the fame apparatus.

If an apparatus, compofed of a very long feries of bottles were employed, it might be feared, that the gas contained in the firft veffel having a very ftrong refiftance to conquer, would caufe the fluid of the firft bottle to expand and rife in the tube; but it is very eafy to remedy this inconvenience, either by ufing bottles of different fizes, or by putting a fmaller quantity of fluid in the firft bottles, than in the following ones.

XI.

A Memoir on the Difcovery of certain Bones of a Quadruped of the Clawed Kind in the Weftern Parts of Virginia. By Thomas Jefferson, *Efq.*

IN a letter of July 3d, I informed our late moft worthy prefident, that fome bones of a very large animal of the clawed kind had been recently difcovered within this ftate, and promifed a communication on the fubject as foon as we could recover what were ftill recoverable of them. It is well known that the fubftratum of the country beyond the Blue Ridge is a limeftone, abounding with large caverns, the earthy floors of which are highly impregnated with nitre; and that the inhabitants are in the habit of extracting the nitre from them. In digging the floor of one of thefe caves, belonging to Frederic Cromer in the county of Greenbriar, the labourers at the depth of two or three feet, came to fome bones, the fize and form of which befpoke an animal unknown to them. The nitrous impregnation of the earth, together with a fmall degree of petrification, had probably been the means of their prefervation. The importance of the difcovery was not known to thofe who made it, yet it excited converfation in the neighbourhood, and led perfons of vague curiofity to feek and take away the bones. It was fortunate for fcience that one of its zealous and well informed friends, Colonel John Stewart of that neighbourhood, heard of the difcovery, and, fenfible from their defcription, that they were of an animal not known, took meafures without delay for faving thofe which ftill remained. He was kind enough to inform me of the incident, and to forward me the bones from time to time as they were recovered. To thefe I was enabled accidentally to add fome others by the kindnefs of a Mr. Hopkins of New York, who had vifited the cave. Thefe bones are,

ıft. A fmall fragment of the femur or thigh bone; being in fact only its lower extremity, feparated from the main bone at its epiphyfis, fo as to give us only the two condyles, but thefe are nearly entire.

* American Tranfactions, IV. 246.

2d. A radius, perfect.

3d. An ulna, or fore-arm, perfect, except that it is broken in two.

4th. Three claws, and half a dozen other bones of the foot; but whether of a fore or hinder foot, is not evident.

About a foot in length of the residue of the femur was found, it was split through the middle, and in that state was used as a support for one of the salt petre vats; this piece was afterwards lost, but its measures had been first taken, as will be stated hereafter.

(To be continued.)

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Letter from Dr. Beddoes; with a Proposal for a Course of Lectures.

To Mr. NICHOLSON.

SIR,

IF you approve the spirit of the undertaking, announced in the enclosed paper, pray notice it in your Journal. I hope the example will be followed. Whether the number of persons, aware of the paramount importance of the science of animal nature, is at present sufficient to ensure a reasonable compensation to those who may attempt to explain it in lectures, I am not uncertain. But this æra is fast approaching. I am persuaded we shall see institutions for this purpose, similar to those which have long existed, or been lately formed for important, but very inferior purposes.

The success at first experienced in the treatment of palsy by the gas, before spoken of in your Journal, continues. I repeat my undoubting conviction, that the introduction of factitious air into medicine, will amply recompence the pains that have been taken to bring it about. I had engaged an able dissector and draughtsman, for the purpose of co-operating with Mr. Davy and myself in physiological researches. But an opportunity offering for his advantage, I of course relinquished my claims upon him; I am much in want of a successor, and I wish some ingenious young man, accustomed to manage the scalpel and the pencil, may be induced, by this notification, to join us in an enterprize which must be congenial to the feelings of every ardent cultivator of medical philosophy. I should make the conditions as agreeable to him as it lies in my power to do.

I am, Sir,

Your's, with great esteem,

March 2, 1800.

THOMAS BEDDOES.

Lectures

Lectures on the Laws of Animal Nature, and on the Means of preserving the System from Injury upon the most important Occasions of common Life.

At some convenient place in *Bristol,* Dr. Beddoes proposes to attempt a popular exposition of the principles of the animal œconomy, with their application to the purposes of individual and domestic welfare, upon a plan widely different from that of any existing publication. For his opinion on the advantage of disseminating physiological information, he may refer to his *Lecture introductory* to Messrs. Bowles & Smith's Course of Anatomy; and an exemplification of the manner in which he thinks the subject ought to be treated will be found in his *Essay on Consumption.*

Heretofore an acquaintance with the causes of his personal condition has seldom been numbered among the *accomplishments* of the scholar, or the *qualifications* with which the man of business is fitted out for success in the world. Yet it will be confessed, that neither success in business, nor proficiency in the sciences, accounted *liberal,* are separately sufficient for rendering the condition of human life desireable. And, in fact, to endeavour by any combination of these materials, to construct a system of personal happiness, is to project an edifice which shall stand secure without a foundation.—Of a truth, so long and so generally neglected, a portion of the public, it is believed, begins to feel that degree of conviction which operates upon conduct. In this belief, the present opportunity of instruction is offered to those who may be desirous of it.

If it be allowed that the moral and physical attributes of human nature are inseparable, persons interested in the art of education will scarce require to be reminded of the value of that species of knowledge which the lectures, here announced, are intended to communicate.

They ought to prevent many of those mortal bruises which travellers along the road of life give themselves for want of knowing the quality and position of the objects in their way.

By presenting a just estimate of *that art* to the operations of which almost every one is sooner or later doomed to submit, they should afford some protection against gross medical incapacity or fraud.

They should reduce to their just value many of those axioms that wander about the world concerning what is wholesome or unwholesome in diet or exercise :—axioms which the instinct of self-preservation impels men to take up; and upon which, however loosely adopted, they act with as full assurance as if they knew them to have the most solid foundation in physiological science.

Numbers fall victims to their own impatience under illness, or to the wavering conduct of their friends. Frequently in the onset of dangerous diseases, people by suffering themselves to be amused by trifling domestic expedients, lose an opportunity which no medical skill can ever retrieve. Upon these evils the prevalence of juster ideas would act as a check. Nor is it paradoxical to suppose that the mortality among infants would be smaller, and debility of constitution at all periods of life more rare, if parents (however instructed in

other

other things) were not in common nearly upon a level with nurfes in that which it fo much imports them to poffefs—an acquaintance with the powers that operate to the injury or advantage, the deftruction or prefervation, of the objects of their affection.

The Author further hopes (if he may repeat his own words) to contribute towards preventing the " ignorant from tampering with the fick ; towards promoting the afcendancy " of fcience over intrigue; alluring curiofity from the pernicious frivolities of literature, " and elevating the conceptions of men to the level of their higheft interefts."

As the whole courfe will be connected, the tickets will not be transferable—The number of lectures cannot be determined beforehand—But that there may be little chance of exclufion by reafon of narrow circumftances, the fubfcription is fixed at ONE GUINEA—The lectures will be calculated for both fexes and different ages—They will be delivered in the evening, and commence fometime in April next—probably near the middle of the month—provided fifty perfons fhall have entered their names by the 31ft of March. This condition is indifpenfable. Without a tolerably numerous audience, the author prefumes he could beftow his time in a manner more advantageous to the public.

Subfcriptions received by Mr. SHEPPARD, Bookfeller, *oppofite the Exchange*, with whom conditions for printing a SYLLABUS may be feen.

RODNEY-PLACE, CLIFTON, *March* 3, 1800.

Extract of a Letter from M. Fabbroni of Florence, to Cit. Van Mons, on the Ethiops of Iron, the Formation of Alcohol and Fermentation. (*An. de Chim. XXX.* 220.)

Take a pound of iron filings, reduce them to a pafte with water, and put it in a capfcule, or what is ftill better, a glafs matrafs, kept in a water bath at about 50 or 60 degrees (Reaumur). Pour on it gradually one or two ounces of aqua fortis, rather diluted, or of nitrous acid very much diluted, and continue ftirring it with a fpatula. It is remarkable, that it undergoes a kind of effervefcence, after which the iron is changed into a very fine black powder, and oxided to the firft degree of oxidation ; that is to fay, converted into martial ethiops. The operation is performed in lefs than half an hour. If the mixture be put in a clofed veffel, and not fhaken, but left from evening till the next day, the furface of the ethiops is found to be covered with a kind of champignons extremely white, and feveral lines high, which are nothing but volatile alkali, or carbonate of ammoniac. The air in the veffel is in this laft cafe, compofed in a large portion of oxigenated nitrous gas. A decompofition here takes place of the water and nitrous acid, both at the fame time, by the iron which feizes their oxygen, in order to become converted into the oxide ; and the component parts of thefe two liquids, that is, the azote and hydrogen, which being difengaged at the fame time, meet whilft they are yet in a ftate of condenfation, or before they have taken the form of a gas, by combining are transformed into ammoniac. A por-

tion.

tion of oxigen appears alfo to be precipitated on the carbon of the iron, which affords the carbonic acid, and cryftallifes the ammoniac, by faturating it as it is formed.

Chemifts in general, have believed and advanced, that alcohol is the produce of the vinous fermentation. I am affured that it is not all fo, for it does not exift in wine. I think I can render this very evident, if I have a method by which I can recover an hundredth part of alcohol, when completely mixed with ftrong wine, in which, by the fame method, I could not before difcover the fmalleft atom, although I could obtain 20 or 25 of brandy, from a hundred parts by diftillation. Thefe were the fimple means by which it was performed: take a glafs tube of fufficient fize to introduce the finger, and divide it into an hundred equal parts; take frefh wine, with which you have mixed one hundredth part of alcohol, and to that put as much pot-afh in powder, as is found by a previous experiment, to be neceffary to precipitate the refinous coloring refidue. Then ftrain the wine, and pour it into the tube, and add as much pot-afh in powder as is required to faturate it. The centime of alcohol, which had been mixed with the wine, will then be feen to rife, and fwim diftinctly on the furface of the alkaline folution. It will be found of the fame degree of ftrength, and in the fame proportion that it had been added, if the operation is performed with fufficient quicknefs to avoid lofing any thing by evaporation. The previous feparation of the coloring matter ferves merely to render the refult more perceptible. Now, if by this means I procure only the fame quantity of alcohol from the wine that I knew it contained before, and not a drop more, it appears to me, I have a right to conclude, that that which I obtain from the fame wine by diftillation, did not exift in it before, but that it is the operation of diftillation which forms it; that its formation is determined by the heat; and laftly, that this liquid is a product, and not an educt of the diftillation of the wine. The heat neceffary to effect this is not therefore very confiderable, for it is formed by the heat of the fermentation, to an heat of diftillation of 14 degrees, fuch as may be obtained during winter. It may be formed in bottles by the warmth of the atmofphere, &c. It is for this reafon, that I have recommended new wine to be ufed in my experiment. All this has been printed in my *Arte di fare il vino*, which was publifhed at Florence in 1788; but Italian books feldom pafs the Alps. The anatomical examination of the grape precedes the chemical part in this work. The theory of fermentation is there found explained according to the ancient principles, though at that epocha I had, for a confiderable time, renounced the phlogiftic theory. The effential ingredients, the active and paffive principles, that is to fay, of fermentation, are there deduced from experiment; and the making of artificial wine is the confirmation of them. The work is concluded, by expofing the chemical means neceffary to difcover the alteration which adulterated wines undergo, and a fummary of all the theories of fermentation that have been prefented, to the time of its publication. You will there, perhaps, find a phenomenon which contradicts them all, and which is very remarkable. It is that the accefs of air is not effential to fermentation; for I have excited it in the vacuum of Toricelli.

Principles

Principles of modern Chemistry, systematically arranged, by Dr. Frederic Charles Gren, late Professor at Halle, in Saxony. Translated from the German, with Notes and Additions concerning late Discoveries, by the Translator, and some necessary Tables. Illustrated by Seven Plates. In two Volumes, Octavo, 964 Pages. Price 16s. Cadell and Davies.

The character of the late Dr. Gren as a chemical philosopher is well known, as is likewise the value of the work from which the present translation is made. I shall, therefore only give a short account of the plan: After a concise historical introduction, the author enters upon his subject, which he divides into twelve chapters, each of which is again subdivided into heads or titles, and those into numerous paragraphs or sections. The first treats of preliminary or general objects, and the operations and instruments of chemistry. 2. The simple substances and process of combustion. 3. Salts in general. 4. Earths. 5. Mineral Acids. 6. Constituent parts of vegetables. 7. Of animal substances. 8. Spontaneous changes in the mixture of organic bodies, fermentation, &c. 9. Bitumens. 10. Mineral coal. 11. Metals. To these the translator, besides many useful notes and additions through the work, has added an appendix, containing the doctrine and tables of chemical attraction, the chemical characters of Haffenfratz and Adet, specific gravities, comparison of thermometers, of weights and measures, together with an index to the whole.

The Chemical Pocket Book, or Memoranda Chemica, arranged in a Compendium of Chemistry, according to the latest Discoveries, with Bergman's Table of single elective Attractions, as improved by Dr. G. Pearson. Calculated as well as for the occasional Reference of the professional Student as to supply others with a general Knowledge of Chemistry. By James Parkinson. Small Twelves, 229 Pages. Price 5s. Symonds.

This is a very good compendium. By using the small type, called burgeois, the author has contrived to give nearly the same quantity of matter in the pages of his little pocket volume as is usually contained in an octavo page. The greater merit, however, consists in the industry he has exerted in collecting the numerous particulars of modern chemistry from the most authentic sources, and condensing them with a degree of neatness and perspicuity which will render his book equally useful as a manual of science to the philosopher and the student. He quotes his authorities much more frequently than is the modern custom; but it will greatly add to the value of his book if in a future edition he should refer in all places by the real title and page to every author of eminence to whom he might wish to refer his reader.

Royal Inflitution of Great Britain:

A courfe of philofophical and chemical lectures is at prefent delivered at the houfe of the Royal Inflitution by Dr. Garnett, which are received with the moft marked attention by an audience of the firft refpectability. The ordinances, bye laws, and regulations of this eftablifhment have likewife been printed. They confift, for the moft part, of a developement and organization of the fubjects contained in their profpectus of laft year. The managers are charged to erect a laboratory and theatre for lectures, and to appoint profeffors. They elect all the fubfcribers. Proprietors are to be propofed as candidates by a manager, and elected at the fubfequent monthly meeting by a majority of two-thirds, and the fum to be paid will be 6ol. after May, 1800;—7ol. after May, 1801;—8ol. after May, 1802;—9ol. after May, 1803; and 100l. after May, 1804. Subfcribers for life, or annual, muft be propofed by a manager, and elected at the fubfequent weekly meeting. Ladies for thefe two claffes are admitted by recommendation of certain ladies intrufted by the managers to hold books for that purpofe. I do not find any fpecific mention of the fums to be paid by life and annual fubfcribers; but upon enquiry at the houfe, I am informed, that on Friday the 7th of this month, the fum for the former clafs was raifed from ten to twenty guineas, and for the latter from two to three guineas. Whether the managers of this great eftablifhment have fhewn a *laudable* partiality to their own clafs by giving them five regular notices of the advances they are to pay, while the the reft of the public is called upon without ceremony for augmentation of fifty and one hundred per cent. may perhaps be queftioned.

Plate 1. Philos Journal Vol IV facing page 48

Mr. Boswell's Blast Ventilator.

Fig. 1.

Fig. 2.

C

E F B

D H

Fig. 3.

G B

E

H

F F

C C

D

D

A

A

Plate 2.Vol. IV. Philos. Journal facing page 48.
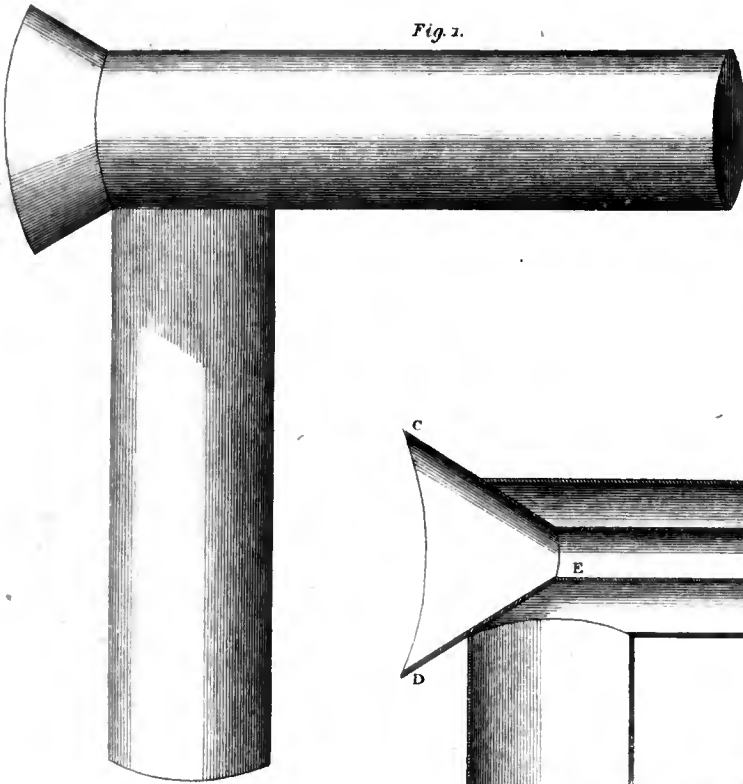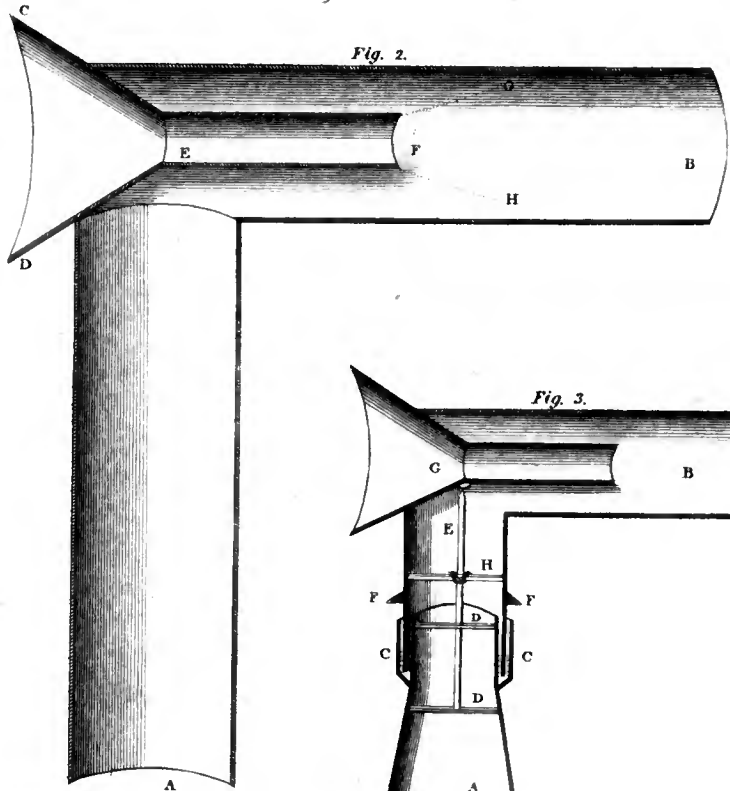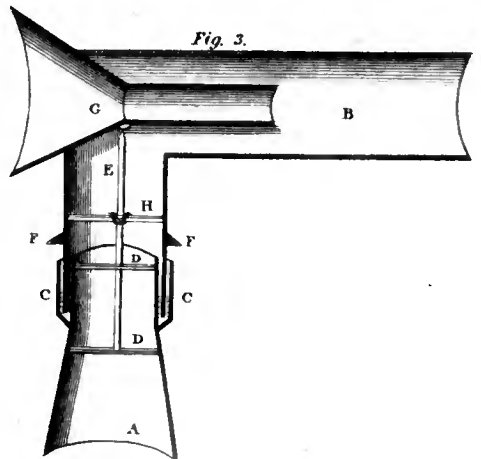
Hydraulic Machine at Schemnitz

Apparatus without Lute

Fig. 2.

Fig. 1.

1 2 3 4 5 6 feet french.

Fig. 3.

Fig. 4.

Fig. 5.
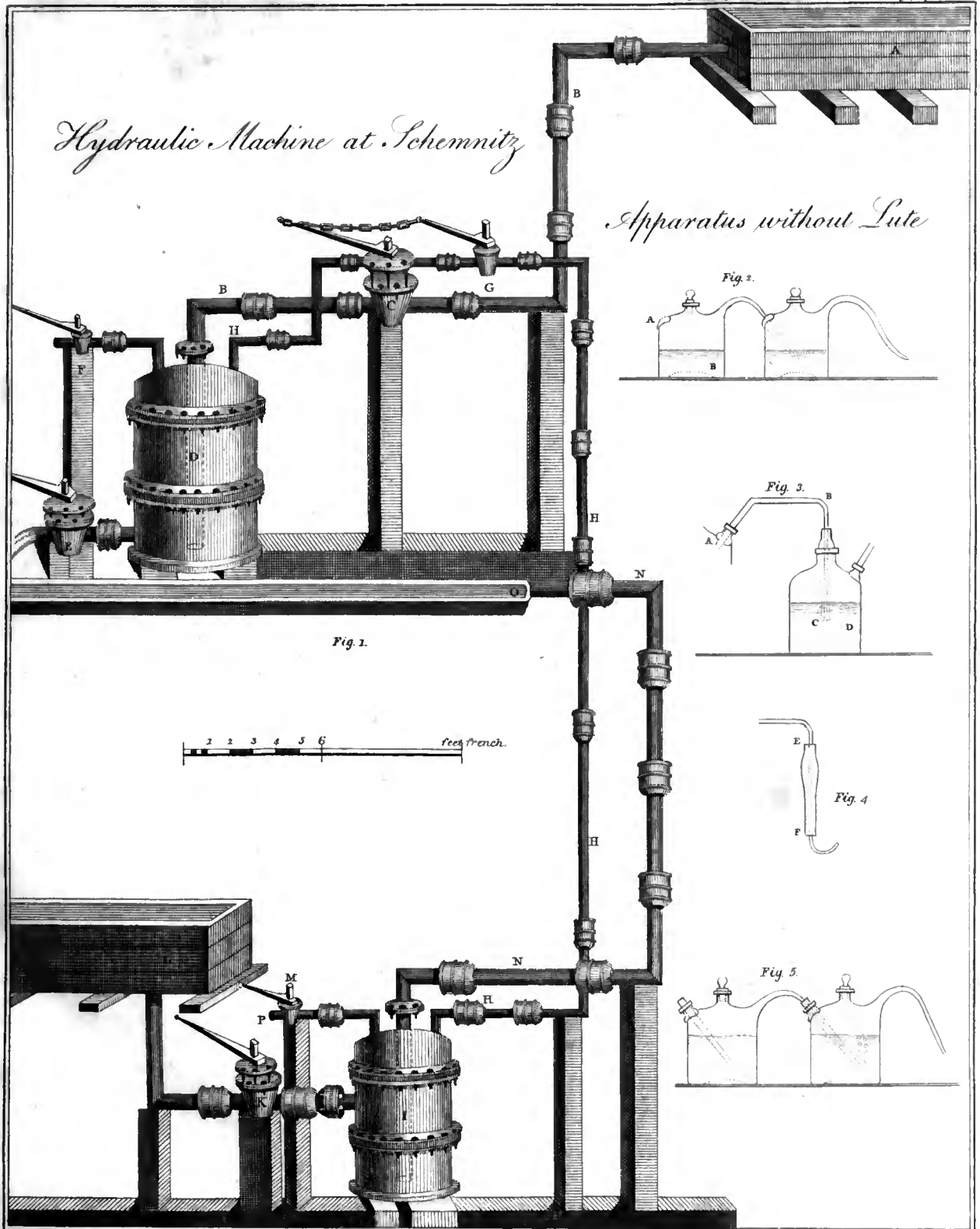
ARTICLE I.

On the different Effects of the Alkalis in the Production of Alum. By Professor HILDEBRANDT, *of Erlangen* *.

IT is a known fact, in the usual preparation or extraction of alum, that pot-ash, or urine, is added to the first solution, which is procured by lixiviating the roasted and weathered aluminous ores. *Bergman* †, being of opinion that these additions tended only to saturate the superabundant acid of the lye by which the crystallization of the salt is impeded, has advised to add pure aluminous earth, or argil, instead of the preceding substances, on the supposition that those additions do contaminate the alum with foreign salts; while, on the contrary, pure alumine would not only saturate the superfluous acid, but increase the quantity of the sulphate of alumine. *Professor Lampadius* ‡, who adopted the same opinion, tried by experiments, according to Bergman's idea, whether he could not render

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* Translated from Dr. Scherer's (now Professor at Halle in Saxony) Chemical Journal, Vol. II. pag. 419.

† *Bergman, de confectione aluminis,* § II. Opusc. Chem. et Phys. I. Lips. 1783. 8vo. pag. 308. Quum autem argilla pura basin aluminis constituat, excedens lixivii acidum nullo alio modo melius, quam ea donatur, quæ superfluum acidum non tantum aufert, sed etiam salis quæsiti copiam auget.

‡ *Lampadius,* pract. chemische Abhandlungen I. Dresden 1795, § 14.

the addition of pot-afh or urine unneceffary, and with this view he had, in the alum-works of Lederbur at Weifgrun, added to the crude lye fome white argil, dug up near the village Chotta.

The fuperabundant acid here fpoken of, is not that portion which is effential to the alum, the acid in this falt being never fully faturated with argil; but that quantity only is meant, which may happen to be prefent in the lye as foreign, or not belonging to the compofition of alum, as is the cafe, when the aluminous ore is too abundant in fulphur, or when a fufficient portion of its argillaceous part has not been combined with the fulphuric acid generated in the procefs.

If we admit that the crude lixivium of alum does really contain an excefs of acid, and that it is this only which impedes the cryftallization of the falt, the addition of argil feems much more proper, than that of either pot-afh or urine; becaufe more alum muft be obtained by the former, while the latter affords fulphate of pot-afh or ammoniac, which diminifh the purity of the product.

But *Marggraf* * has long ago obferved, that the fulphate of alumine cannot be generated from argil and fulphuric acid alone, and that an alkaline falt is one of its effential ingredients. To prove this fact, I have feveral times diffolved fome aluminous earth (that had been precipitated from Roman alum by means of pot-afh, and properly edulcorated) in fulphuric acid carefully rectified, and attempted to cryftallize the clear filtered lye, by evaporation and cooling. But I never obtained the leaft appearance of cryftallized alum. The mixture confifting of argil and fulphuric acid, rather in excefs, is more foluble in water than true alum : fo that two parts of water at a temperature of $50°$ Fahrenheit, are fufficient to diffolve one part of that mafs, merely dried without ignition; whereas the alum requires at the fame temperature, 35, or more parts of water, to diffolve it. Even after that lye had been reduced by evaporation to a much fmaller compafs, than a lixivium of true alum would admit of without cryftallizing, it continued liquid, though expofed to a freezing cold. On evaporating it ftill farther, and by fubfequent cooling, it affords indeed a compact mafs, but without any determinate figure, which occupies the bottom of the veffel in the form of a dried jelly. It was but feldom that, in a certain degree of the evaporation, I could perceive a kind of cryftallization in groups; but there was no appearance of fuch hard octahedral cryftals, as prefent themfelves in a folution of true alum, containing upwards of twenty parts of water to one of falt diffolved in it †.

If, on the contrary, to the above folution of argil in fulphuric acid, a fmall quantity of vegetable alkali only be added, and it be then evaporated and left to cool, regular cryftals of alum will be obtained at each fubfequent refrigeration.

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* *Marggraf*, Chim. Schriften. I. Berlin, 1768. S. 196.

† *Marggraf*, however, obferved that when the argil was ftrongly calcined, it afforded fome cryftals with the fulphuric acid, which refembled thofe of alum, but were not fo fine as thofe obtained by the addition of an alkaline lye. I wifh he had ftated this more accurately. But, as my prefent object was to afcertain, which of the alkalis may be moft fuitable to the generation of alum, I have not yet repeated that experiment.

In order to difcover the proportion of the alumine to the fulphuric acid, nothing more is required, than to combine as much of the earth with the acid, as it will diffolve in the common temperature (from 50° to 60° Fahrenheit). An excefs of the acid will, in that cafe always remain, as fhewn by the teft of Litmus. For at that temperature the acid does not take up fo much of the argil as to become neutralized. The folution muft, however, be left ftanding long enough (24 hours) upon the remaining undiffolved part of the earth, becaufe the argil is diffolved much more flowly by this acid, than the calcareous and magnefian earths.

In order to difcover the due proportion of pot-afh in this procefs, *Marggraf* directs us to continue adding gradually of the alkaline lye by drops, till a light pulverulent precipitate (that is to fay, precipitated argil) appears. In the following experiments I added fo much alkali, whether pot-afh, or either of the other two, that (the quantity of diffolved pure alumine being half an ounce) there remained a fufficiently perceptible quantity of precipitated earth, though very fmall in proportion to the whole. For, it is to be obferved, that the very firft drops of the alkaline lixivium, if neither itfelf nor the folution of the earth be too weak, produce a turbidnefs; but this firft precipitate is re-diffolved in the excefs of the acid prefent. I am much inclined to fuppofe, that it may be immaterial to determine the precife quantity of alkali, becaufe I have conftantly obtained alum by the addition of pot-afh, notwithftanding that in fome inftances I added more of it, and in others lefs.

To afcertain the regular form of the cryftals of alum, one or more threads fhould be hung into the lye, in order that perfect, or nearly perfect octahedrons may be formed round them; for thofe cryftals which are formed at the bottom, or on the fides of the veffel, are always defective in one or more of their angles.

Having decided from thefe experiments, that a fmall portion of alkali is effentially requifite to the production of fulphate of alumine, I mentioned the fact in the advertifement of *Prof. Lampadius's Chemical Effays*. It cannot be denied, but that in one of his experiments * he has obtained alum, from mere argil and fulphuric acid, without the addition of either pot-afh or urine : but not to mention, that the quantity of alum obtained by the faid procefs was indeed exceeding fmall; in proportion to what has been produced on adding pot-afh or urine, it is probable, that as much vegetable alkali as was fufficient for the formation of that fmall portion of alum may have entered into the crude lixivium, from the afhes of the wood employed in the roafting of the aluminous ore, and of which a greater or lefs portion is unavoidably mixed, with the roafted mineral. Or that alkali may actually have pre-exifted in the ore; (or if not, at leaft the nitrogen and hydrogen, from which the alkali was generated, may have been prefent.) *Profeff. Klaproth* † has difcovered pot-afh in

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* *Lampadius,* loc. cit. pag. 16.

† *Klaproth* difcovered in the *native alum* from *Mifeno* fo large a proportion of pot-afh, that 1000 grains of it afforded 470 grains of cryftallized alum, and in thefe 27 grains of fulphate of pot-afh. (*Beytrage zur Kentnifs der Mineral Koerper.* I. Berlin, 1795. deile 313.)

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the *native alum* from *Miseno*. Only one part of its aqueous solution would crystallize into genuine alum, in which also he traced out the alkali; the remaining portion of the lye yielded no alum spontaneously, but only when a little pot-ash had been superadded. *Bergman* * likewise, found that alkali in the artificial alum, even in the Roman; and Prof. *Lampadius* † himself considers it at present as an essential-principle of alum, in consequence of having found it in four different species of factitious alum: namely, those from *Commotin* and *Weissgrün* in Bohemia, and those from *Reichenbach*, and from the *Schwemsal* in Saxony.

On considering the share, which the vegetable alkali has in the production of alum, the question naturally arises: whether *pot-ash* alone, or *soda* likewise, as well as *ammoniac*, are capable of combining with sulphuric acid and argillaceous earth into alum ‡ ?

The aptitude of volatile alkali to enter into such union, seems to be deducible from the circumstance, that, in some alum-works, urine only and no pot-ash is employed. However, as I have already observed in another respect, it is not impossible but that a quantity of vegetable alkali may be afforded by ashes formed in the roasting of the alum-ore, sufficient to complete the alum.

The alkaline portion of the alum may be ascertained in three different ways by analysis.

1. Let the argil be thrown down from a watery solution of alum by means of *ammoniac*; cause then the salt contained in the clear decanted lixivium to crystallize, and subsequent to this expose it to a subliming heat. In this process, the *sulphate of ammoniac* sublimes, but the *fixed sulphate of pot-ash*, if any has been in the alum, remains behind it in a compact form. This method discovers the presence of fixed alkali only.

2. To a boiling hot aqueous solution of alum, add carbonate of lime. By this management the sulphate of argil is decomposed, and the lime is converted into gypsum; at the same time that the sulphate of pot-ash, if any was present, remains unaltered. This last would likewise be decomposed by caustic or pure lime; but it is well known that the crude calcareous earth, on account of its strong attraction for carbonic acid, has no such power on sulphate of pot-ash. What quantity of crude lime may be necessary to accomplish that decomposition, may in some manner be determined, by the known proportions of argil to sulphuric acid in alum, as well as by the proportion of calcareous earth to the same acid in gypsum, and lastly that of crude lime to carbonic acid in carbonate of lime. However, as this determination is subject to some difficulties, it will be sufficient to be attentive to the effervescence of the lime, and according to this effect, to add rather too much than too little, because the redundant calcareous earth falls down, unchanged, together with the

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* *Bergman*, cit. § 77, page 307. Yet he says: *haud raro*.

† *Lampadius*, Pract. Chim. Abhandlungen II. 1797. S. 228.

‡ *Marggraf* (cit. loc. page 196) says only, that the addition of fixed alkali is necessary.----*Lampadius* (cit. p. 230) has not yet given any results of his experiments, made with the mineral and volatile alkali.---- *Bergman* (cit. 307.) says: *crystallizationis obstaculum alcali volatili æque tollitur, non vero alcali minerali et calce*; yet without relating any experiment.

<div align="right">precipitated</div>

precipitated argillaceous earth, from which it may be feparated by the known methods, if it fhould be required to afcertain the quantity of the argil. A certain portion of the fulphate of lime, thus produced, is likewife precipitated at the fame time, and in greater quantity, the lefs water there is in the folution. The remainder of the gypfum continues diffolved in that fluid. Let the liquor be carefully poured off from the precipitate, and let this laft be edulcorated with a fufficient quantity of boiling water ; let the wafhings be added to the fluid at firft decanted ; and the whole of the fluid be gradually evaporated, and left to cool, by alternations. By this management the gypfum will by degrees be feparated, and afterwards the fulphate of alkali, which was an ingredient into the alum, whether it has been pot-afh, or ammoniac. The fulphate of lime will fall firft, becaufe moft difficultly foluble ; the fulphate of pot-afh will be the next in order ; and, laftly, the ammoniac, being the moft foluble of all. If at the fame time the alum under examination has contained a portion of fulphate of foda *, it will be more difficult to feparate this from the fulphate of ammoniac, becaufe it is alfo very foluble. But it will prefently be fhewn, that no foda is contained in alum.

3. Pour into a faturated folution of alum, in fix times its quantity of water (heated to about 120° Farenh.) a faturated folution of acetate of lead. Sulphate of lead will be produced, which falls down ; and alfo, the acetate of alumine as well as the acetate of pot-afh (or of ammoniac), which, being readily foluble, remain diffolved in the water. The fupernatant fluid muft then be decanted from the fulphate of lead, and fubjected to evaporation, until it beomes turbid ; for by this means a little of the metallic fulphate, that was ftill held in folution by the water, is thrown down. When the fluid has been a fecond time filtered, it muft be evaporated to drynefs. The dry refiduum confifts of acetated argil and acetated pot-afh. And if it be then ignited, the acetic acid, together with the ammoniac, if prefent, will be volatilized. Water muft now be poured upon the refidue ; which, if only ammoniac has entered into that compound, extracts nothing from it, by reafon of the volatilization of this alkali, and of the infolubility of aluminous earth in water. But if a portion of vegetable alkali was contained in it, it is on account of its fixity, left behind in the argil, and will be diffolved in the water. The alkali receives fome carbonic acid from the deftruction of the acetic, but is ftill far from being faturated with it ; and hence a part of the argil will be diffolved along with it by the water. The earth muft, in that cafe, be precipitated, by faturating the folution with carbonic acid, or letting it ftand for a long time in open veffels, flightly covered.

Analytical experiments alone are, neverthelefs, infufficient to decide with certainty, whether all the three alkalis, or only one, or two, of them are by their acceffion capable of producing alum ; and therefore our arguments cannot yet reft on a complete induction. For, fuppofing I had decompofed artificial as well as native alum in a thoufand different ways, and had found no foda in it, I cannot fairly conclude that the mineral alkali is

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* I need not here obferve, that falts of more difficult folubility are, for the moft part, (though not perfectly) feparated by the gradual and fucceffive diminution of the water, and cryftallization.

unfit for the production of alum. Not to mention the few fpcies of alum, which I have examined, I fhould not choofe to draw fuch an inference from the refults I did obtain from them.

Synthetical experiments, on the contrary, I am perfuaded, ought to decide in this cafe. And by thefe, (although *Marggraf* mentions, in a general way only, the pot-afh as a neceffary requifite to the generation of fulphate of alumine, without excluding the foda) I have until the prefent moment been convinced, that the combination of foda with fulphuric acid and argil does *not* yield *any alum,* I mean that very falt, which is known by that name; but that it is generated by the addition of pot-afh.

I employed, in my experiments, native foda (natrum) from Hungary, purified in a high degree. It was this purity that induced me to inftitute thofe experiments. And to affure myfelf that no portion of pot-afh did interfere, I have precipitated the argil from Roman alum, not by means of pot-afh, but by foda. If, therefore, fomewhat of the neutral fulphate, generated by the precipitation, had remained in the earth after it was edulcorated, it muft neceffarily have been fulphate of foda.

The precipitated alumine was then properly wafhed; and I thought it needlefs to free it, according to *Richter,* from the adhering fulphuric acid, the prefence of which was not detrimental in this inftance. I then diffolved it in purified fulphuric acid, in excefs, fo that the folution reddened litmus-paper dipped in it. As I have already obferved, the due proportion of the earth to the acid is found of itfelf, by permitting the acid, moderately diluted, to diffolve as much of the earth as it can in a low temperature. When I afterwards added to the filtered folution fucceffive portions of diffolved carbonate of foda, till part of the alumine itfelf began to precipitate, I filtered the folution once more, and began to attempt the cryftallization of the falt, by gradually repeated evaporation and alternate cooling.

But in none of thefe inftances did I obtain alum; but rather a *very foluble falt,* altogether *different* in its *form.* This combination is of fo eafy folution, that I might at firft have repeatedly fufpected an accidental tranfpofition of my evaporating difhes, if I had not been fure of the contrary, by the fmall quantity of the refidual liquor, and its refufing to cryftallize. That great folubility agrees very well with that of Glauber's falt (fulphate of foda) as *vice verfâ,* the difficult folution of alum correfponds with that fulphate of pot-afh. When, in one of my experiments, I added juft fo much of foda, that the lixivium began to acquire a permanent cloudinefs (owing to the argillaceous particles firft precipitated, and which are rediffolved by the redundant acid, if the fluid be agitated, and thus the earthy particles be brought into contact * with its not yet alkalized part), there refulted from its farther evaporation and cooling fome minute, fingle, fhort needled cryftals, refembling thofe of gypfum. Thefe, however, merely contained aluminous, and no calcareous earth; for they readily diffolved in water; the oxalic acid precipitated nothing from their folution; pure

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* The earthy particles newly precipitated are, in general, more foluble, than thofe of a longer ftanding and deficcated; becaufe the former are fubtilely divided, and moiftened through the whole of their mafs.

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(cauftic)

(cauftic) ammoniac, on the contrary, threw down the argillaceous earth they contained. And when the lye was ftill farther evaporated, it coalefced upon refrigeration into a coherent faline mafs, in which, however, the needle-fhaped texture could be diftinguifhed. In another experiment, in which I added a little more of (the fame) foda in fuch proportion that the quantity of precipitated argil became fomewhat more confiderable, I obtained by evaporating the filtered lixivium to a fmall remainder, and as it cooled, longifh, flat, tabular fix-fided cryftals; (that is, very low hexagonal prifms, with broad ends and very low lateral facets, the two oppofite ones of which were longer, and the other four fhorter.) After the mother-water, which in proportion to the clofely grouped cryftals was but fmall in quantity, had been decanted, and the cryftals wafhed with a little cold water, I attempted to take them out of the veffel by means of a filver fpoon; but they were too foft, and were immediately compreffed. Cold water (at 60° Farenh.) eafily and quickly diffolved them; I have not yet determined the quantity required for that purpofe; but certainly no more than *three* parts are neceffary for *one* of the falt. The folution tafted like that of alum; it gave to paper, ftained with litmus, of a faint red; (the undiminifhed folution, before the fhooting of the cryftals, afforded a ftronger red); whence it follows, that there was a fmaller pro-portion of the acid in the cryftallized falt than in the whole fluid.

My attempts to produce alum by the acid of foda, being thus fruftrated, I doubted very much whether *ammoniac*, the properties of which is far lefs analogous to thofe of pot-afh, than the properties of foda, would ferve to produce alum, by combination with fulphuric acid and argillaceous earth. However, experience teaches that in this as well as in various other cafes, reafonings from analogy prove very often incorrect, when reforted to in our enquiries into nature. Precipitate the argil from an aqueous folution of Roman alum, by means of ammoniac, wafh the precipitate well; diffolve it then in pure fulphuric acid, in the manner pointed out above; and then continue adding ammoniac (either car-bonated or cauftic) by degrees, until the folution begins to affume a permanent turbidnefs. Let the whole then be reduced by evaporation fo much, that for *one* part of the diffolved falt (the quantity of which may be afcertained by the quantity of alum employed at the beginning of the procefs) there fhall remain from fix to eight parts of water. As this lye cools, a falt will cryftallize, which, with refpect to its octahedral form, (in the beft way to be perceived around the threads hung into the liquid) to the folidity and hardnefs of its cryftals, and to its difficult folubility, &c. perfectly agrees with the alum, which is formed by means of pot-afh *.

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* On this fubject fee this Journal, I. 318.

II.

Experiments on Whinstone and Lava.   By Sir JAMES HALL, *Bart. F. R. S. & F. A. S.*
Edin *.

(Concluded from page 18.*)*

THE suppositions which these gentlemen have thus advanced, and have seriously maintained in various parts of their works, have arisen in both from the belief, that, in our fires, nothing but glass can be produced from a lava after complete fusion.   This being taken for granted, it would certainly be very difficult to explain the phenomena of actual eruptions, by means of the known agents of nature.   Recourse has therefore been had, by one of these gentlemen, to a hypothetical modification of these agents; and by the other to the influence of substances, which have left behind them no trace of their existence †, and which, had they been present, could not have produced the effects ascribed to them.

According to both suppositions, the heat of volcanos is conceived to be of very little intensity; but the few observations I had occasion to make; which are confirmed by innumerable facts related by travellers, convince me that it must far exceed what is requisite for the most perfect fusion of the lavas, and of all the substances contained in them ‡ ; and the experiments already described supersede the necessity of supposing any thing different from the common course of nature; for they afford, analogically, an easy solution of the difficulty, by showing that glass is not the only result of fusion, and that whin, a substance like lava, when cooled slowly after fusion, resumes its stony character.   But, not content with analogy alone, I resolved to ascertain the truth of these conclusions in a direct manner, and performed the following experiments with specimens of six different lavas, four of which, to my certain knowledge, had made part of external volcanic currents.   In the present state of geology, too much pains cannot be bestowed in ascertaining that the specimens collected are really lavas, since this circumstance has been frequently overlooked, as I shall endeavour to shew, when I speak of the differences between them and whinstone.

* It ought to have been noticed in the last Number that this paper is inserted in the Edinburgh Transactions, though, by some culpable neglect, the copies of that work have not yet reached London.   N.

† None of the lavas I have seen contained the smallest vestige of petroleum; nor did I meet with any sulphur but what was evidently produced by the condensation of vapours, rising through crevices, long after the eruptions had ceased.

‡ I conceive, therefore, that the formation of the insulated substances contained in lavas, as well as the other peculiarities of internal structure, possessed by lavas in common with granite and basaltes, must be ascribed in all of them to crystallization during slow cooling after fusion, as I had stated formerly in Spring 1790, *(Transf. Edin. vol. III.)*   The year following, Dr. Beddoes presented to the Royal Society of London a paper, in which he also explains the character of granite and basaltes by crystallization, in consequence of slow cooling.

No.

No. 1. *Lava of Catania.*

This is the celebrated lava, which, in 1669, laid waſte great part of the town of Catania. The interior part of the current (accurately deſcribed by M. Dolomieu, *Iſles Ponces*, p. 256 *), from which the ſubjeſt of our experiment was taken, conſiſts of a light grey baſis, interſperſed with cryſtals of felſpar and of ſchorl, (augit). It bears a general reſemblance to the rock of the baſaltic columns on Arthur's Seat, and exhibited the ſame phenomena in our experiments. After ſtrong heat, the whole was reduced, by rapid cooling, to pure black glaſs; but when the heat applied was moderate, the felſpars remained unchanged. Being maintained, after a ſecond fuſion, in a temperature of 28, both theſe glaſſes yielded ſtony and cryſtallized ſubſtances, ſomewhat leſs fuſible than the original; and when expoſed to a temperature of 22, they cryſtallized rapidly, like moſt of the whins, into the liver cryſtallite. This laſt property is common to all the lavas.

No. 2. *Lava of Sᵗᵃ Venere.*

This current has flowed in the neighbourhood of a little chapel, called Sᵗᵃ Venere, above the village of Piedimonte, on the north ſide of Mount Ætna. Owing to the ſtrong reſemblance which it bears to ſtones ſuppoſed not volcanic, we took care that our ſpecimens ſhould be broken from the actual current; and to one of them, though moſtly compaſt, is attached a ſcorified maſs, which had made part of the external ſurface. The ſolid part is of a black, or rather dark blue, colour; very fine grained and homogeneous, having a multitude of minute and ſhining facettes viſible in the ſun; in this, and in other circumſtances, it greatly reſembles the rock of Edinburgh caſtle. This lava is the ſecond in M. Dolomieu's *Catalogue*, and is well deſcribed, p. 186 †.

The pure black glaſs formed from this lava yielded, in the regulated heat, the moſt highly cryſtallized maſs we obtained from any lava or whin.

No. 3. *Lava of La Motta di Catania.*

This is likewiſe compaſt and homogeneous, but for a number of ſmall yellow grains of chryſolite ſcattered through it, (deſcribed by M. Dolomieu, p. 191 ‡). It has been thrown up by a partial eruption burſting through the ſandſtone hills which ſurround Mount Ætna. The ſituation of this maſs is ſingular: it reſts upon a little hill, formed of looſe

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* " Elle eſt formée d'une pâte de roche de corne griſe, à grains fins, mêlée d'écailles, et de criſtaux de
" feld-ſpath de même couleur; elle contient un très grand nombre de criſtaux de ſchorl noir, et de grains de
" cryſolites jaunes, les uns et les autres quelquefois chatoyans, de différentes couleurs dans leurs fraſtures.
" . . . . . . Cette lave a une caſſure ſeche, et un grain rude, ſurtout dans le centre des courans; c'eſt là
" où elle a toujours conſervé une couleur plus claire, qui doit être celle de ſa baſe; ſur les bords et les ſur-
" faces elle s'eſt fort noircie; elle y a acquis une aſſez forte action ſur l'aiguille amantée que celle du centre
" n'a preſque point."

† " Lave homogene noire: ſon grain eſt fin et ſerré, il eſt un peu brillant, comme micacé lorſqu'on le
" préſente au ſoleil; ſa caſſure nette et ſeche eſt conchéide comme celle du ſilex."

‡ It belongs to the fifth variety of his compaſt lavas.

fcoria, the fummit and fides of which are covered by the ftony mafs, fo that no crater is vifible. It ftruck me on feeing it, and I found M. Dolomieu had formed the fame opinion, that the lava had rifen up in a perpendicular direction, and had flowed over on all fides. Its great thicknefs, and fmall extent, feem to favour a conjecture which this naturalift has formed with regard to feveral lavas, that they were erupted at the bottom of an ocean which once covered Sicily, and, being quickly cooled by the contact of water, had been prevented from flowing far. The conjecture feems plaufible enough *; and, having no proof that this fubftance made part of an external current, as I have with refpect to the firft two mentioned, I do not exhibit it as a lava with the fame confidence. Whatever be its hiftory, however, it poffeffes the chemical properties common to whin and lavas.

Its glafs yielded a dark grey cryftallite of uniform texture. Befide it in the drawer, now on the table, I have placed a cryftallite, formed from the whin No. 1. which refembles it in every refpect.

No. 4. *Lava of Iceland.*

I received the fpecimen from a perfon who found it on the fpot; but not being acquainted with the circumftances of its original pofition, I cannot be certain that it is a lava. It has however every appearance of being fuch.

It is a blue homogeneous fubftance, having fome chryfolites fcattered irregularly through it. Nearly half its bulk is occupied by large air holes, which do not appear to have contained any extraneous matter.

It produced a very fufible glafs, from which was formed a cryftallite much more refactory than the original.

No. 5. *Lava of Torre del Greco.*

This lava, which flowed from Vefuvius to the fea in the middle ages, has been an object of much attention, on account of its confpicuous bafaltic form. It confifts of a grey bafis, the fracture of which is coarfe and rough, and in which are embedded large and well characterized cryftals of fchorl, (augit), with a few chryfolites, (olivins).

It was found to be lefs fufible than any of the others, yet its glafs cryftallized in a lower temperature.

No. 6. *Lava of Vefuvius, eruption* 1785.

From the circumftances in which the above five lavas have been feen to cryftallize after fufion, it can fcarcely be doubted that the fame procefs takes place in a volcanic ftream, which in confequence of its bulk, muft cool with confiderable flownefs, and that a vitreous character would be affumed by the whole mafs, were it cooled with fufficient rapidity.

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* M. Dolomieu afcribes the formation of part of Mount Ætna itfelf to a fimilar caufe. I fhall have occafion, in another part of this paper, to confider that opinion.

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The truth of this laſt opinion is demonſtrated by ſome facts which I accidentally obſerved, long before my preſent views had occurred, when, in ſpring 1785, I had an opportunity of examining a ſtream of lava, which flowed from Veſuvius. The eruption was comparatively ſo gentle, that I was able, though not without inconvenience, to approach and examine the fiery ſtream on three different days. It was in general concealed by a thin white ſmoke, which the wind blew aſide occaſionally, ſo that I could diſtinctly ſee the lava as it burſt from the ſide of the hill. It was then of a bright white heat, and flowed with the agility and rapidity of water, in all reſpects reſembling melted iron running from the furnace. The liquid, at its firſt emergence, manifeſted a ſtrong effervescence, which ſubſiding as the heat abated, ſhewed itſelf at laſt only in the burſting of ſome very large bubbles, accompanied with a white ſmoke. Where I approached the ſtream, it was ſtill of a ſtrong red heat, and had the conſiſtence of honey. I thruſt a ſtick into it with eaſe, to the end of which ſome of the lava adhering, by its viſcidity, allowed itſelf to be drawn out into threads, and was found, when cold, to have a ſhining ſurface, and a vitreous fracture.

Being thus convinced that I had met with a lava of glaſs, I prepared ſome moulds of ſtucco, in which I meant to take caſts with that rare ſubſtance; and with this view returned to the mountain. I found the ſtream was not ſo liquid as at firſt, but I was able, by means of a ladle fixed on the end of a pole, to lift the ſpecimen now before us in a ſtate like dough. I then preſſed it with a ſeal, by which means, though too coarſe to receive an accurate impreſſion, it took the ſhape it now bears, which is that of the ladle. It is very porous, one-third of it nearly being occupied by air holes. It contains a great number of ſmall white cryſtals of Veſuvian garnet, embedded in a black ſubſtance, which completely reſembles the glaſs obtained in our experiments from lava by rapid cooling after fuſion. Beſides all their other properties, it poſſeſſes the fuſibility of the glaſſes, ſince it ſoftens completely at 18, that is, 14 or 15 degrees below the ſoftening point of any of the ſtony lavas. Being expoſed to the proceſs of regulated cooling, it gave the ſame reſult as all the other lava glaſſes. In the lower points it yielded a liver cryſtallite infuſible under 30, and in the higher a ſtony ſubſtance like a common lava or whin, and fuſible only at 35.

What has been ſaid is applicable to the interior parts of lavas; but I was at a loſs to underſtand the ſtate of their external ſurface, which, cooling much more rapidly, might be expected to poſſeſs a vitreous character; yet glaſs is not found on the ſurface of lavas, except in a very few caſes, and has occurred only in a ſingle ſpot on Ætna. This difficulty was removed, however, by the following conſideration: though the ſurface of a lava cools with far more rapidity than the reſt of the maſs, yet, owing to the contact of the fiery ſtream, that rapidity can never be very great; and we muſt ſuppoſe that the temperature of the ſurface employs more than a minute or two in deſcending from 23 to 21. Where this happens, we have ſhown that the ſubſtance conſolidates into the liver cryſtallite, which completely reſembles the ſcoria of a lava. A ſmall fragment of the maſs, which I

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took

took from the running ſtream, being placed in the temperature of 22, loſt its vitreous character in two minutes, as already ſtated; and had the maſs itſelf been allowed to remain but a very little longer in the ſtream, it would certainly have acquired, as well as the reſt of the ſurface, the dull character of ſcoria.

The ſame property accounts for the cruſt which is formed on the ſurface of flowing lavas, and which conſtitutes ſo remarkable a feature in their hiſtory. Were lava to congeal after the manner of pitch or wax, by an uniform and gradual increaſe of viſcidity throughout, no cruſt would be formed, or if, by the action of cold air, the upper ſurface were to harden a little, it might he ſoftened again by an influx of freſh matter a very little hotter than itſelf. In lavas, however, as we have proved, when the ſurface cools down to 21, it rapidly congeals to a hard ſubſtance, capable of reſiſting any heat under 30. The cruſt thus formed ſerves as a pipe, within which the flowing lava is confined. In ſeveral places on Ætna we meet with vaſt galleries, along which, and out of which, the lava has flowed, leaving the cruſt entire *.

The irregular manner in which a lava flows, when not extremely heated, may likewiſe be referred to the ſame cauſe. On the lower part of the running ſtream a cruſt is formed, ſo ſtrong as to retard its progreſs during a certain time, but the liquid behind, accumulating by degrees, at laſt acquires ſufficient ſtrength to force open the cruſt; the lava then flows out with rapidity, and continues its courſe till it is again retarded by the formation of a new cruſt.

Theſe experiments ſeem to eſtabliſh, in a direct manner, what I had deduced, analogically, from the properties of whinſtone, namely, that the ſtony character of a lava is fully accounted for by flow cooling after the moſt perfect fuſion; and, conſequently, that no argument againſt the intenſity of volcanic fire can be founded upon that character. We are therefore juſtified in believing, as numberleſs facts indicate, that volcanic heat has often been of exceſſive intenſity.

In the compariſon inſtituted between whin and lava, the two claſſes are found to agree ſo exactly in all their properties which we have examined, as to lead to a belief of their abſolute idenity. This idenity has been fully eſtabliſhed by Dr. Kennedy, who has performed an exact analyſis † of ſeveral of the very ſpecimens of whinſtone and lava mentioned in this paper; by which he diſcovered, that the elements of the two claſſes are the ſame: above all, that they both contain 4 or 5 *per cent.* of ſoda. Their agreement in this eſſential circumſtance ſeems to account for their common properties, whilſt the varieties of proportion, among their component elements correſpond to the ſlight differences of reſult we have obſerved between the individuals of the ſame claſs ‡.

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* As at Malpertui above Piedimonte.

† An account of Dr. Kennedy's analyſis is publiſhed in this volume. (Edin. Tranſ.)

‡ Though chemiſts have hitherto overlooked, in their experiments, the mode in which bodies were cooled after being reduced to a ſtate of fuſion; yet many reſults, which we are now entitled to aſcribe to flow cooling, have been occaſionally obſerved. The ſlag of a furnace bears a ſtrong reſemblance to what we
have

So close a resemblance affords a very strong presumption in favour of Dr. Hutton's system, according to which both classes are supposed to have flowed by the action of heat; but the circumstances under which they were exposed to this action being materially different, we have reason to look for indications of that difference. Such are not wanting.

Calcareous spar frequently occurs in whinstone, either in veins or in detached nodules, but is never found in lava, and could not exist in a volcanic stream; for heat, in such circumstances, would infallibly drive off the carbonic acid, and compel the lime to unite with the other component elements of the mass. In whinstone, which Dr. Hutton supposes to have flowed, at some remote period, in crevices of the earth, at a great depth below what was then its surface, the weight and strength of the superincumbent mass of strata * has been sufficient to resist the expansion of the carbonic acid, and to constrain it, upon the principle of Papins digester, to continue in combination with the lime. This compound

have called the liver crystallite, and is probably formed in the same manner. I have seen a mass possessing, in a great measure, the stony character of whins and lavas, which was produced in a lime-kiln by the fusion of an impure limestone; and Dr. Beddoes has observed a crystallized texture in the flags of some iron furnaces. I am informed, that the celebrated Mr. Klaproth has described some striking examples of crystallization after fusion, which he obtained in exposing various substances to the heat of the porcelain furnace at Berlin.

* It may be asked, what has become of this superincumbent mass; and by what means it has been removed. Dr. Hutton answers, that it has been gradually worn away during an immense course of ages, by the action of those causes which continue, under our eyes, to corrode the surface of the globe: that the solid parts, being conveyed to the bottom of the ocean, are there deposited in beds of sand and gravel, which, in some future revolution, being exposed to heat, may be again converted into stony strata.

The whole of this system appears to me well founded, except in what regards the removal of the superincumbent mass, which has been performed, I conceive, in a very different manner. I am inclined to agree on this point with M. Pallas, M. de Saussure, and M. Dolomieu, and to believe that, at some period very remote with respect to our histories, though subsequent to the induration of the mineral kingdom, the surface of the globe has been swept by vast torrents, flowing with great rapidity, and so deep as to overtop the mountains; that these torrents, by removing and undermining the strata in some places, and by forming in others immense deposits, have produced the broken and motley structure, which the loose and external part of our globe every where exhibits.

In the Alps and in Sicily I have witnessed several of those curious facts, upon which M. de Saussure and M. Dolomieu found their opinion, and which seem to justify their conclusions. I have likewise observed, in this country, many phenomena which denote the influence of similar agents. Lord Daar, who joins me in agreeing with Dr. Hutton in almost every article but this, has added great weight to the argument by some general observations on lakes, and by some very interesting facts which he has observed in the Highlands of Scotland. We propose to pursue this subject, and to lay the result of our inquiries before the society. Dr. Hutton, in the second volume of his *Theory of the Earth*, has taken great pains to refute all that has been said about these torrents; but, in my opinion, their existence is not only quite consistent with his general views, but seems deducible from his suppositions, almost as a necessary consequence. When the strata, according to his system, were elevated from the bottom of the sea, the removal of so much water, if not performed with unaccountable slowness, must have produced torrents, in all directions, of excessive magnitude, and fully adequate to the effects I have thus ascribed to them.

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seems to have entered readily into fusion, along with the whinstone, but to have kept separate from it, as oil separates from water through which it has been diffused, thus giving rise to the spherical form, which the nodules of calcareous spar generally exhibit with more or less regularity *.

This circumstance accounts for an appearance which has misled some of the early observers of our minerals. Many whinstone rocks externally resemble very porous lavas, but when broken are always found to be quite compact internally, and to contain numerous round nodules of calcareous spar. Near the surface, the nodules, being washed out by rain, have left the cavities which have given rise to this deception. The spherical form of the air holes in lavas, and of the nodules of calcareous spar in whins, seems to have been produced by a cause common to both, the mutual repulsion of two fluids intermixed, but not disposed to unite.

It must be owned, that this theory of calcareous spar is as yet hypothetical; but it is supported by strong analogy, and promises to be of service, by leading to decisive experiments and observations. I cannot help believing, that, by a careful examination of the volcanic countries, facts may yet be discovered which will throw light on this subject. In order to promote and direct such researches, I shall beg leave to state some observations which I made in those countries in 1785, before I was attached to any system of geology.

It is generally supposed, that some lavas of Ætna contain calcareous spar and zeolite; but this I conceive to be a mistake. It is true, as I have seen, that many rocks of Ætna contain these substances in abundance; but in my opinion these rocks are no lavas, but have flowed subterraneously like our whins, and are the same with them in every respect. A particular district of Ætna, comprehending the Cyclopian Isles, and the country round La Trezza, and the castle of Jaci, is decidedly of this description; and vestiges of the same kind occur in other parts of the mountain. In one place fossile coal has been found, and in another we saw marine shells. In the neighbourhood of Bronte we observed a high ridge formed of strata of sandstone and limestone, partly overflowed and concealed by recent lavas, but so placed as to render it evident that its continuation formed no inconsiderable part of the mountain. Thus, Ætna being composed, partly of the subterranean, and partly of the external productions of fire, may be expected to afford numberless opportunities of pursuing the comparison between these two classes †.

A most interesting scene for such a comparison occurs likewise on Vesuvius. The history of this volcano is simpler than that of Ætna, for it has been evidently formed, with all its appendages, by the continued action of external eruptions, which have raised it, at some remote period, from the bottom of a sea, occupying all the Campi Phlegræi, and washing the surrounding Appenines. The whole volcano seems once to have consisted of

* The modifications of the action of heat, occasioned by pressure, which have been taken into account by no geologist but Dr. Hutton, distinguish his theory from all other igneous theories.

† M. Dolomieu has observed this distinction; but supposes that the masses which we conceive to have flowed subterraneously were erupted at the bottom of the sea.

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a fingle large cone, the greateft part of which has funk during fome violent eruption, pro-
bably that which took place in the time of Pliny, leaving a fragment of its bafis, now called
the mountain of Somma. This fragment retains its original fhape; and on the fide front-
ing the towns of Somma and Otajano, the external conical furface, along which the an-
cient lavas had flowed, is ftill entire. Fronting the centre of the cone, Somma breaks off
abruptly, and prefents a vertical cragg, fome hundred feet in height, which is concave in-
wards. From the gulf, produced by the ruin of the ancient mountain, though not exactly
from its centre, have arifen the explofions which, by repeated accumulation, have formed
the prefent cone of Vefuvius. Next the fea, this cone has extended itfelf fo as completely
to cover all remains of the ancient one, forming a continued flope from the crater to the
foot of the mountain. On the oppofite fide it meets the bafe of the craggs of Somma, and
forms an angle, into which many fucceffive ftreams of lava have flowed, producing a nar-
row horizontal valley, in the form of a crefcent, called the *Atrio del Cavallo*. From this
valley the craggs of Somma prefent a complete view of the internal ftructure of the an-
cient mountain, correfponding, in moft things, to what might have been fuppofed.

The various fubftances, depofited fucceffively on the external furface of the ancient
cone, being cut vertically in this cragg, their fucceffion is diftinctly feen, the fection of
each ftratum prefenting to the view part of a horizontal circle; the whole confifts of alter-
nate layers of thin ftreams of lava, and very thick beds of loofe frothy *rapilli*, which laft
being thrown into the air in a foft ftate, had fallen in fhowers on the fides of the moun-
tain.

In various places the regularity of this arrangement is interrupted by certain vertical
lavas, from two feet to ten or twelve in thicknefs, which crofs the ftrata juft defcribed in
an irregular manner, and pafs upward, without diftinction, through the folid beds, and
through the loofe ones. It immediately occurred to us *, that thefe lavas muft have flowed
in fiffures of the ancient mountain; and we accounted for them by fuppofing, that a melted
ftream, flowing along the external furface, had met in its courfe with one of thofe cre-
vices which are formed in all great eruptions, and had flowed into it fo as to return again
into the heart of the mountain. This conjecture very nearly agrees with thofe advanced by
M. Dolomieu, and by M. Breiflack, who both mention thefe vertical lavas of Somma †.

I have fince been induced to confider this phenomenon, which formerly feemed to pre-
fent only an amufing variety in the hiftory of volcanic eruptions, as of the utmoft confe-
quence in geology, by fupplying an intermediate link between the external and the fubter-

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* I faw this place in company with Dr. J. Home in 1785.

† M. Dolomieu conceives thefe lavas to have flowed over the lips of the crater, *(Ifles Ponces, p.* 100.);
M. Breiflack, that they had firft filled the open cavity of the crater, and from thence had flowed into cre-
vices formed in its fides, " che una lava avendo riempita la cavita del cratere fi foffe infinuata per quefte
" fenditure," *(Topografia Fifica della Campania, p.* 115.) This laft mentioned work, publifhed in 1798,
contains many interefting and accurate defcriptions. Should the circumftances of the times permit, the
author will have it in his power to follow out, with every advantage, the hints I have fuggefted.

raneous productions of heat. I now think, that, though we judged rightly in believing thofe lavas to have flowed in crevices, we were miftaken as to their direction; for inftead of flowing downwards, I am convinced they have flowed upwards, and that the crevices have performed the office of pipes, through which lateral explofions have found a vent. This will appear in the higheft degree probable, when we attend to the known hiftory of volcanic eruptions. It generally happens, that the lava begins to flow from the fummit, in confequence of the crater being filled with liquid matter up to the brim. At that moment the bafis of the mountain muft be preffed outwards by a very great hydroftatical force, equal to the weight of a column of liquid lava as high as the mountain itfelf. It is natural then to expect, that this preffure, affifted by ftrong percuffions of explofion, fhould lacerate the body of the hill, and form great rents. The lava, urged upwards by the fame preffure, would flow through thefe rents, and emerge at the furface with violence. The difcharge would continue through this channel till the propelling force had ceafed, when the rents would be left full of lava; which, cooling in that pofition, would produce vertical lavas, fuch as thofe of Somma. This fuppofition is confirmed by various phenomena: the lava ceafes to flow from the crater as foon as a lateral eruption has begun; when it rufhes with fuch violence from the fide of the mountain as to fly to a great height into the air, like a *jet d'eau;* and it often makes its appearance, in the fame inftant, at various mouths, which are not fcattered at random, but placed in one continued line, indicating the difcharge from a rent. Some circumftances likewife, which I obferved on a clofe examination of the vertical lavas, indicate that the crevices had performed the office of pipes. Frequently the fubftance at the middle differs from that on each fide, whilft the fides refemble each other exactly. I explain this, by fuppofing that the lava, which had firft flowed through the pipe, and had coated its fides with folid matter, had been followed by a ftream fomewhat different, which had remained there on the cooling of the whole. In one cafe, I found lava on each fide, and in the middle tuffa, which is generally fuppofed to have been erupted in the ftate of watery mud. In another, the fubftance in immediate contact with the mountain is vitreous, the reft being common lava. This is fully explained by our experiments, if we fuppofe the ftream to have flowed into a cold crevice.

To apply thefe obfervations to the general hiftory of the globe: it is evident that the vertical lavas bear the clofeft refemblance, in point of pofition, to veins of every defcription, which, in all parts of the world, are found penetrating the ftrata, and which, according to Dr. Hutton, have flowed by means of fubterraneous heat. The veins, or dikes, (as they are called), of whinftone, which fo commonly occur in this country, differ from them in no circumftance which I had the means of obferving. It is therefore natural to expect that, if examined with particular care, their agreement will be found complete. Of this, however, we muft not form too fanguine expectations; for though the vertical lavas of Somma have undoubtedly fuftained the preffure of a great fuperincumbent mafs, we have no proof that this force was fufficiently ftrong, as Dr. Hutton fuppofes was the cafe in whinftone, to reprefs the volatility of carbonic acid. On the other hand, as we are

yet

yet entirely ignorant of the degree of force requisite for this purpose, we have no proof that it has been too weak. All the veins of Somma I examined were absolutely compact, except one which was full of pores. I am unable to determine whether this was the real porosity of a lava, or whether, as in our whins, it arose from the removal, near the surface, of nodules of calcareous spar. Granting that these pores were real air holes, the circumstance was peculiar to that single stream, and may have been owing to an inferior degree of pressure; for, in this respect, we have reason to look for the greatest diversity. Some vertical streams must have flowed while the mountain was yet low; others may have found vent at a low level; in both which cases, the pressure would be feeble: whereas other streams, communicating with elevated lateral eruptions, would sustain, in their lower parts, the full re-action of deep columns of liquid lava, and may be expected to exhibit the effects of great pressure. Should any future traveller be fortunate enough to meet with a nodule of calcareous spar in a lava, occupying the crevice of a mountain formed by undoubted external eruptions, all that has been said of the effects of pressure would cease to be hypothetical, and this fundamental article of Dr. Hutton's theory would be established beyond dispute.

I have now examined the relation between whinstone and lava in various points of view; and the result of the investigation, by showing the intimate connection between the two classes, tends strongly to confirm the ideas of Dr. Hutton. I flatter myself, likewise, that the experiments, independently of the general views of geology, are of some value, by accounting for the stony character of lavas, and thus enabling us to dispense with the various mystical suppositions which have of late perplexed the history of volcanic phenomena.

TABLE OF FUSIBILITIES.

	Substances.	Original softened.	Glass softened.	Crystallite softened.
No. 1.	Whin of Bell's Mills Quarry - - - - -	40	15	32
2.	Whin of Castle Rock - - - -	45	22	35
3.	Whin of Basaltic Column, Arthur's Seat - - -	55	18	35
4.	Whin near Duddingstone Loch - - -	43	24	38
5.	Whin of Salisbury Craig - - - - -	55	24	38
6.	Whin from the Water of Leith - - -	55	16	37
7.	Whin of Staffa - - - - - -	38	14½	35
No. 1.	Lava of Catania - - - - -	33	18	38
2.	Lava of Sta Venere, Piedimonte - - -	32	18	36
3.	Lava of La Motta - - - - -	36	18	36
4.	Lava of Iceland - - - - - -	35	15	43
5.	Lava of Torre del Greco - - - -	40	18	28
6.	Lava of Vesuvius, 1785 - - - -	18	18	35

III.

A Memoir on the Difcovery of certain Bones of a Quadruped of the Clawed Kind in the Wef-
tern Parts of Virginia. By THOMAS JEFFERSON, *Efq.*

(Concluded from page 43.)

THESE bones only enable us to clafs the animal with the unquiculated quadrupeds;
and of thefe the lion being neareft to him in fize, we will compare him with that animal,
of whofe anatomy Monfieur Daubenton has furnifhed very accurate meafures in his tables
at the end of Buffon's Natural Hiftory of the Lion. Thefe meafures were taken, as he [*]
informs us, from " a large lion of Africa," in which quarter the largeft [†] are faid to be
produced. I fhall felect from his meafures only thofe where we have the correfponding
bones, converting them into our own inch and its fractions, that the comparifon may be
more obvious: and to avoid the embarraffment of defignating our animal always by cir-
cumlocution and defcription, I will venture to refer to him by the name of the Great-Claw
or Megalonyx, to which he feems fufficiently entitled by the diftinguifhed fize of that
member.

	Megalonyx. Inches.	Lion. Inches.
Length of the ulna, or fore-arm	20.1	13.7
Height of the olecranum	3.5	1.85
Breadth of the ulna, from the point of the coronoide apophyfis to the extremity of the olecranum	9.55	
Breadth of the ulna at its middle	3.8	
Thicknefs at the fame place	1.14	
Circumference at the fame place	6.7	
Length of the radius	17.75	12.37
Breadth of the radius at its head	2.65	1.38
Circumference at its middle	7.4	3.62
Breadth at its lower extremity	4.05	1.18
Diameter of the lower extremity of the femur at the bafe of the two condyles	4.2	2.65
Tranfverfe diameter of the larger condyle at its bafe	3	
Circumference of both condyles at their bafe	11.65	
Diameter of the middle of the femur	4.25	1.15
Hollow of the femur at the fame place	1.25	
Thicknefs of the bone furrounding the hollow	1.5	
Length of the longeft claw	7.5	1.41
Length of the fecond phalanx of the fame	3.2	1.41

[*] Buffon, XVIII. 38. Paris edition in 31 vols. 12mo.
[†] 2. De Manet, 117.

The

The dimenfions of the largeft of the foot bones are as follow:

	Inches.
Its greateft diameter, or breadth at the joint - - - -	2.45
Its fmalleft diameter, or thicknefs at the fame place - - -	2.28
Its circumference at the fame place - - - - -	7.1
Its circumference at the middle - - - - - -	5.3

	Of longeft toe.	Middle fized toe.	Shorteft toe.
2d. Phalanx. Its length - - -	3.2	2.95	
Greateft diameter at its head or upper joint -	1.84	2.05	
Smalleft diameter at the fame place - -	1.4	1.54	
Circumference at the fame place - -	5.25	5.8	
3d. Phalanx. Its length - - -	*7.5	†5.9	3.5
Greateft diameter at its head or upper joint -	2.7	2.	1.45
Smalleft diameter at the fame place -	.95	.9	.55
Circumference at the fame place - -	6.45	4.8	

Were we to eftimate the fize of our animal by a comparifon with that of the lion on the principle of *ex pede Herculem,* by taking the longeft claw of each as the module of their meafure, it would give us as a being out of the limits of nature. It is fortunate therefore that we have fome of the larger bones of the limbs which may furnifh a more certain eftimate of his ftature. Let us fuppofe then that his dimenfions of height, length and thicknefs, and of the principal members compofing thefe, were of the fame proportions with thofe of the lion. In the table of M. Daubenton an ulna of 13.78 inches belonged to a lion 42½ inches high over the fhoulders: then an ulna of 20.1 inches befpeaks a megalonyx of 5 feet 1.75 inches height, and as animals who have the fame proportions of height, length, and thicknefs have their bulk or weights proportioned to the cubes ‡ of any one of their dimenfions, the cube of 42.5 inches is to 262 lb. the height and weight of M. Daubenton's lion as the cube of 61.75 inches to 803 lb. the height and weight of the magalonyx; which would prove him a little more than three times the fize of the lion, I fuppofe that we fhould be fafe in confidering, on the authority of M. Daubenton, his lion as a large one. But let it pafs as one only of the ordinary fize, and that the megalonyx, whofe bones happen to have been found, was alfo of the ordinary fize. It does § appear that there was diffected for the academy of fciences at Paris, a lion of 4 feet 9⅜ inches height. This individual would weigh 644 lb. and would be in his fpecies, what a man of eight feet height would be in ours. Such men have exifted. A megalonyx equally monftrous would be 7 feet high, and would weigh 2000 lb. but the ordinary race, and not the monfters of it, are the object of our prefent enquiry.

* It is actually 6¾ inches long, but about ¼ inch appear to have been broken off.

† Actually 5.65 but about ¼ inch is broken off.

‡ Buffon xxii. 121.   § Buffon xviii. 15.

I have

I have ufed the height alone of this animal to deduce his bulk, on the fuppofition that he might have been formed in the proportions of the lion. But thefe were not his proportions, he was much thicker than the lion in proportion to his height, in his limbs certainly, and probably therefore in his body. The diameter of his radius, at its upper end, is near twice as great as that of the lion, and, at its lower end, more than thrice as great, which gives a mean proportion of $2\frac{1}{2}$ for 1. The femur of the lion was lefs than $1\frac{1}{4}$ inch diameter. That of the megalonyx is $4\frac{1}{4}$ inches, which is more than three for one. And as bodies of the fame length and fubftance have their weights proportioned to the fquares of their diameters, this excefs of caliber compounded with the height, would greatly aggravate the bulk of this animal. But when our fubject has already carried us beyond the limits of nature hitherto known, it is fafeft to ftop at the moft moderate conclufions, and not to follow appearances through all the conjectures they would furnifh, but leave thefe to be corroborated or corrected by future difcoveries. Let us only fay then, what we may fafely fay, that he was *more* than three times as large as the lion: that he ftood as preeminently at the head of the column of clawed animals as the mammoth ftood at that of the elephant, rhinoceros, and hippopotamus: and that he may have been as formidable an antagonift to the mammoth as the lion to the elephant.

A difficult queftion now prefents itfelf. What is become of the great-claw? Some light may be thrown on this by afking another queftion. Do the wild animals of the firft magnitude in any inftance fix their dwellings in a thickly inhabited country? fuch, I mean, as the elephant, the rhinoceros, the lion, the tyger? as far as my reading and recollection ferve me, I think they do not: but I hazard the opinion doubtingly, becaufe it is not the refult of full enquiry. Africa is chiefly inhabited along the margin of its feas and rivers. The interior defart is the domain of the elephant, the rhinoceros, the lion, the tyger. Such individuals as have their haunts neareft the inhabited frontier, enter it occafionally, and commit depredations when preffed by hunger: but the mafs of their nation (if I may ufe the term) never approach the habitation of man, nor are within reach of it. When our anceftors arrived here, the Indian population, below the falls of the rivers, was about the twentieth part of what it now is. In this ftate of things, an animal refembling the lion feems to have been known even in the lower country. Moft of the accounts given by the earlier adventurers to this part of America make a lion one of the animals of our forefts. Sir John Hawkins * mentions this in 1564. Thomas Harriot, a man of learning, and of diftinguifhed candor, who refided in Virginia in 1587 † does the fame, fo alfo does Bullock in his account of Virginia,‡ written about 1627, he fays he drew his information from Pierce, Willoughby, Claiborne, and others who had been here, and from his own father who had lived here twelve years. It does not appear whether the fact is ftated on their own view, or on information from the Indians, probably the latter. The progrefs of

* Hakluyt, 541. edition of 1589.
† Ibid. 757, and Smith's Hiftory of Virginia, 10.
‡ Bullock, page 5.

the

the new population would foon drive off the larger animals, and the largeft firft. In the prefent interior of our continent there is furely fpace and range enough for elephants and lions, if in that climate they could fubfift; and for mammoths and megalonyxes who may fubfift there. Our entire ignorance of the immenfe country to the weft and north-weft, and of its contents, does not authorife us to fay what it does not contain.

Moreover it is a fact well known, and always fufceptible of verification, that on a rock on the bank of the Kanhawa, near its confluence with the Ohio, there are carvings of many animals of that country, and among thefe one which has always been confidered as a perfect figure of a lion. And thefe are fo rudely done as to leave no room to fufpect a foreign hand. This could not have been of the fmaller and manelefs lion of Mexico and Peru, known alfo in Africa both in * ancient and † modern times, though denied by ‡ M. de Buffon: becaufe like the greater African lion, he is a tropical animal; and his want of a mane would not fatisfy the figure. This figure then muft have been taken from fome other prototype, and that prototype muft have refembled the lion fufficiently to fatisfy the figure, and was probably the animal the defcription of which by the Indians made Hawkins, Harriot, and others conclude there were lions here. May we not prefume that prototype to have been the great-claw?

Many traditions are in poffeffion of our upper inhabitants, which themfelves have heretofore confidered as fables, but which have regained credit fince the difcovery of thefe bones. There has always been a ftory current that the firft company of adventurers who went to feek an eftablifhment in the county of Greenbriar, the night of their arrival were alarmed at their camp by the terrible roarings of fome animal unknown to them: that he went round and round their camp, that at times they faw his eyes like two balls of fire, that their horfes were fo agonifed with fear that they couched down on the earth, and their dogs crept in among them, not daring to bark. Their fires, it was thought, protected them, and the next morning they abandoned the country. This was little more than 30 years ago.—In the year 1765, George Wilfon and John Davies, having gone to hunt on Cheat river, a branch of the Monongahela, heard one night, at a diftance from their camp, a tremendous roaring, which became louder and louder as it approached, till they thought it refembled thunder, and even made the earth tremble under them. The animal prowled round their camp a confiderable time, during which their dogs, though on all other occafions fierce, crept to their feet, could not be excited from their camp, nor even encouraged to bark. About day-light they heard the fame found repeated from the knob of a mountain about a mile off, and within a minute it was anfwered by a fimilar voice from a neighbouring knob. Colonel John Stewart had this account from Wilfon in the year 1769, who was afterwards Lieutenant Colonel of a Pennfylvania regiment in the revolution-war; and fome years after from Davies, who is now living in Kentucky.

* Ariftot. Animal, 9. 4. Pliny, 8. 16.   † Kolbe.   ‡ Buffon, xviii. 18.

Thefe

Thefe circumftances multiply the points of refemblance between this animal and the lion. M. de la Harpe of the French Academy, in his abridgment of the General Hiftory of Voyages, fpeaking of the Moors, fays *, " it is remarkable that when, during their huntings, they meet with lions, their horfes, though famous for fwiftnefs, are feized with fuch terror that they become motionlefs, and their dogs equally frightened, creep to the feet of their mafter, or of his horfe." Mr. Sparrman in his voyage to the Cape of Good Hope, chap. 11. fays, " we could plainly difcover by our animals when the lions, whether they roared or not, were obferving us at a fmall diftance. For in that cafe the hounds did not venture to bark, but crept quite clofe to the Hottentots ; and our oxen and horfes fighed deeply, frequently hanging back, and pulling flowly with all their might at the ftrong ftraps with which they were tied to the waggon. They alfo laid themfelves down on the ground, and ftood up alternately, as if they did not know what to do with themfelves, and even as if they were in the agonies of death." He adds that " when the lion roars, he puts his mouth to the ground, fo that the found is equally diffufed to every quarter." M. de Buffon (xviii. 31.) defcribes the roaring of the lion as, by its echoes refembling thunder: and Sparrman c. 12. mentions that the eyes of the lion can be feen a confiderable diftance in the dark, and that the Hottentots watch for his eyes for their government. The phofphoric appearance of the eye in the dark feems common to all animals of the cat kind.

The terror excited by thefe animals is not confined to brutes alone. A perfon of the name of Draper had gone in the year 1770, to hunt on the Kanhawa. He had turned his horfe loofe with a bell on, and had not yet gone out of hearing when his attention was recalled by the rapid ringing of the bell. Sufpecting that Indians might be attempting to take off his horfe, he immediately returned to him, but before he arrived he was half eaten up. His dog fcenting the trace of a wild beaft, he followed him on it, and foon came in fight of an animal of fuch enormous fize, that though one of our moft daring hunters and beft markfmen, he withdrew inftantly, and as filently as poffible, checking and bringing off his dog. He could recollect no more of the animal than his terrific bulk, and that his general outlines were thofe of the cat kind. He was familiar with our animal mifcalled the panther, with our wolves and wild beafts generally, and would not have miftaken nor fhrunk from them.

In fine, the bones exift : therefore the animal has exifted. The movements of nature are in a never ending circle. The animal fpecies which has once been put into a train of motion, is ftill probably moving in that train. For if one link in nature's chain might be loft, another and another might be loft, till this whole fyftem of things fhould evanifh by piece-meal; a conclufion not warranted by the local difappearance of one or two fpecies of animals, and oppofed by the thoufands and thoufands of inftances of the renovating power conftantly exercifed by nature for the reproduction of all her fubjects, animal, ve-

* Gentleman's, and London Magazines, for 1783.

getable,

getable, and mineral. If this animal then has once exifted, it is probable on this general view of the movements on nature that he ftill exifts, and rendered ftill more probable by the relations of honeft men applicable to him and to him alone. It would indeed be but conformable to the ordinary economy of nature to conjecture that fhe had oppofed fuffi-cient barriers to the too great multiplication of fo powerful a deftroyer. If lions and tygers multiplied as rabbits do, or eagles as pigeons, all other animal nature would have been long ago deftroyed, and themfelves would have ultimately extinguifhed after eating out their pafture. It is probable then that the great-claw has at all times been the rareft of animals. Hence fo little is known, and fo little remains of him. His exiftence however being at length difcovered, enquiry will be excited, and further information of him will probably be obtained.

The Cofmogony of M. de Buffon fuppofes that the earth, and all the other planets pri-mary and fecondary, have been maffes of melted matter ftruck off from the fun by the in-cidence of a comet on it : that thefe have been cooling by degrees, firft at the poles, and afterwards more and more towards their. equators: confequently that on our earth there has been a time when the temperature of the poles fuited the conftitution of the elephant, the rhinoceros, and hippopotamus ; and in proportion as the remoter zones became fuc-ceffively too cold, thefe animals have retired more and more towards the equatorial regions, till now that they are reduced to the torrid zone as the ultimate ftage of their exiftence. To fupport this theory, he [*] affumes the tufks of the mammoth to have been thofe of an elephant, fome of his teeth to have belonged to the hippopotamus, and his largeft grinders. to an animal much greater than either, and to have been depofited on the Miffouri, the Ohio, the Holfton, when thofe latitudes were not yet too cold for the conftitutions of thefe animals. Should the bones of our animal, which may hereafter be found, differ only in fize from thofe of the lion, they may on this hypothefis be claimed for the lion, now alfo reduced to the torrid zone, and its vicinities, and may be confidered as an additional proof of this fyftem ; and that there has been a time when our latitudes fuited the lion as well as the other animals of that temperament. This is not the place to difcufs theories of the earth, nor to queftion the gratuitous allotment to different animals of teeth not differing in any circumftance. But let us for a moment grant this with his former poftulata, and afk how they will confift with another theory of his " qu'il y a dans la combinaifon des elemens et des autres caufes phyfiques, quelque chofe de contraire a l'aggrandifement de la nature. vivante *dans ce nouveau monde* ; qu'il y a des obftacles au developpement et peutetre a la formation des grands germes [†]." He fays that the mammoth was an elephant, yet [‡] two or three times as large as the elephants of Afia and Africa : that fome of his teeth were thofe of a hippopotamus, yet of a hippopotamus [§] four times as large as thofe of Africa : that the mammoth himfelf, for he ftill confiders him as a diftinct animal [||], " was of a fize

[*] Buffon, Epoq. 2. 233, 234.   [†] Buffon, xviii. 145.   [‡] 2. Epoq. 223.
[§] 1. Epoq. 246. 2. Epoq. 232.   [||] 2 Epoq. 234, 235.

superior to that of the largest elephants. That he was the primary and greatest of all terrestrial animals." If the bones of the megalonyx be ascribed to the lion, they must certainly have been of a lion of more than three times the volume of the African. I delivered to M. de Buffon the skeleton of our palmated elk, called orignal o r moose, feet high over the shoulders, he is often considerably higher. I cannot find that the European elk is more than two-thirds of that height: consequently not one-third of the bulk of the American. He * acknowledges the palmated deer (daim) of America to be larger and stronger than that of the Old World. He † considers the round horned deer of these States and of Louisiana as the roe, and admits they are of three times his size. Are we then from all this to draw a conclusion, the reverse of that of M. de Buffon. That nature, has formed the larger animals of America, like its lakes, its rivers, and mountains, on a greater and prouder scale than in the other hemisphere? Not at all, we are to conclude that she has formed some things large and some things small, on both sides of the earth, for reasons which she has not enabled us to penetrate; and that we ought not to shut our eyes upon one half of her facts, and build systems on the other half.

To return to our great-claw; I deposit his bones with the Philosophical Society, as well in evidence of their existence and of their dimensions, as for their safe-keeping; and I shall think it my duty to do the same by such others as I may be fortunate enough to obtain the recovery of hereafter.

<div style="text-align:right">TH: JEFFERSON.</div>

Monticello. Feb. 10*th*, 1797.

P. S. *March* 10*th*, 1797. After the preceding communication was ready to be delivered in to the Society, in a ‡ periodical publication from London I met with an account and drawing of the skeleton of an animal dug up near the river La Plata in Paraguay, and now mounted in the cabinet of Natural History of Madrid. The figure is not so done as to be relied on, and the account is only an abstract from that of Cuvier and Roume. This skeleton is also of the clawed-kind, and having only four teeth on each side above and below, all grinders, is in this account classed in the family of unquiculated quadrupeds destitute of cutting teeth, and receives the new denomination of megatherium, having nothing of our animal but the leg and foot bones, we have few points for a comparison between them. They resemble in their stature, that being 12 feet 9 inches long, and 6 feet 4½ inches high, and ours by computation 5 feet 1.75 inches high: they are alike in the colossal thickness of the thigh and leg bones also. They resemble too in having claws: but those of the figure appear very small, and the verbal description does not satisfy us whether the claw-bone, or only its horny cover be large. They agree too in the circumstance of the two bones of the fore-arm being distinct and moveable on each other; which however is believed to be so usual as to form no mark of distinction. They differ in the

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* Buffon, xxix. 245.   † Ibid, xii. 91, 92, xxix. 245. Vide Suppl. 201.
‡ Monthly Magazine, Sep. 1796.

<div style="text-align:right">following</div>

following circumſtances, if our relations are to be truſted. The megatherium is not of the cat form, as are the lion, tyger, and panther, but is ſaid to have ſtriking relations in all parts of its body with the bradypus, daſypus, pangolin, &c. According to analogy then, it probably was not carnivorous, had not the phoſphoric eye, nor leonine roar. But to ſolve ſatisfactorily the queſtion of identity, the diſcovery of fore-teeth, or of a jaw bone ſhewing it had, or had not, ſuch teeth, muſt be waited for, and hoped with patience. It may be better, in the mean time, to keep up the difference of name.

IV.

A Letter from Mr. John Heckewelder, to Dr. Barton, giving ſome account of the remarkable Inſtinct of a Bird called the Nine-Killer*.

Bethlehem, December 18th, 1795.

HAVING an opportunity by a friend of mine to Philadelphia, I muſt mention to you a curious fact, that came to my knowledge but yeſterday.

I went to a farm, about eleven miles and a half from this place, to view a young orchard, which had been planted, about five weeks ago, under my direction, where on viewing the trees, I found, to my great aſtoniſhment, almoſt on every one of them, one and on ſome two and three graſshoppers, ſtuck down on the ſharp thorny branches, which were not pruned when the trees were planted. I immediately called the tenant, and aſked the reaſon and his opinion of this. He was much ſurpriſed at my ignorance about the matter, and informed me, " that theſe graſshoppers were ſtuck up by a ſmall bird of prey, which the Germans called *Neun-toedter* (in Engliſh, Nine-killer); that this bird had a practice of catching and ſticking up nine graſshoppers a day, and that as he well knew they did not devour the graſshoppers, nor any other inſects, he thought they muſt do it for pleaſure. I aſked him for a deſcription of this bird, and was perfectly ſatisfied that it lived entirely on ſmall animals, ſuch as ſmall birds, mice, &c. for I had paid attention to this bird as early as the year 1761, when, in the winter, one of the ſame ſpecies took a favourite little bird out of my cage at the window, from which time I have watched them more cloſely, and have found them more numerous in the weſtern-country than here. Not being ſatisfied with what the tenant had told me reſpecting the intention of the bird's doing all this (viz. for diverſion ſake), and particularly obſerving each and every one of theſe graſshoppers ſtuck up ſo regularly, and in their natural poſition as when on the ground, not one of them having its back downwards, I began to conjecture what might be the real intention which the bird had in this, and my determined opinion was, that this little bird-

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* Addreſſed to Dr. Benj. Smith Barton, and inſerted in the Tranſactions of the American Philoſ. Society, IV. 124:

hawk,

hawk, by inſtinct, made uſe of this art, in order to decoy the ſmaller birds, which feed on inſects, and by theſe means have a fair opportunity of catching them. All this I communicated to my friends, on my return home, and they were not leſs aſtoniſhed at what I had related to them, than I had been on diſcovering the fact. It being agreed that one or more gentlemen of learning and obſervation ſhould more minutely examine into this matter, the proprietor of this farm, with another gentleman and myſelf, went this day out for the purpoſe, and viewing the graſshoppers on a number of theſe ſmall trees (ſome of which we cut off, and took home), we returned to the tenant, who not only himſelf but alſo his father and ſiſter gave us the beſt aſſurances, that they had, long ſince, and from time to time, obſerved this bird catching graſshoppers and ſticking them up in the manner already related, and that ſometimes they had obſerved, in places where this ſpecies of bird keeps, numbers of graſshoppers ſtuck up on a thorn-buſh in like manner. The Reverend Mr. V. Vleck is perfectly ſatisfied that this bird-hawk is the Lanius Canadenſis (in Bartram *), and has obligingly communicated the following account of this little bird-hawk to me : it is extracted from a German publication printed at Goettingen, in 1778, under the title of " Natural Hiſtory for Children, by M. George Chriſtian Paff," who after giving a deſcription of the different ſpecies of this bird, concludes thus : " Why is this bird of prey called the nine-killer ? Becauſe it is ſaid to have the habit of ſticking beetles or other inſects, and perhaps ſometimes nine of them in ſucceſſion, upon thorns, that they may not eſcape until he has leiſure to devour them all at once. And for the ſame reaſon, it is ſometimes called the thorn-ſticker." Now by the above account, we ſee that it is known in Europe that this ſame ſpecies of birds actually does ſtick up inſects of different kinds on thorns, &c. but it is ſuppoſed they eat them immediately after being ſtuck up. Here the caſe is quite otherwiſe. They remain ſtuck up, for we muſt ſuppoſe theſe to have been ſtuck up at leaſt ſome weeks ago, and before the hard froſts ſet in. The very birds (as we ſuppoſe) that ſtuck them up are now on the ſame ground, watching the ſmaller birds that come out to feed, and have been ſeen catching the latter but a few days ago. If it were true, that this little hawk had ſtuck them up for himſelf; how long would he be feeding on one or two hundred graſshoppers ? But if it be intended to ſeduce the ſmaller birds to feed on theſe inſects, in order to have an opportunity of catching them, that number, or even one half, or leſs, may be a good bait all winter : and all of us, who have conſidered theſe circumſtances, are firmly of opinion, that theſe inſects thus ſtuck up, are to ſerve as a bait, &c. through the courſe of the winter.

You will readily excuſe my being ſo lengthy on this ſubject. The matter appeared to me of too much conſequence to paſs over haſtily. I ſhall be glad to hear your opinion on this ſubject.

* I do not find that Mr. Bartram has mentioned, in any part of his _Travels_, a Lanius Canadenſis. Since the date of this letter, Mr. Heckewelder has favoured me with a well-preſerved ſpecimen of the bird-hawk. It proves to be the Lanius Excubitor of Linnæus, the great-ſhrike of Mr. Pennant. B. S. B.

I ſend

I fend you a few of thefe grafshoppers, as I cut them from the trees. They being hard and dry, moft of their legs broke off in taking them home.

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I am, with great refpect,

Dear Sir,

Your moft obedient and

Humble Servant,

JOHN HECKEWELDER.

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V.

Letter from Dr. Beddoes on the Experiments made at the Royal Inftitution with the Nitrous Oxide.

To Mr. NICHOLSON.

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SIR,

<div align="right">

Clifton, 17*th April.*

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THOUGH I muft be too well aware of the caufes of variation in medical and phyfiological teftimony, to be much moved by groundlefs contradiction, yet I own I did expect that competent experimenters would nearly agree in their reports of the effect, produced by refpiring the *nitrous oxide.*

A few days ago, however, I was well informed, that on trial at a refpectable inftitution in London, the gas had fallen far fhort of what Mr. Davy and myfelf had taught the public to expect. The gentleman who brought this intelligence being at Clifton, it was eafy to give him perfonal proof of our fidelity.

Yefterday my friend Mr. William Reynolds, of Ketley, in Shropfhire, affured me that he had himfelf refpired the gas at the *inftitution* in London to no purpofe; and that moft others had done the fame. The agency of the nitrous oxide is much too diftinct and certain to leave a doubt, but that there muft be fome ftrange miftake here. On feeing a paralytic patient take it, Mr. W. R. was at once aware of the reafon (at leaft, of one fufficient reafon) for the failures in town, and on refpiring a very fmall quantity indeed, he felt effects exactly the fame in kind, as the agreeable ones defcribed in my *notice.* He had previoufly inhaled atmofpheric air without knowing it to be fuch. He expatiated with fatisfaction on the fenfations *now* excited by the nitrous oxide, and faid he fhould like another dofe. On being afked, if he had felt any fuch defire after the experiment in town, he anfwered, *no not the leaft.* We are conftantly accuftomed to the eager expreffion of this defire.

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In our ample experience, we have not found the experiment to fail above once in twenty times. With a given degree of accuracy and perseverance, it would, I apprehend, *never* fail. I do not mean that the result would be always agreeable.

The gas being procured genuine, the lungs should be emptied by a strong expiration, the nose held tight, and the lips exactly closed on the mouth-piece. From 5 to 8 quarts breathed backwards and forwards into the reservoir (a proceeding of which Mr. Davy's researches will shew the propriety) have proved an average dose. The feelings will determine the time. To the hysterical and the exquisitely sensible, the gas should not be administered.

It is very well to know from actual trial, what the London experiments prove, that interrupted respirations, during which the gas is probably much diluted with residuary air in the lungs, can seldom have effect. This, however, an observer of any sagacity could have predicted.

A laborious analysis, and the result of a vast variety of careful observations, will ere long appear, to correct the misconceptions which may have arisen from loose casual trials. Meanwhile, I beg you to insert this explanation in your valuable Journal. I think it of no small importance to mankind, that there should exist nothing to prevent this great agent from being employed, whenever direct experience or circumspect analogy warrants its use.

I am, Sir,

Respectfully your's,

THOMAS BEDDOES.

VI.

On the colouring Matter of Dog's Mercury (Mercurialis perennis Lin.) By a Correspondent.

To Mr. NICHOLSON.

SIR,

RESEARCHES concerning the colouring principles of vegetable substances, have been very general and extensive; but I have never read any thing concerning the colour which resides in the juice of dog's mercury, and therefore conclude, that the properties of this vegetable are not very generally known.

The herb grows plentifully in woods and hedges. Its medical virtues are at present neglected; and it is never removed from its solitary abode for any useful purpose, but the instruction of the botanist. Accident may, perhaps, have disclosed this fact to him, that its juice affords a blue stain or colour: any one, however, may ascertain its truth by the following easy experiment:

Take

4.

Take a ftalk of the herb; rub the broken end upon a piece of fine white paper; and fqueeze out fome of the juice. No difference will be perceived at firft, where the juice has touched the paper, but fome hours afterwards it will be ftained blue.

This colour is in the greateft perfection, when the herb is young, and contains the moft juice, the thinneft and moft colourlefs part of which produces the beft blue : and it will be found by the experiment, that when the ftalk is very forcibly fqueezed, the paper is coloured rather green at firft, and never becomes of a good colour. On the contrary, the place touched with the cleareft and thinneft part of the juice, difcovers no colour for many hours, but at laft exhibits the fineft blue contained in the vegetable. This colour is not very durable, but after fome time turns to a red, which will continue for years.

A friend of mine, Mr. R. P. to whom I communicated thefe properties of the juice, and requefted him to devife fome experiments, introduced fome of the leaves and ftalks of the herb, into a fmall phial containing a folution of prepared ammonia, and in a little time the folution was coloured blue.

The herb, when dry, exibits no particular appearance ; but the roots, which are long, flender, and white, when dried, difcover in fome places a fine blue tinge upon the outer fkin ; and if a quantity be fteeped in water, it becomes of a blue colour, which afterwards turns red.

A folution of ammonia extracts the colour much fooner than water alone, and, I think, improves it.

Some years ago, I difcovered that the juice would ftain a blue colour when laid upon a piece of white paper or fine white cloth ; previous to that, however, I underftood, that certain individuals had endeavoured to procure a body of colour from this vegetable ; but of their fuccefs, or the methods of conducting their proceffes, I could learn nothing with certainty.

The properties of this vegetable appear fingular to me, and, if they are not generally known, may perhaps deferve the attention of your philofophical readers, as the elucidation of the fubject feems likely to be of fome advantage to the arts.

It is proper to be obferved, that as the plant is of a poifonous nature, it muft not be carelefsly ufed.

I am, Sir,

Your humble fervant,

C.

April 19*th*, 1800.

VII.

Account of a Work entitled " The Observations of NEWTON concerning the Inflections of Light; accompanied by other Observations differing from his; and appearing to lead to a change of his Theory of Light and Colours."   By a Correspondent.

THIS anonymous work confists of the obfervations of Newton concerning the inflections of light contained in his third Book of Optics, reprinted, and accompanied by new facts and obfcrvations * made concerning the fame phænomena, together with inferences, which differ altogether from thofe of Newton.

In his firft obfervation, Newton, repeating an experiment of Grimaldi, perceived as he had done, that in a beam of folar light paffing through a fmall hole $\frac{1}{42}$ of an inch wide, the breadth of the fhadow of a hair obferved at a diftance from the hole, was confiderably greater than that of the hair itfelf. This he conceived could only be effected by a *repulfive* power in the hair upon the *parallel* rays of the beam, turning the light afide *from* the fhadow, and rendering the fhadow confiderably *broader than it ought to be*, if the rays paffed by in right lines. By actual-meafurement he further afcertained, that the fhadow was broader in proportion to the diftance from the hair when nearer, than when farther from it; and this he imagined to depend upon the diminution of the repulfive power upon the more diftant rays of light, in confequence of which they were lefs turned afide from, and therefore in their progrefs invaded and reduced the dimenfions of the fhadow.

This of Newton is followed by an obfervation marked α, of the fame appearances. In which the firft thing noticed is, that the beam of light after paffage through the fmall hole, is no longer a beam of parallel rays, as Newton fuppofed, and has drawn it in his figure, but confifts of divergent rays. This is proved to be the cafe by actual meafurement, due allowance being made for the penumbral light of the fun, and is ftated to appear more clearly by immediate infpection of the various changes produced in the beam. This divergence is afcribed to the well eftablifhed principle of an attractive force exercifed by bodies upon light, in confequence of which when the hole is very fmall, its edges act upon the whole of the paffing light, and render it altogether divergent.

In fuch divergent light, therefore, the fhadows of bodies ought to be of dimenfions confiderably greater than the bodies themfelves, in proportion to the divergence of the rays, and to the diftance of obfervation. This proportion, however, is not exactly preferved, becaufe the rays in paffing by the hair, undergo from the *attraction* of the hair a change of direction *towards* the fhadow, in confequence of which the fhadow is really *lefs broad*, than it would have been if the rays had paffed in right lines, and this proportion of the fhadow is greater at fmaller, than at larger diftances. Thefe things are clearly fhewn by a figure,

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* The obfervations of the author are defignated by the letters α, β, γ, δ, &c. of the Greek alphabet.----N.

and the principle of attraction is so well established, as to leave no doubt of its affording the proper explanation of these appearances.

In opposition to the authority of Newton, he is quoted against himself. Five passages are produced from his works, which prove, that on different occasions he reasoned inconsistently and contradictorily concerning these phenomena.

This observation is concluded with the curious remark, that strictly speaking, all this reasoning has been applied, to what in fact is not a shadow, formed by the interception of light by the body, as appears in a subsequent observation γ, but a dark interval of the nature of those observed between the fringes of colours afterwards mentioned. So little have these phenomena been understood!

The second and third observations of Newton next follow, and are succeeded by the author's observation β, in which, instead of only three noticed by Newton at the termination of the shadow of the hair, an uninterrupted succession of fringes are described, the colours of which are stated to vary somewhat from those, the fringes themselves admitting of various changes, and this new arrangement being made with a view to analogies, that are stated to exist between these and other similar appearances, and in order that they may generally accord therewith, and with other observations of Newton made elsewhere.

Newton's fourth observation describes the successive appearance of the three fringes, with their dark intervals at different distances from the hair, and concludes from measures taken, that the squares of the breadths of the fringes, and their intervals together, are in the continual progression 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, or thereabouts. Observation γ, states, that at a very small distance from the hair, the shadow appears distinct, well-defined, and intensely black; that at a greater distance it is divided through its whole length by a line of light, and changed to a double shadow, resembling the shadows of two hairs, but not so intensely black as before; and that at yet greater distances the shadow increases in breadth, and diminishes in blackness, whilst the line of light putting on colours, and becoming more and more dilute, spreads over and disappears within the shadow, which again loses its double appearance. The first only is a proper shadow, which is quickly invaded and destroyed by the inflected lights of the two nearest fringes on both sides of the hair, which meet together at that distance therefrom, and the apparent double shadow consists of the two succeeding dark intervals, which become single as the intermediate light becomes dilute, and ceases to appear. This is what has been called a shadow by Grimaldi and Newton, and has been considered and measured as such.

The fringes and their intervals, as they are formed under various circumstances, and their various dimensions are then shortly noticed, and it is affirmed, contrary to the observation of Newton, that the breadths of the fringes and intervals, so far from observing generally the proportions assigned by him, do vary indefinitely through almost all degrees of their possible extension.

According to the fifth observation of Newton, a beam of light passing through a hole $\frac{1}{4}$ of an inch wide by the edge of a knife, on the blade of which part of the light fell,

exhibited

exhibited *two* ftreams of faint light fhooting out both ways from the beam into the fhadow like the tails of comets, at an angle of about ten or twelve degrees, and fometimes farther; at the fame time that a line of light appeared at the edge of the knife, vifible even out of the direction of the general light from parts beyond either the point or handle of the knife. This light, Newton obferves, was *contiguous* to the edge, and narrower than the light of the *innermoft* fringe, and paffed *between* it and the knife. The author, in his obfervation δ, remarks, that the preceding circumftances conftitute only one of the many cafes of the inflexions of light in which no fringes are formed, but only a general derivation and divergence of the rays is produced, which is owing to the parallelifm of the rays of light not being changed by their paffage through the large hole, but wherever the diverging of a beam of light is fuch that the inflexions of a body can give them the due arrangement, fringes are formed of endlefs varieties as thefe circumftances vary. Three cafes of fringes, making, with the former, four general cafes of inflexions, are then ftated as including thefe varieties of appearance as the difperfion of the beam from its greateft, is diminifhed to its fmalleft ftate of divergency and to parallelifm, and it is explained and fhown by figures how the formation of fringes depends upon the divergency, and is deftroyed by the parallelifm of a beam. The Newtonian experiment is then repeated with a beam of light paffed through a hole quarter of an inch wide, in a plate of lead, the *two* ftreams of light obferved by him concerning the origin and exiftence of which much doubt and difficulty had arifen among philofophers, are fhown to be owing the one to the edge of the lead of the hole, the other to the edge of the knife, and to be produced in the parallel beam of light. The line of light obferved by Newton is noticed as indeed worthy of particular attention, being in a very remarkable manner derived out of the whole body of the light paffing by the edge, and not merely from that *contiguous* to it, and not being narrower than the *inmoft* fringe, or paffing *within* it, becaufe in this cafe no fringes are or can be formed, paffing, however, at thofe very points near the body at which, under other circumftances, the *many* neareft fringes are formed.

According to obfervations the fixth and feventh, by approaching the parallel edge of another knife to that of the former, at the diftance of the 400th part of an inch, juft before they touch, the whole paffing light is divided into two ftreams, each bent towards the neareft edge with a dark fhadow between them, which ftreams vanifh upon contact of the knives, their parts *fartheft* from the direct light vanifhing *laft*. As the knives, however, approached each other, and before the fhadow appeared, the three fringes themfelves appeared on the inner ends of the ftreams on either fide of the direct light, and grew diftincter and larger until they *all* vanifhed, the outmoft firft, the middlemoft next, and inmoft laft, leaving the ftreams of light defcribed in the fifth and fixth obfervations. Thefe ftreams, therefore, Newton concluded paffed by the edges at *lefs* diftances than any of the fringes, and the fringes at different diftances according to the order of their vanifhing.

<div align="right">Obfervation</div>

Obfervation ε diftinctly ftates all the phænomena that occur at the parallel edges of two knives in a beam of parallel light approaching towards each other in the fame plane. At firft appear two borders of inflected light, one at each edge, fimilar each to the one before obferved at the fingle edge. In thefe borders, and of this light, are made two fets of fringes, one at each edge, which beginning in the form of flender white ftreaks and dark intervals on the edges of the borders neareft to the knives become broader, and coloured, and by degrees occupy the whole light between the edges, forming two complete fets of fringes, each compofed of the colours before defcribed in obfervation β, and beginning in the centre of the paffing light. Thefe fringes grow broader, then vanifh by pairs, until at laft only one pair, with a dark interval between them, remains, which decreafe and vanifh upon the contact of the knives. From this ftatement it appears, that Newton was miftaken in confidering this laft pair of fringes as ftreams of light diftinct from the fringes, and as being the very borders themfelves of inflected light obferved at the edge of a fingle knife, out of which they are indeed formed, and of which they really are only an inconfiderable part. In this cafe each edge by inflecting gives the light paffing neareft to it a due divergency, and the other forms fringes therein.

In obfervation ζ a very fmall hole, $\frac{1}{50}$ of an inch wide, being ufed, gives a due divergency to the light, and, therefore, the two edges, when even at a diftance, form fringes to themfelves of an arrangement of colours, the inverfe of thofe in the preceding obfervation inftead of borders of white light, but upon their nearer approach break up and diffolve thefe, and by their more powerful mutual attractions form fringes in a contrary order, fimilar to thofe of obfervation ε, thus exhibiting, under different circumftances, two different fets of fringes.

Obfervation η, fhews that even in different diftant planes the fame phænomena, with due change of circumftances, are produced at the edges of two knives.

Obfervation θ, by placing parallel fided plates of lead, of different breadths, in a divergent beam of light paffed through a very fmall hole, produced not only the two ufual fets of fringes at their external edges, but alfo a fecond double fet within the fhadow beginning from the centre thereof, both fets of fringes growing broader as the breadth of the lead was diminifhed, and the fringes in the fhadow vanifhing by pairs, and at laft entirely difappearing. Thefe fringes are the coloured ftreaks obferved by others on the fhadows of fmall bodies. Cylinders alfo of fucceffively reduced diameters down to the hair of the firft obfervation, produce the fame appearances, and even with the fame cylinder or plate of a due fize, the fame appearances may be produced by diftance or obliquity of obfervation.

In Newton's eighth obfervation the edges of the knives being inclined until they met, and the beam paffing through a hole $\frac{1}{42}$ of an inch wide, at a very fmall diftance from the knives the fringes appeared at, ran along the edges and met, and where they met ended; but at a much greater diftance they appeared to grow broader as they approached, then met and croffed each other, and then appeared broader than before, whence he concluded,

that by the approach of the edges the diftances of the fringes therefrom are not *increafed* or *changed*, but that the bendings of the rays are thereby *increafed*, and that the neareft knife determines which way the ray fhall be bent, and the other *increafes* the bent. Obfervation *ı*, repeats the experiment, and fhows how by the inclination of the edges all the appearances which at different times were before produced by the fucceffive approach of the edges, are now all at one time exhibited at their different diftances from each other. The fringes where the edges are diftant are formed in the light rendered divergent by the fmall hole, where they appear to have croffed they are made by each remote edge in the light bent by the neareft edge, in light which, if the remote edge was abfent, would there form the fringes of the neareft edge; upon the approach, therefore, of the knives, the diftances of the fringes of each knife *are changed* and increafed, contrary to what Newton obferves, for they are then made in light beyond that of the firft fringes of each edge, that light neareft to each edge compofing *then* the fringes of the remoter edge, and as each edge thus makes its fringes in the light bent by the oppofite edge, the nearer it approaches in more bent light will the fringes be made ; not that the approach bends the light, it leffens the bent by contrary attraction, but changing at the fame time the light of the fringes, it makes them in other light nearer to the bending edge, and, therefore, more bent than the former light, and, therefore, the fringes are alfo broader. This making of broader fringes in light more bent led Newton into a wrong conclufion. The light does not increafe its bending and divergence, the fringes change their light: by the approach of the edges they are made in light more bent and diverging, but the general bent of the rays is really *diminifhed* by that approach, and whilft the neareft knife continues to bend the rays towards itfelf, the other knife *diminifhes* that bent.

In obfervation the ninth, Newton having meafured at different diftances from the knives the dimenfions of the fringes, intervals, and fhadows, and finding that the diftances of the point of croffing of the dark lines between the firft and fecond fringes from that of the concourfe of the fhadows of the edges, continually increafed, and taking it for granted, contrary to the fact, that thefe gave the true meafures of the diftances of paffage of the light itfelf from the concourfe of the edges, concluded, that the light which made the fringes was *not the fame* light at all thofe diftances, but paffed *nearer* to the knives, and was *more* bent when the fringes were obferved at fmall than at greater inftances. Obfervation *ı*, fhows that thefe increafed dimenfions are merely produced by the divergency of the light of the experiment, a circumftance which efcaped Newton in his firft obfervation alfo, and that, confequently, the fringes are always *the fame* at all diftances of obfervation, and are made in the *fame* light *equally* bent, and paffing at the *fame* diftance from the edge.

Obfervation the tenth of Newton is employed in an attempt by actual meafurement to afcertain the nature of the curves formed by the fringes and intervals in the preceding experiment. They are determined to be hyperbolic, and their afymptotes drawn. The fringes themfelves are by him feparated, and diftinguifhed from another triangular light,

3   fuppofed

fuppofed to appear between the two laft neareft the concourfe of the edges; but obferva-
tion λ, remarks, that this light is really the light of the laft pair of fringes neareft
to the concourfe of the edges exhibiting the appearance and colours which fhould belong to
thefe fringes under fuch circumftances, and from the overfight difcovered with refpect to
this pair of fringes, from the accuracy required in the meafurements of thefe fhadowy
parts, from the actual parallelifm of the lines of the fringes on one fide to, and the ap-
parent probability that they will crofs and cut the afymptotic lines on the other fide,
queftions the accuracy of the determinations refpecting thefe lines. Obfervation μ, fhows
how by ufing a triangular piece or cone of lead, or the points of pins of various fizes,
inftead of the parallel-fided pieces or cylinders of obfervation ϑ, all the appearances of that
obfervation are at once produced at the edges of the triangle or cone.

In obfervation eleventh, Newton examines the fringes made by differently coloured
prifmatic lights, and finding the fringes made by red light alone formed at greateft, by
violet at fmalleft, and by intermediate colours at intermediate diftances, as if all thofe
lights were mixed in the white, and early feparated at the time of inflexion, concludes
that the colours arife not from any modifications impreffed upon the rays of light by the
hair, but from fimple feparation by various inflexions of the feveral forts of rays. To this
mode of explanation another is oppofed in obfervation ν, namely, that the different rays
when apart are, indeed, inflected in the fame manner to make fringes of various colours,
as are rays of the fame colour when being at once varioufly feparated from white light
they make, together with the reft of the colours, party-coloured fringes. This, however, is
attributed not to fimple feparation alone, but to modification at the time of feparation,
which gives to each portion of the white light thus feparated a permanent condition,
diftinct from all the feparated portions, and the original portion out of which they were
feparated.

Separating by inflection a fmall portion of white light into various colours, and then re-
turning the various rays back again to the fame point under the fame circumftances, or
fuppofing them fo returned, they will unite into the original portion, or fuppofing them
returned without other change to different points, they will move in parallel lines to each
other, preferving their diftinct characters; or if now a fecond time returned without
further change, coming in the fame parallel direction, they will be inflected differently.
Thefe circumftances evince a change and permanent conftitution, fuperinduced upon each
of thefe portions, and by which they are rendered diftinct from each other, and from the
original portion until their re-union into the fame, as a portion fimilar to that from which
they were derived. Thefe fmall portions of white light are its rays, which pafs through
the intervals between the particles of bodies, and coloured rays are always formed there-
from by fimilar modifications, produced by the actions of the particles of other bodies at
the fame time, modifying as well as feparating them. The very mode of diftribution of
the white light argues the action of a principle, modifying as well as feparating, rather

M 2   than

than the exiftence of different forts of rays, which may not eafily be fuppofed fo numerous, and capable of fo regular a difperfion. Thus modification is eftablifhed, as well as feparation, and thefe principles of condition and formation alone fuit the phænomena. The fringes before obferved, require complete fets of the different coloured rays to iffue *at the fame time*, from *each interval* or point of paffage of the medium. For this purpofe an appropriate arrangement is required of the rays, if of originally different forts in different paffages, and only mixed in the white light, left a nearer red fhould be more inflected than a further blue, and the like, and this arrangement will even then agree with only one ftation of the inflecting body; but this ftation is indifferent in producing the phænomena. A confufed mixture therefore, of rays, is inconfiftent with the phænomena, no collateral arrangement will anfwer; and if all the different rays muft be confidered as paffing through all the intervals of the medium, they cannot all iffue from, or be returned at the *fame time*, to *the fame* place as the phænomena require. By fuppofing, however, that the time of fucceffion of all the rays in the fame paffages is imperceptible to fenfe, then the hypothefis of fimple feparation, and original diftinctnefs of the rays, may anfwer for thefe phænomena, as well as that of feparation and modification; but as they will both then anfwer equally well, other the general phænomena can alone determine which is true, and by them they muft be judged.

Omitting, however, all confideration of thefe two hypothefes, it is clear, that in the preceding obfervations concerning inflections, Newton is miftaken as to all the moft important points. He has upon different occafions, endeavoured to eftablifh a power altogether repulfive, or a power changed at different diftances from attractive to repulfive. This repulfive power has been always hitherto ufed in philofophy, to account for the reflections of light, and as it is clearly fhewn not to exift, another manner of accounting for reflection is to be fought for.

The concluding obfervations of the work are general. It is ftated among other things, that the Newtonian doctrine of light and colours is unfounded as to both its principles; the conftitution of folar light (queftioned indeed in the laft obfervation), and the fits of eafy tranfmiffion and reflection of the rays. The fame Author is preparing to publifh, Obfervations concerning the Colours of thin Plates, fhewing the phænomena to be merely inflections of light, that the Newtonian fits of eafy tranfmiffion and reflection have no exiftence, and that Newton's doctrine of the grounds of the colours of natural bodies is unfounded.

The obfervations of Newton in the above work marked with the numerical figures, are printed with a reduced margin; the new obfervations marked with the letters of the Greek alphabet, are printed with a full margin.

VIII.

Singular Inftance of Fecundity. In a Letter from the Rev. W. PEARSON of Lincoln.

To. Mr. NICHOLSON.

SIR,

I Was lately informed by Mr. Morris of Dunham, in this county, that he has an ewe which on Friday night, and Saturday morning laft, yeaned *five* lambs all full grown; which I confider as fo remarkable an inftance of fecundity, that I am induced to tranfmit a notice of it for public record in your Journal.—The ewe is fomewhat above a middle fize, and is a *crofs* of the Lincolnfhire and Leicefterfhire breeds, three years old, and the ram which fhe was put to, is of a thorough Leicefterfhire ftock: three of the lambs were male, and two female; they are all dead, but four of the five were found alive, and it is fuppofed would have furvived, if the inclemency of the night had not ftarved them. The fhepherd, who vouches for the veracity of thefe facts, afferts, that the fourth lamb came in an unnatural pofition, and that the ewe when he firft went to her about midnight, was turned upon her back, and muft have perifhed, on account of being in that pofition, if he had not accidentally come to her affiftance: fhe is, however, perfectly recovered, and many circumftances concur in proving indubitably, that, though there were about thirty other ewes in the fame grounds at the time, yet the five lambs in queftion were all yeaned by the fame ewe: I will mention one proof, which of itfelf may fatisfy thofe readers, who may otherwife feel difpofed to doubt the authenticity of my information:—the fhepherd, upon a fuppofition that the two firft lambs that came were all that he had to expect, took them into a fhelter, and returned to take care of the ewe, when, to his furprife, he was prefented with two more; thefe were taken away alfo, and on his fecond return, the fifth was found dead newly yeaned, and no other fheep was near:—the fhepherd is not guilty of fabricating tales, and Mr. Morris, who is a refpectable yeoman, has at this time all the five fkins, which were taken from the lambs.

It may not be improper to add, that the other ewes of the fame ftock, and in the fame pafture, have fome two lambs, and fome only one each, with the exception of one which has three living.

<div align="center">I am, Sir,</div>

<div align="center">Your obliged Correfpondent,</div>

Lincoln, April 14, 1800.   W. PEARSON.

P. S. Since the receipt of the above, the learned writer has favored me with an extract from the Lincoln, Rutland, and Stamford Mercury of 18th laft, ftating that "on the 24th ult. at Mardon Woole, in Northumberland, an ewe, the property of T. Reed, Efq.

<div align="right">yeaned</div>

yeaned five lambs, four of which were then living, the other appeared to be something of an embryo fœtus, though nearly perfect;" and that " what tends to heighten astonishment, is, that the same ewe has had an equal number at each time for two preceding years."—W. N.

IX.

On the Phenomena of the Morning and Evening Dew.　By C. A. Prieur *.

THE evening and morning dews (*le Serein et la rosée* †) present themselves so often to our observation, and in so many different situations, that it is surprising that philosophers should have attended so little to these phenomena, or that they should have been contented with very loose explanations on this subject.

In the year 1788, I had occasion to ride out very frequently on horseback in the morning and the evening, and being thus exposed to the impressions of the dews, I was induced to meditate particularly on these appearances, hitherto so little examined. I well knew that the humidity deposited on bodies in the open air at the setting of the sun, is not the same as is afterwards observed upon them at its rising; and that consequently there is an interruption in the appearance, namely, an evaporation of the evening dew, or humidity, and a new production of moisture in the morning. I was also aware of that partial explanation of the evening dew, by which it is said, that the diminution of the heat in the air renders it incapable of retaining any longer the water it had taken up during the day. But why is there a wind always blowing from that side of the horizon occupied by the sun, and constantly accompanying this precipitation of water; and again, how does it happen that the same luminary a short time before its rise, and even after having given a slight degree of warmth to the air by its presence, appears to cause a greater cold, a stronger wind, and a more abundant precipitation of water than in the evening.

To remove this difficulty, I had recourse to the fundamental principles, by the assistance of which, Monge has so ingeniously accounted for the greater part of the phenomena of meteorology ‡; and I soon perceived, that they were sufficient to explain the appearances which formed the subject of my enquiry.

These principles, which it will be proper to enumerate, are the three following; 1. The air in like circumstances dissolves more water the greater its density, that is to say, the more it is mechanically compressed.

　2. And also more the higher its temperature.

* From a letter addressed to Cit. Hassenfratz, and inserted in the Journal of the Polytechnic School, vol. ii. cahier 6. p. 409.
† We have not distinct words corresponding with these in our language ?---N.
‡ Annales de Chimie, tome V.

3. Under

3. Under equal preſſure, and at like temperatures, air holding water in ſolution has a leſs ſpecific gravity than air alone; and this ſpecific gravity is leſs the greater the proportion of water ſo diſſolved.

It muſt alſo be recollected, that the changes of preſſure and weight which may take place in certain columns of the atmoſphere, muſt neceſſarily diſturb the equilibrium, and produce motion or currents in the ſame.

Theſe principles being eſtabliſhed, let us ſuppoſe for a moment, to render our inquiry more ſimple and eaſy, that the earth deprived of its rotatory motion, ſhall remain motionleſs in the preſence of the ſun, and let us beſides diſmiſs from our conſideration all local influence, and in general all thoſe cauſes which might diſturb the regularity of the effects.

In this ſtate of things, what will be the conſequence with regard to our atmoſphere?

The air expoſed to the rays of the ſun will be heated, and principally in the part contiguous to the earth, on account of its greater denſity, and the reverberation of that planet. This heated air will acquire a greater diſſolving power, and will, in fact, diſſolve much water if that fluid be preſent. It will take it from the ſeas, lakes, rivers, ponds, and other reſervoirs, whoſe ſurface is expoſed to its action, and will even abſorb a portion of the humidity of the ground.

Let us in the next place conſider, what happens in any aſſumed vertical column in the atmoſphere, and firſt in that placed immediately beneath the ſun. The air heated at the lower part diſſolving water, and thus becoming ſpecifically lighter, will riſe, and be replaced by other contiguous air. An aſcending current will therefore be eſtabliſhed in the column. If this column were ſeparated from the others, as if it were contained in a vertical tube, in proportion as the lower air roſe loaded with water the upper air would deſcend, become charged in its turn, riſe and be replaced; and that perpetually. And the air holding water in ſolution, would in conſequence of under ſaturation from cold and diminiſhed preſſure, when it had arrived at a certain height, let fall the exceſs of water, and form a miſt or cloud, which might continue to riſe by virtue of its acquired motion, but which, after a certain accumulation, would fall again in rain. It may alſo be conceived, that this tranſlation of the air upwards and downwards, would be made either by a mutual infiltration, or currents in both directions, which would be conſtantly and regularly kept up.

But this is not entirely the caſe with our vertical column, becauſe it is not in fact ſeparated from the others in its vicinity. Theſe alſo are ſubjected to the ſame operations, with the exception only, that the effects are leſs the more remote they are from the column immediately beneath the ſun. The heat, the ſolution of water, and the force of aſcenſion, gradually diminiſh as the diſtance from this central column increaſes. If therefore the ſurface of the earth were a plane, the proper repreſentation of the riſing air would be a cone with its ſummit directed to the ſun; and on the other hand, as the abſolute weight of each column is increaſed by the whole of the water it has diſſolved, the equilibrium requires

that

that there fhould be on all fides a lateral divergence, which muft evidently take place where the preffure is leaft. Thus on the fame fuppofition of the earth being a plane, we fhould obferve the upper air defcend and precipitate itfelf along the fides of the cone, producing by this oblique direction to the axis, a current on all fides directed from the fun; and this current would be increafed by the vacuum formed at thofe places where the air is fuffi-ciently elevated, and could precipitate its fuperabundant water.

This image will require very little more dification to apply it to the exterior of our globe. The conical furface will be converted into a cap, enveloping the enlightened portion of the earth, and following the curvature produced by the currents.

Infpection alone of the figure in Plate 3, will fhew this curvature. The globe may be readily feen placed in the center of a circular ftratum, fuppofed to be filled by the atmof-phere. The circumference of the earth has twenty-four divifions, from which are raifed a like number of perpendiculars to its furface, or vertical lines, in order to afford a notion of the change of phenomena from hour to hour. The fun fuppofed to be placed at S, in the in the continuation of the line T S, is fuppofed to have all its mafs united in the center; and laftly, the curve a, b, c, though arbitrarily drawn, becaufe its law is unknown, will be fufficient to fhew the exiftence of the effects which we are interefted to explain.

Such, therefore, will be the refult of the fuppofed circumftances. Water diffolved by the lower air expofed to the fun; a motion of afcent in this part; defcending divergent currents on all fides, fpreading over the earth. Thefe currents, moreover, communicate their water to the heated columns, which rife and diverge laterally; and this water is pre-cipitated, becaufe the air of the currents proceeding from the upper regions is too cold to keep it in folution, or becaufe the preffure is lefs from the diminifhed weight of the columns, as their diftance from the line immediately beneath the fun is greater, as well as becaufe they may mix with other cold air near the furface; and laftly, the ground, and all other bodies in the direction of this precipitated water, will be wetted therewith.

The morning and evening dews are here evident to our confideration, with the wind and the cold which attend them; but a few more remarks are wanting to complete the defcription. We muft firft obferve that the defcending currents are prolonged till the refiftance of the air, through which they pafs, has entirely deftroyed their motion. Again, we fhall fee immediately beneath the fun, a circular fpace greatly heated, which does not prefent to the inhabitants of that region the phenomenon we have been defcribing. As we depart from this fpace, and in proportion as the fun appears in a more oblique direc-tion, we arrive at a region lefs heated, where the wind from the direction of the fun, and the precipitation of moifture, begins to be perceived. This region forms a crown round the circular fpace before mentioned. And, laftly, by departing ftill more from the inner border of this crown, the wind is found to be ftronger, the cold more perceptible, and the precipitation of water more abundant; which effects afterwards diminifh to a certain diftance, and entirely ceafe at the furface of the earth diametrically oppofite the fun.

In

In this manner, on the hypothefis of the immobility of the earth with regard to the fun, there would be beneath that luminary a very extended region eternally, and uninterruptedly fubjected to the phenomenon of the morning and evening dew, accordingly as the obferver was placed to the eaft or the weft.

But let us now affume the real ftate of things, and reftore to the earth its diurnal rotation. The preceding phenomena will then take place on fucceffive parts of its furface. Thofe places from which the fun is defcending, and ready to fet beneath the horizon, will foon perceive the appearance of the evening dew with a weft wind fpringing up, which phenomena will increafe gradually till after the fetting of the fun, and then the effect will diminifh and entirely ceafe. During the night the humidity will evaporate, and entirely difappear, provided the air be not already too much loaded with moifture. Towards the next morning, juft before day-break, the phenomenon will again prefent itfelf on the eaftern fide, with the fame circumftances and gradations, the maximum of effect being alfo when the fun is yet beneath the horizon; but with this very remarkable difference, that the effects will be much ftronger than thofe of the evening; that is, there will be more wind, more moifture depofited, and a more fenfible degree of cold. The reafon of this is, that in the evening the precipitation of water, the wind, and the cold which acaccompany it, ought to be diminifhed, becaufe the whole takes place in the vicinity, and by the mixture of air which the fun has heated during the day; whereas in the morning the coldnefs of the night air permits, or gives a much greater effect to the phenomenon.

We fee likewife, that in the two temperate zones, where the winter and fummer have a great difference of temperature, where the length of days and nights vary much, the effects of the evening and morning dews are varied and irregular. In fummer, if on the one hand the air diffolves more water in the day, on the other hand, the precipitation of the evening dew is made in an air very much heated, and that of the morning dew in air which the fhortnefs of the night has cooled only to a certain point. In this cafe the folution of moifture is confiderable, and the precipitation little. In winter, on the contrary, the caufe of folution is lefs, but that of precipitation more effectual. Local circumftances, more efpecially the vicinity of water, muft alfo influence the effects. Fine weather increafes, and renders them more fenfible. Clofe weather weakens or deftroys them.

Under the torrid zone the days and nights are more nearly equal, and at the equator they are equal at all times. The difference between the fummer and winter temperatures is lefs confiderable than in our latitude, and the fky is almoft conftantly clear. It muft follow, that the morning and evening dews will be heavier under this burning climate, than elfewhere on the earth. And this agrees with the teftimony of travellers. In Egypt, in Afian Turkey, at the Antilles, at Mexico, in veffels failing between the Tropics, the morning and evening dews fall fo abundantly, that they produce the fame effect as fhowers of rain.

But there is a very important confequence, which it feems allowable to draw from thefe phenomena; namely, that they muft influence the produ&ion and permanence of the trade winds. For every day, almoft regularly, the air of the torrid zone being folicited to move in two oppofite dire&ions by forces very different in quantity, it muft tend in fa& to ac-quire and preferve a motion or current, in the dire&ion of that ftrongeft power, which in this cafe is from the eaft, being the wind which brings the morning dew. This caufe muft be the more effe&ual, as it a&s in the lower part of the atmofphere, where we feel the trade winds, and becaufe it affe&s the denfeft portion, and confequently muft the more readily move the whole.

Under the glacial zone, where particularly during the winter the fun fcarcely fkims the horizon through the whole day, the precipitation of water will be very confiderable, by reafon of the coldnefs of the climate, and a thick fog will prevail, which will fcarcely be diffipated in fummer; but in winter will extend far into the temperate zones. Thus we fee during the winter feafon, in our country, very confiderable fogs, which have only a feeble light even in the middle of the day.

It would be curious to prefent in this place a numerous feries of accurate obfervations, on the circumftances which accompany the morning and evening dews at different times of the year, and in different countries, the hours at which thefe phenomena begin and end, the intermiffions or irregularities, with which they are affe&ed in different fituations; but if the generality of the caufes to which they are here attributed, fhould excite the attention of philofophers, the interefting tafk here indicated will affuredly and fpeedily be per-formed.

X.

Thoughts on Magnetifm, by RICHARD KIRWAN, *Efq;* L.L.D. F.R.S. *and* M.R.I.A*.

1. THERE are two ways of explaining a natural phænomenon; the firft, is by dif-covering the conditions and circumftances of its produ&ion and the laws by which its a&ion is governed; the fecond, is by fhewing its analogy, fimilarity, or coincidence with fome general fa& with whofe laws and exiftence we are already acquainted; this laft mode is by far the moft perfe& and fatisfa&ory. In the firft fenfe of the word ele&ricity and magne-tifm have been in fome meafure explained, but in the laft fenfe neither; the primary caufe of magnetifm in particular has hitherto been fuppofed to relate to iron alone, or its ores, and to ftand unconne&ed with all other natural phenomena.

2. If therefore any other general fa& or power can be difcovered to which it bears fome analogy or fimilarity, it may fo far be faid to be explained. Now fuch fa& or power I think may be affigned, namely, the power of *cryftalization.*

* Tranfa&ions of the Royal Irifh Academy. Vol. VI.

3. By

3. By cryſtalization I underſtand that power by which the integrant particles of any ſolid poſſeſſing ſufficient liberty of motion unite to each other; not indiſcriminately and confuſedly, but according to a peculiar uniform arrangement, ſo as to exhibit in its laſt and moſt perfect ſtage regular and determinate forms.

4. This power is now known to be poſſeſſed by all ſolid mineral ſubſtances.

5. The forms which cryſtals, even of homogeneous ſubſtances, exhibit, are often very numerous ; however in moſt caſes they may be reduced to a few primordial forms, which, as Abbé Hauy has lately experimentally proved, are derived from certain original forms appertaining to the minuteſt particles of their concretion.

6. The aſſemblage of theſe ultimate particles into viſible aggregates, ſimilarly arranged, neceſſarily requires that one of their ſurfaces ſhould be attractive of that particular ſurface of the other, which preſents a correſponding angle, and repulſive of that which preſents a different angle, otherwiſe the various regular rhomboidal and other polygon priſms and pyramids, which cryſtals preſent us, could never exiſt ; conſequently the minuteſt priſm, being once formed, could never be prolonged if one end of ſuch priſms were not attractive, and the other repulſive of the ſame given ſurface.

7. Hence it has been obſerved that cryſtalization never takes place in the middle of any ſolution, but always begins at the ſurface or on the bottom or ſides of the veſſels that contain it, for the particles in the middle of the ſolution being confuſedly mixed with each other, and exerting their repulſive as freely as their attractive powers, the one conſtantly counteracting the other, no ſenſible accretion of a regular kind could take place, whereas the repulſive power of the uppermoſt particles, or of thoſe that reſt on the ſides or bottom of the veſſel, is reſtrained and impeded.

8. The repulſive power of cryſtalizing ſubſtances alſo appears in many other inſtances (of the attractive no doubt has ever been formed.) Thus if ſaturate ſolutions of nitre, common ſalt, and tartar vitriolate be mixed and ſet to cryſtalize, each will cryſtalize a part, which could not happen if the particles of each of theſe ſalts did not only attract their ſimilar homogeneous, but alſo repel thoſe of a different ſpecies, otherwiſe the mere caſual circumſtance of greater proximity to one than to the other would impel them to unite indiſcriminately. Again, if a ſaturate ſolution of allum be mixed with a turbid mixture of clay, and abandoned to inſenſible evaporation, after ſome time the clay will ſubſide and form a dry maſs, but in the interior of this maſs large regular cryſtals of allum will be found, whoſe component particles muſt, to reunite, have diſplaced and repelled the particles of clay with which they were ſurrounded.

9. If to a ſaturate ſolution of a ſalt that difficulty cryſtalizes, a cryſtal of a ſalt of the ſame ſpecies be inſerted, the whole ſolution will ſoon be brought to cryſtalize, as the cryſtal inſerted attracts the particles diſſolved, by its different ſurfaces ; but if a ſalt of a different nature be inſerted this will not happen, cryſtalization will not be promoted.

10. If to a ſolution of 2 parts nitre, and 3 parts Glauber's ſalt in 5 parts water, a cryſtal of nitre be inſerted, the nitre alone will cryſtalize ; or if inſtead of nitre a cryſtal of Glauber

be

be inferted into it, the Glauber alone will cryftalize, Do not thefe experiments fully evince both the attractive and repulfive powers, not only of different falts but of different furfaces of the fame falt?

11. Thefe powers within their proper fphere of action have been found indefinitely great; thus water confined in cannon feveral inches thick, and expofed to a degree of cold much beneath the freezing point, has been obferved to cryftalize into ice that burft the metallic impediment oppofed to the form it then affumes.

12. The vaft difference however attending the developement of thefe two powers (of magnetifm and cryftalization) will undoubtedly ftrike many as an infuperable objection to their identity, yet their *direction* in all its varieties being exactly the fame, difference in other circumftances feems to me to indicate rather a variety of *degrees*, in the fame power, than any effential difference in the powers themfelves.

I now come to the application of the above principles to the magnetic phænomena. Thefe may in general be reduced to the following, viz. *Attraction, Repulfion, Polarity.*

Communication.
Declination.
Inclination.
Exclufive appropriation to Iron.
Deftruction of the Magnetic power.

1ſt, Attraction, Repulfion, Polarity.

The quantity of iron found on and within fuch parts of the furface of the globe as we are acquainted with, far furpaffes that of any other mineral fubftance fingly taken, or even of many of them taken together; fcarce any ftone or metallic ore or earth is found free from it; it enters into their compofition in the proportion of from 2 to 18 or 20 per cent. and perhaps at a medium we may ftate it in all of them at 6 per cent.; moreover its own ores are of all others the moft common and the moft copious; in many places, particularly in the moft northern climates, whole mountains of it are found, and many of them magnetic. When to this confideration we add that of the fpecific gravity of the globe, which has been found to be 4,5 times heavier than water, notwithftanding the immenfe quantity of water that covers the greater part of its furface to confiderable unknown depths, and notwithftanding that the fpecific gravity of by far the greater part of the ftones and earths it contains does not exceed and fcarcely amounts even to three times the weight of an equal bulk of water, and that the quantity of mineral fubftances whofe fpecific weight exceeds four times that of water is almoft infinitely fmall in comparifon to the other known component parts of the globe, and finally that the weight of moft iron ores is about four or five times that of water; all this I fay confidered, it is difficult to avoid concluding that the interior part of the globe confifts chiefly of iron ore difpofed in one or more aggregate maffes; a conclufion that is farther confirmed, on reflecting that volcanic lavas ejected

from

from the deepeft receffes with which we are acquainted contain from 15 to 20 or 25 per cent. of iron in the ftate moft favourable to magnetic attraction.

Taking then this affertion to be as fully proved as its fubject matter is capable of being afcertained, we may deduce from it the following corollaries :

1ft. That as the ferruginous matter in the globe being by far the moft copious, its univerfal attractive power is principally feated in the ferruginous part.

2d. That as all terraqueous matter was originally in a foft ftate, its parts were at liberty to arrange themfelves according to the laws of their mutual attraction, and in fact did coalefce and cryftalize in the direction in which they were leaft impeded by the rotatory motion of the globe, namely in that which extends from North to South, and principally and moft perfectly in the parts leaft agitated by that motion, namely thofe next the centre.

3d. That this cryftalization, like that of falts, might have taken place in one or more feparate *fhoots*, or as we may here call them, immenfe feparate maffes, each having its poles diftinct from thofe of the other, thofe in the fame direction repulfive of and diftant from each other.

In confequence then of the univerfal law of attraction of the particles of matter to each other, thefe internal magnets exert a *double* power of attraction ; the firft and moft general, on the particles of *all* bodies indifcriminately in proportion to their denfity, and the direct or inverfe ratio of the fquares of their diftances according as thofe bodies are found within or without the earth's furface ; and the fecond, on bodies of their own fpecies in proportion to their homogenity, and to the correfpondence of the arrangement of their integrant particles with that of the integrant particles of thefe internal magnets.

A magnet therefore is a mafs of iron, or of iron ore, whofe oxygenation does not exceed 20 per cent. or thereabouts, whofe particles are arranged in a direction fimilar to that of the great internal central magnets of the globe. This I call the *magnetic arrangement*.

The particles of iron attract each other more forcibly than thofe of any other known fubftance. This appears by its cohefion, hardnefs, elafticity, and infufibility, in each of which properties, or at leaft in the combination of moft of them, it exceeds all other known bodies.

Hence a magnet *attracts iron* when within the fphere of its action, by forcing, in virtue of its attractive power, a certain proportion of its integrant particles into a difpofition and arrangement fimilar to that of its own. For in this cafe it exerts a double attractive power, that of the particles of iron to each other, which we have feen to be the greateft of all others, and that of cryftalizing bodies, which we have alfo feen to be indefinitely great.

The cryftalizing power being at once attractive and *repulfive*, according to the direction of the furfaces, (No. 6.) hence we fee that one part or end of the magnet muft repel that which the other has attracted, as long as the fame difpofition of parts remains.

The difpofition of parts in a particular magnet, being fimilar to that which obtains in the great internal general magnet, extends in the direction of from North to South. Hence

magnets,

maguets, when at liberty to move with a certain degree of freedom, and iron, when a fufficient number of its particles are arranged in that direction, and has fufficient liberty to conform to it, points to thofe poles.　Hence this property is called *Polarity.*

The magnetic power is greater or leffer according to the number and homogenity of the particles *fimilarly* and *magnetically* arranged.　Hence fm:ll magnets may be more powerful than a larger, and hence a magnet will attract a magnetized needle at a greater diftance than one not magnetized.

The magnetic power decreafes in a certain ratio of the diftance of the particles that exercife it.　Hence it is ftrongeft in the point of contact, and at the poles, as it is there moft *unfaturated,* and weakeft in the central part, which feparates the two oppofite poles.

When a magnet is broken into fmall pieces its power is nearly deftroyed, becaufe though the poles fhould be all of the fame kind, yet the diftance of each from the oppofite pole is fo fmall that their powers counteract, and confequently deftroy each other.

If when a needle is attracted by the fouth pole of a magnet a bar of iron be placed on the north pole, the needle is ftill more ftrongly attracted, becaufe the iron acquires alfo a fouth pole, whofe force is joined to that of the magnet.

If two needles be fufpended from any given pole of a magnet they will diverge, becaufe they both acquire the fame polar arrangement.　If a bar of Iron be laid on that pole of the magnet, the divergence will diminifh, becaufe the next end of the iron will acquire the difpofition of the oppofite pole, and confequently counteract the repulfive power of the magnet.

A magnet will not tranfmit its power through a bar of iron if this be too long. Mufchenbrouck limits their length to fix feet, but this depends on the ftrength of the magnet.

The power of a magnet (every thing elfe being equal) depends on the *number of its furfaces magnetically arranged,* and the *accuracy* of that arrangement.

The arrangement is accurate when the fynonimous furfaces are exactly parallel to each other, and originally conformed to and parallel with thofe of the great general magnet.

The magnetic attraction is ftrongeft in the direction perpendicular to the magnetic furfaces, and weakens in proportion to the magnitude of the angle of direction with the perpendicular, and confequently is *null* when at a right angle with it.　Hence the magnetic power feems concentrated at the *poles,* and the lateral powers are the weakeft, as they originate only in the oblique direction of furfaces, or from furfaces inaccurately arranged.

(To be continued.)

XI.

Defcription of the Revolving Doubler. W. N.

AS the doubler of Bennet, to which I adapted the following machinery in the year 1788, is not yet generally noticed in elementary works, and, confequently, little known, I have thought it might be acceptable to copy the fhort Defcription and Plate from my paper in the LXXVIIIth volume of the Philofophical Tranfactions.

Plate IV. reprefents the apparatus of the doubler fupported on a glafs pillar $6\frac{1}{2}$ inches long. It confifts of the following parts. Two fixed plates of brafs, A and C, are feparately infulated and difpofed in the fame plane, fo that a revolving plate B may pafs very near them, without touching. Each of thefe plates is two inches in diameter; and they have adjufting pieces behind, which ferve to place them accurately in the required pofition. D is a brafs ball, likewife of two inches diameter, fixed on the extremity of an axis that carries the plate B. Befides the more effential purpofe this ball is intended to anfwer, it is fo loaded within on one fide, that it ferves as a counterpoife to the revolving plate, and enables the axis to remain at reft in any pofition. The other parts may be diftinctly feen in fig. 2. The fhaded parts reprefent metal and the white reprefent varnifhed glafs. ON is a brafs axis, paffing through the piece M, which laft fuftains the plates A and C. At one extremity is the ball D already mentioned; and the other is prolonged by the addition of a glafs ftick, which fuftains the handle L and the piece GH feparately infulated. E, F, are pins rifing out of the fixed plates A and C, at unequal diftances from the axis. The crofs-piece GH, and the piece K, lie in one plane, and have their ends armed with fmall pieces of harpfichord-wire, that they may perfectly touch the pins EF in certain points of the revolution. There is likewife a pin I, in the piece M, which intercept a fmall wire proceeding from the revolving plate B.

The touching wires are fo adjufted, by bending, that when the revolving plate B is immediately oppofite the fixed plate A, the crofs-piece GH connects the two fixed plates, at the fame time that the wire and pin at I form a communication between the revolving plate and the ball. On the other hand, when the revolving plate is immediately oppofite the fixed plate C, the ball becomes connected with this laft plate, by the touching of the piece K againft F; the two plates, A and B, having then no connection with any part of the apparatus. In every other pofition the three plates and the ball will be perfectly unconnected with each other.

Mr. Cavallo's difcovery, fo well explained in the laft Bakerian Lecture, that the minute differences of the electrization in bodies, whether occafioned by art or nature, cannot be completely deftroyed in any definite time, may be applied to explain the action of the prefent inftrument. When the plates A and B are oppofite each other, two fixed plates A and C may be confidered as one mafs; and the revolving plate B, together with the ball

D,

D, will conftitute another mafs. All the experiments yet made concur to prove, that thefe two maffes will not poffefs the fame electric ftate; but that, with refpect to each other, their electricities will be plus and minus. Thefe ftates would be fimple and without any compenfation, if the maffes were remote from each other; but as that is not the cafe, a part of the redundant electricity will take the form of a charge in the oppofed plates A and B. From other experiments I find that the effect of the compenfation on plates op-pofed to each other, at the diftance of one-fortieth part of an inch, is fuch that they require, to produce a given intenfity, at leaft one hundred times the quantity of electricity that would have produced it in either, fingly and apart. The redundant electricities in the maffes under confideration will therefore be unequally diftributed: the plate A will have about ninety-nine parts, and the plate C one; and, for the fame reafon, the revolving plate B will have ninety-nine parts of the oppofite electricity, and the ball D one. The rotation, by deftroying the contacts, preferves this unequal diftribution, and carries B from A to C, at the fame time that the tail K connects the ball with the plate C. In this fituation, the electricity in B acts upon that in C, and produces the contrary ftate, by virtue of the communication between C and the ball; which laft muft therefore ac-quire an electricity of the fame kind with that of the revolving plate. But the rotation again deftroys the contact, and reftores B to its firft fituation oppofite A. Here, if we attend to the effect of the whole revolution, we fhall find that the electric ftates of the refpective maffes have been greatly increafed: for the ninety-nine parts in A and in B remain, and the one part of electricity in C has been increafed fo as nearly to com-penfate ninety-nine parts of the oppofite electricity in the revolving plate B, while the communication produced an equal mutation in the electricity of the ball. A fecond rotation will, of courfe, produce a proportional augmentation of thefe increafed quantities; and a continuance of turning will foon bring the intenfities to their maximum, which is limited by an explofion between the plates.

If one of the parts be connected with an electrometer, more efpecially that of Bennet, thefe effects will be very clearly feen. The fpark is ufually produced by a number of turns between eleven and twenty; and the electrometer is fenfibly acted upon by ftill fewer.

If the ball be connected with the lower part of Bennet's electrometer, and the plate A with the upper part, and any weak electricity be communicated to the electrometer, while the pofition of the apparatus is fuch that the crofs-piece GH touches the two pins; a very few turns will render it perceptible. But here, as well as in the common doubler, the effect is rendered uncertain by the condition, that the communicated electricity muft be ftrong enough to deftroy and predominate over any other electricity the plates may poffefs. I fcarcely need obferve, that if this difficulty fhould hereafter be removed, the inftrument will have great advantages as a multiplier of electricity in the facility of its ufe, the very fpeedy manner of its operation, and the unequivocal nature of its refults.
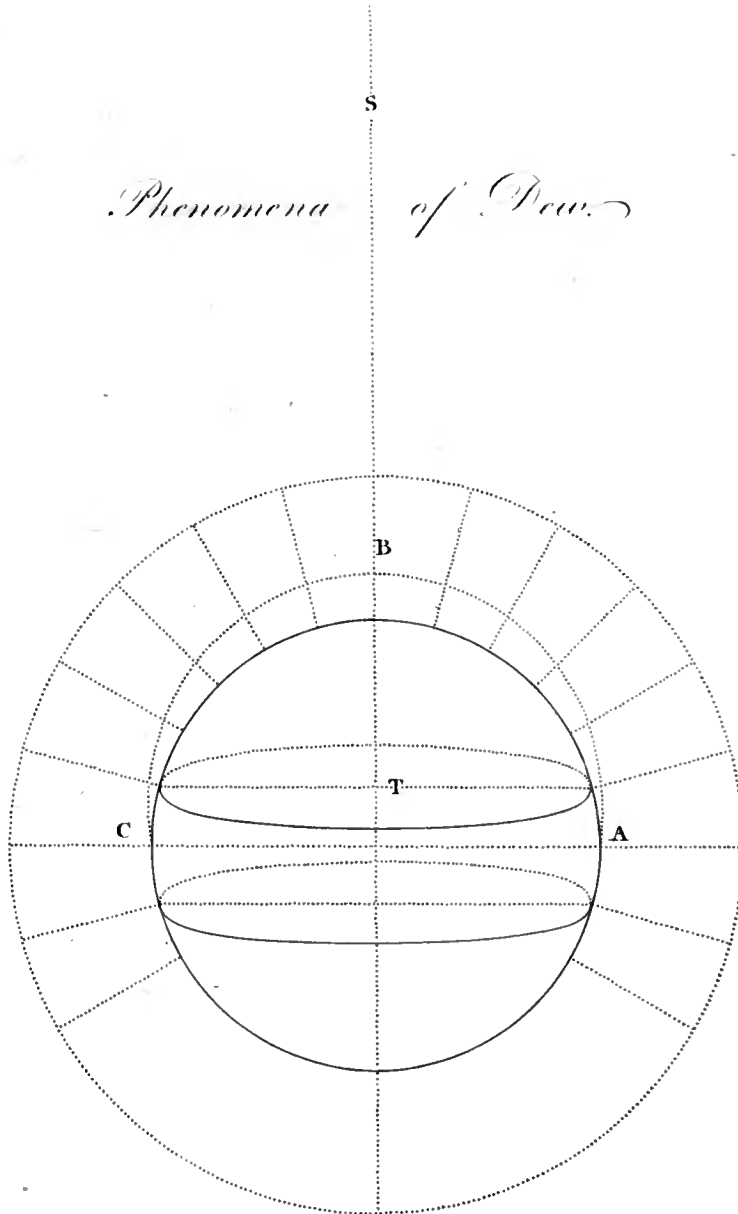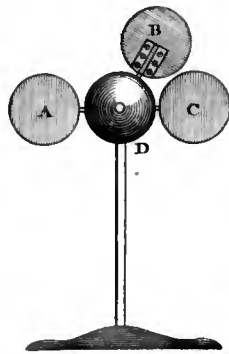
S

Phenomena of Dew.

B

T

C   A

S

Fig. 1.
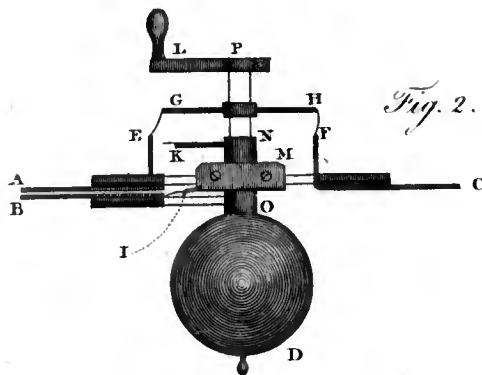
Fig. 2.

ARTICLE I.

Obſervations on the Proofs of the Huttonian Theory of the Earth, adduced by Sir James Hall, Bart. By R. KIRWAN, Eſq. Communicated by the Author.

AS ſome poſitions which I laid down in my examination of Dr. Hutton's Theory of the Earth, may ſeem queſtionable from the ingenious reaſoning employed by Sir James Hall in the third volume of the Edinburgh Tranſactions, to corroborate ſome of Dr. Hutton's aſſertions, and may even be thought inconſiſtent with ſome of the curious reſults that occurred in the highly intereſting experiments inſtituted by the worthy Baronet, and inferted * in the fifth volume of the Edinburgh Tranſactions (a printed tranſcript of which he has had the goodneſs to ſend me), I think it a duty incumbent upon me to examine both the general reaſoning employed by him, and the conſequences fairly deducible from his experiments. Fanciful and groundleſs as the Huttonian theory ſeems to me to be, it may, like the reſearches for the philoſopher's ſtone, be highly uſeful by ſuggeſting new experiments.

In the third volume of the Edinburgh Tranſactions, Hiſt. p. 9, we are informed, that Sir James Hall, though convinced from various obſervations that granite had once flowed in a ſtate of fuſion, yet acknowledged that ſome difficulties accompanied this opinion ;

* Alſo in this Journal, IV. 8. 56.

among

among which the moſt conſiderable appeared to him to be this, that in ſome caſes the felſpar is ſeen in this ſtone with its cryſtals regularly defined, whereas the quartz forms a confuſed and irregular maſs, being moulded on the cryſtals of felſpar, whereas if the granite were formed by fuſion, the very contrary, he ſays, ſhould, it would ſeem, be ex- pected; felſpar being very fuſible, and quartz, on the contrary, highly infuſible. In anſwer to which, hc ſays, " that when quartz and felſpar are mixed and pounded to- " gether, it is well known they may bc melted without difficulty into a kind of glaſs, the " felſpar ſerving as a flux to the quartz, or the felſpar may be conſidered as a menſtruum " in which the quartz is diſſolved; and in this view we may expect by analogy phænomena " ſimilar to thoſe of the ſolution of ſalt in water. Now it is certain that when exceſſive " cold is applied to ſalt water, the water is frozen to the excluſion of the ſalt; why ſhould " not the ſame thing happen in the ſolution of quartz in the liquid. Felſpar, when the " maſs is allowed to cool beneath the point of congelation of the menſtruum? The felſpar " may cryſtallize ſeparately from the quartz, as we have ſeen pure ice formed ſeparately " from the ſalt."

In this anſwer ſeveral particulars deſerve conſideration. In the firſt place, water (to which felſpar is here aſſimilated) is never *regularly* cryſtalized when frozen by exceſſive refrigeration, though, indeed, vapour may; conſequently, ſince in the preſent caſe the felſpar is ſaid to be regularly cryſtallized, the parity does not hold. Again, to juſtify the compariſon of felſpar acting on quartz as a menſtruum, as water does upon ſalt, the felſpar ſhould *always* be in the larger, and quartz in the ſmaller proportion to each other, as water always is to ſalt, and this is, indeed, the commoneſt caſe even where the felſpar is not regularly cryſtalized, yet in Swiſſerland this does not happen, as Mr. Hoepſner atteſts, 4 Helvetic Magaz. p. 266, of which ſpecimens may be met in 2 Leſke Catal. Engliſh edition, p. 375, 376, No. 37, 38, 40, 41; nor in Sileſia, as Gerhard remarks, 1 Grundriſs Min. Syſtem, p. 404 and 405. How then could the felſpar have ſerved as a menſtruum or flux to the quartz in theſe caſes?

3dly, It is allowed by all obſervers, that the caſes in which felſpar in granite is regularly cryſtalized, are exceeding few; ſee Lentz, Emerling, Widenman, &c. Granites, in which ſuch cryſtals are obſerved, are called *porphyraceous granites*, and from that very cir- cumſtance judged by many obſervers not to be ancient granites, but of modern formation; ſee 2 Widenman, p. 1005, in the note. An obſervation ſimilar to that of Sir James Hall has alſo been made by Mr. Beſſon, in Limoges, 29 Roz. Jour. p. 89, for he diſcovered veins of granite in an argillite, though this ſchiſt did not border upon any granitic maſs, and hence he judged it of modern formation. Citizen Dolomieu alſo tells us, that ſuch inſtances had occurred to him in his travels, but he thinks them perfectly diſtinct from the granite which forms granitic mountains, 16 Journ. des Mines, p. 22. Neither was Sauſſure a ſtranger to ſuch granitic veins; but he accounts for their origin very differently from Sir James. §. 600.

4thly,

4thly, Various attempts have been made to fufe granites, in moft of which, as has already been faid, felfpar is the moft abundant ingredient ; but in almoft all the finely pulverized, the quartz remained unfufed, and might be diftinguifhed by a lens ; fee I Sauffure, §. 172, 173, and 174, 1 Gerh. Gefch. §. 51, and in the firft part of his new Mineral Syftem, publifhed in 1797, p. 412, and Hacgueft in 1 Crell Baytrage, p. 34, 35, &c. It is plain, then, that in all heats with which we are acquainted, the felfpar cannot but in very rare cafes ferve as flux or a menftruum to the quartz with which it is found in granites, the full proportion of quartz, which can be rendered fufible by its other component earths, being already contained in the felfpar ; and, in faЄt, there is no analogy betwixt water aЄting as a menftruum on falt, and felfpar aЄting on quartz, for water and falt are fubftances perfeЄtly heterogeneous to each other, whereas felfpar and quartz are both earthy fubftances, of which the former contains a large proportion of the latter, as effential to its compofition, and is fufible only by reafon of its compound nature ; but if the quantity of the quartzy ingredient be increafed, the whole becomes infufible, as I have experienced ; whereas if the proportion of falt in water be increafed, ftill the water will be congealable if confiderably cooled ; moreover quartz frequently bears the impreffion of ftones more fufible than itfelf, which could not happen in any poffible fuppofition, if all had been in ftate of fufion.

Again, Sir James obferved, that a quantity of green glafs, which had been allowed to cool flowly, was found to have loft all its vitreous properties, being opaque, white, and refraЄtory ; but being again melted by a blow pipe, and fuddenly cooled, it refumed its former properties, and became glafs : hence he infers, that if the glafs produced by the fufion of granite had been allowed to cool with fufficient flownefs, it might have cryftalized, producing a granite fimilar to the original, p. 11.

The obfervation on glafs, here mentioned, is perfeЄtly juft, and has been often repeated ; but the analogy betwixt this cafe, and the formation of granite from a complete fufion of its ingredients, is far from being accurate. Glafs confifts of a fimple earth, namely, the filiceous, united to an alkali. To form this union, it is neceffary that the integrant affinity of the filiceous particles to each other fhould yield to the -chemical affinity which the alkali bears to them, and this can happen only in fo high a degree of heat, as confiderably leffens the affinity of the filiceous particles to each other. If, when this union is effeЄted, the compound is confiderably and *rapidly* cooled, yet the union will ftill continue, becaufe the alkaline menftruum being congealed, the filiceous particles cannot move through it to reunite to each other, though their affinity to each other in a *low* temperature be greater than their affinity to an alkali, and thus they continue in that ftate which we call *glafs.* Two experiments fet this explication beyond all doubt, the firft is, that if a folution of falt in water be fuddenly cooled from 140 degrees above to 6 degrees below 0 of Fahrenheit, the whole will be congealed, and no feparation of the falt will take place, fee 8 Nov. Comment. Petropol. p. 346. This cafe is perfeЄtly analogous to that of glafs. The fecond experiment is that of Tromfdorf, 22 Ann. Chym. p. 115, where we

find

find the filiceous particles to have feparated by long ftanding (8 years) from the alkaline in a folution of filicited alkali, and to have formed perfect cryftals hard enough to ftrike fire with fteel.

That the glafs thus formed, being fuffered to cool flowly, fhould be decompofed, is very natural; it is what happens when certain falts, for inftance, nitre, are diffolved in water to faturation, in a boiling heat; if the water be flowly cooled, moft of the nitre will cryftalize and feparate itfelf. That the filiceous earth, thus feparated, fhould be more re-fractory than before, fhould be alfo expected; both becaufe it is not repulverized (at leaft not ftated to have been fo) and becaufe much of the alkali, which is its menftruum, eva-porates, and is volatilized during the flow refrigeration. But if the heat applied be much greater than at firft, it may be vitrified a fecond time, as more of moft falts may be diffolved in a fmall quantity of at 212 than at 150.

But to reproduce granite from a general fufion of all its ingredients by a refrigeration ever fo low, is a very different cafe from that we have juft confidered.

Granite is an aggregate ftone, confifting of quartz, felfpar, and mica; of thefe the moft fufible is undoubtedly the felfpar, and the quartz the leaft: let us then, to indulge the worthy Baronet, fuppofe all three in perfect fufion in a high degree of heat, and after-wards flowly cooled, and thus each (though vouched by no experiment) gradually repro-duced; the quartz, with the exception of the proportion thereof, which enters into the compofition of felfpar and of the mica, would undoubtedly cryftalize firft on the fmalleft diminution of heat, and being congealed in a medium ftill in a liquid ftate, I do not fee why it fhould not form regular cryftals, which, neverthelefs, fcarce ever occur in granite, except in cavities: over this, and after a confiderable interval of time, the mica fhould alfo be regularly cryftalized, and laft of all the felfpar fhould coalefce and congeal (at leaft in the Baronet's fuppofition) in regular cryftals; now as the cryftalizations of thefe three fpecies of ftone take place each at a diftinct portion of time, each fhould occupy alfo a diftinct portion of fpace, the firft fet of cryftals being loweft, the next over that, and the laft uppermoft, as we find to happen when falts of very different folubility, and yet in equal quantity, are diffolved and cryftalized in water, or when fubftances of different degrees of volatility are fublimed by fire. Now among the immenfe maffes of granite that have been obferved and examined in various parts of the globe, not above half a dozen have occurred in which the three conftituent parts of granite were regularly cryftalized, very few in which diftinct layers were feen, and *none at all* confifting of diftinct regular cryftals of each, fuperimpofed on each other. On the contrary, in far the greater number of granitic maffes the three above-named conftituent maffes lie intermixed with each other in the moft confufed and irregular manner, without any appearance of regular cryftalization, in-fomuch, that none can fay, from bare infpection only, which was cryftalized firft and which laft. Nay, granitic maffes not unfrequently occur, in which it is evident that the mica muft have cryftalized contemporaneoufly with the quartz; for in breaking the quartzy part, flakes of mica are found within it. See 6 Sauff. §. 1621.

4   Laftly,

Laftly, I muft add, that even on the fuppofition that diftinct cryftals of quartz, felfpar, and mica, could be produced by fufion, they ftill would be far from refembling thofe we are acquainted with, which effentially contain fome particles of water, as I have elfewhere fhewn.

Perhaps fome may fay, that the fame difficulties occur in accounting for the cryftalization of granite in the moift way; on mature confideration, however, it will readily be feen, that the caufes of coadunition in the dry and moift way are very different, and that their effects fhould alfo be different; for fuppofing the earths that enter into the compofition of granites diffolved in the moift way, their precipitation and imperfect cryftalization may be afcribed to the union they contract with each other, forming maffes of each of the confti-tuent ingredients of granite, which water can no longer hold fufpended; hence the preci-pitation of each of the three fpecies of ftone is nearly contemporaneous, whereas if the formation of thefe ingredients fhould take place in the dry way, it would neceffarily be fucceffive keeping place with the fucceffive diminutions of heat, and then the above-men-tioned confequences would naturally enfue.

The ftate of the granatic ingredients in fufion, which I have above given, agrees pretty nearly with that prefented by Sir James himfelf; he fuppofes the quartz felfpar, fhorl, mica, and garnet, &c. melted together, and the moft fufible of them to be the menftruum in which the reft are diffolved, and that they differ from each other in their properties of folution, as falts differ from each other. Some of them being more foluble in the men-ftruum when very much heated, than when it is comparatively cold, and others may be foluble in it when little warmer than its point of congelation. "If then we fay, for in-"ftance, that the congealing point of the folvent is 1000° of Fahrenheit, and if the folu-"tion is at the temperature of 2000°, we may conceive one portion of the matters dif-"folved, as held by the fimple diffolving power of the menftruum, and another as held by "means of its elevated temperature; when therefore a mafs of this kind is allowed to cool "very flowly, thofe fubftances held in folution by the heat of the folvent, will firft feparate, "and being formed in a liquid, will affume their cryftaline form with regularity." This confequence is truly deduced from the Baronet's hypothefis, but being contrary to fact, difcovers the falfhood of that hypothefis; for if any of the fore-mentioned component parts of granite can be faid to be held in folution by the high heat of the folvent, it is furely the quartz; now the quartz is fcarce ever found regularly cryftalized when forming a compo-nent part of granite, as all mineralogifts atteft, and is a matter of univerfal obfervation.

"But the Baronet continues—"whereas thofe fubftances which were held by the men-"ftruum fimply as a fluid, will not feparate until the congelation of the folvent itfelf takes "place, when the cryftals of the various fubftances will intermix and confound the regu-"larity of form which each would have affumed if left to itfelf. In this manner one of the "common kinds of granite will be produced, confifting of perfect cryftals of fhorl, mica, "or garnet, inclofed in a confufed mafs of felfpar, quartz, and fhorl."—This conclufion

is

is as objectionable as the foregoing; for not to mention that granites, in which shorl, and especially garnets are found, are far from being common assured by shorl and garnet, approach more to the fusibility of felspar, (the supposed menstruum) than either quartz or mica. These, therefore, are those which should crystallize without any regular form in the Baronet's hypothesis, and not the quartz and mica; which is just the contrary of what he himself has observed, for he tells us, p. 9, " he found the crystals of felspar regularly " defined."

Sir James has since, very wisely, declined justifying his theory of the formation of granite by fusion; and by the advice, Dr. Hope very properly applied himself to experiments on various species of *whin*, a denomination which in Scotland comprehends grunstein, basalt, trap, wacken, and porphyry.

Porphyry stones, in which, except the last, none of the component ingredients are found regularly crystalized, and on the last he has made no experiment. The former, he tells us, were softened or fused in a heat of from 38° to 55° of Wedgwood, the glasses to which they were reduced were softened on a range of from 15° to 24°, and the masses of the original stony appearance, to which these glasses, reduced by flow cooling, were softened in degrees of heat from 32 to 45. To the formation of these last he constantly applies the term *crystalization*, and calls them *crystalites*. To the vague term of crystalization I must however object, for as those stones in their original state present no regular crystals, but are at most internally, and imperfectly crystalized, so they must be when reduced from a glassy state, to one resembling their original, and thus discover rather a *nisus* towards crystalization, than perfect crystals, which latter the term crystalization generally applied, would lead us to expect.

Before I proceed to the detail of these experiments, I must observe, that the different fusibilities of these *crystalites*, as he calls them, indicate a very different state from that in which they originally existed; the former requiring a heat of from 32° 45°, and the latter a heat of from 38° to 55°, the reason of which is easily discovered, when the two states are deduced from a different origination, but is in vain sought for when both are to be deduced from one and the same origin.

Passing over the general preliminary accounts of these experiments, which are to be found from p. 7, to p. 10 of this dissertation, I shall now examine the most important particulars of each, as far as they give occasion to any striking observations. In this examination I am much assisted by the ingenious, accurate, and skilfully conducted analyses of Dr. Kennedy, who bids fair to rival the excellence attained by the greatest masters of that sublime and difficult art.

(To be concluded in our next.)

II.

Experiments on the Combustion of the Diamond, the Formation of Steel by its Combination with Iron, and the pretended Transmission of Carbon through the Vessels. By Sir GEORGE STUART MACKENZIE, Bart.

To Mr. NICHOLSON.

SIR,

THE account which I now send you, of some experiments with regard to the properties of the diamond, and the formation of steel, was read at a meeting of the Royal Society of Edinburgh, on the 3d of February last. By giving it a place in your valuable Journal, you will much oblige,

<div align="center">Sir,</div>

<div align="center">Your obedient servant,</div>

<div align="right">GEORGE S. MACKENZIE.</div>

Edinb. 1st. May, 1800.

The phænomena exhibited by the diamond during combustion have been described by various authors *, but the temperature at which this combustion takes place has not as yet been ascertained. I have made several experiments on diamonds with a view both to satisfy myself, as to their appearance while burning, and also to determine at what degree of Wedgwood's pyrometer they begin to be consumed. My experiments were conducted in the following manner †.

A diamond was placed on a thin flat piece of baked Cornish clay, and introduced into a muffle, previously heated red hot. It soon acquired the same redness as the muffle, but in a few moments more, became distinguished by a bright glow ‡. It was then removed, and on examination, its transparency and lustre appeared to be affected. It had a slight milky appearance §, its angles were blunted, and its bulk was also considerably diminished. After being replaced in the muffle, it was consumed slowly, and at last disappeared entirely.

The French chemists take notice of their having observed on diamonds after being subjected to a strong heat, black specks, and a blackishness upon their surfaces. Mitouard says, that in one of his experiments, a diamond had become as black as jet. In the many

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* D'Arcet, Rouelle, Cadet, Lavoisier, &c. Journal de Physique for 1772,-73,-89.

† All the diamonds used in these experiments were cut and polished.

‡ This glow is described by D'Arcet and Rouelle as being more or less bright, in different diamonds, exposed to heat at the same time. This difference, I suppose, was owing to their being nearer to, or farther from the mouth of the muffle, and consequently differently exposed to the action of the air.

§ " Qui est d'une transparence un peu *laiteuse*." Journ. de Ph. Vol. I. for 1772, p. 485.

<div align="right">experiments</div>

experiments detailed in the Journal de Phyfique, this effect was not uniformly produced, and has not been properly explained. I did not obferve any of thefe appearances on diamonds which had been expofed to various degrees of heat between 15° and 30°. There was a fort of luminous haze round the diamond while burning, but fo feeble as to be fcarcely perceptible *.

During the combuftion of a diamond, I placed befide it a fmall piece of plumbago, and obferved, that the latter exhibited a luminous appearance fimilar to that of the former, but it began at a lower temperature. The air being excluded for a few minutes by clofing the mouth of the muffle, the diamond and plumbago both loft their brightnefs; but this foon returned, after the air was allowed to circulate in the muffle, and was much increafed by blowing on them with a pair of bellows.

I expofed a diamond fixed in a fine platina wire to the action of the blow pipe, during which treatment its confumption was extremely flow, owing, I prefume, to the impurity of the air blown upon it.

In order to afcertain at what temperature this fubftance begins to be confumed, I placed a diamond with a pyrometer on a piece of baked clay, and pufhed them gradually into the muffle. As foon as both were perfectly red throughout, the pyrometer was withdrawn, and indicated 13°. The diamond had acquired the dim milky appearance already mentioned. The diamond and pyrometer were replaced in the muffle, and the heat being flowly increafed, till the glow appeared, was continued as equal as poffible, till the diamond was totally confumed: the pyrometer was then meafured, and indicated 14° †. In another diamond, the heat requifite to produce the glow was 15°, and at this temperature it was wholly confumed.

Thefe obfervations were made in prefence of Lord Webb Seymour, Sir James Hall, and Dr. Kennedy.

The experiments above-mentioned were feveral times repeated with different diamonds: and the refult of the two laft fhows, that the heat required for their combuftion is far below what has hitherto been fuppofed neceffary.

The experiment of Guyton, in which the diamond was totally converted into carbonic acid by its combuftion in oxygene gas, has afforded a decifive proof of its identity with carbon. This important difcovery has been farther confirmed by obtaining fteel from the union of diamond with foft iron. I repeated the latter experiment in the following manner.

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* " On a apperçu à leur furface, déja diminuée de volume, une flamme legère, et entierement femblable, pour la couleur, a celle que l'on voit onduler fur une portion de Phofphore expofe a l'air libre." Journ. de Ph. Vol. II. for 1772, p. 114.

† The pyrometers ufed in thefe experiments were made fome years ago by the late Mr. Wedgwood; and are the fame with thofe which he has defcribed in Vol. 76 of the Philofophical Tranfactions of London. I mention this becaufe thofe lately fold at the manufactory, do not agree with the former ftandard.

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Into a fmall hollow cylinder of foft iron, clofed at one end, I put fome diamonds, and having introduced into the cavity above them a ftopper of the fame foft iron, which fitted exactly, the two pieces were rivetted together at the top. The ftopper was nearly in contact with the diamonds, but I did not fill up the intermediate fpace with iron filings, as the French chemifts did, for fear of introducing fome fragments of fteel from the file. Having placed the cylinder in a Heffian crucible, I furrounded it with a mixture of dry fand and clay*. I luted a lid on this crucible, and placed it in another, on which a lid was alfo luted, and a fmall clay cafe containing a pyrometer piece, was attached to its outfide. I wifhed to fubject the iron to a heat not exceeding 150°; for which purpofe I placed the crucibles in a forge. After they were red hot, the fire was raifed, till I thought it had attained the pitch defired, and was then continued as equal as poffible for an hour. The pyrometer indicated 151°.

On opening the crucibles, I found that the upper part of the iron had been melted, and I obferved feveral bright metallic globules adhering to the compacted mafs of fand and clay next the iron. The lower part of the cylinder retained its fhape, but except a portion of the bottom which remained fmooth, was bliftered on the furface. Having polifhed both ends of it, I found that on touching them with diluted nitric acid, they exhibited the fpot which that acid ufually produces on fteel. The fpot on the end which had been fufed, was confiderably darker than that on the other. I cut off the portion which had remained fmooth, and having heated it red hot, and plunged it into cold water, it became fo hard, that no impreffion could be made on it with a file. Several cavities were found within the cylinder, but the diamonds had totally difappeared.

The whole cylinder was thus converted into fteel; one end being reduced to the ftate of caft fteel, and the other remaining in that of cemented fteel; from which it feems probable, that the upper part alfo had paffed into the ftate of cemented fteel before it began to melt. It may therefore be inferred, that diamond may be combined with iron fo as to form fteel by the fimple procefs of cementation.

Mr. Mufhet, of the Clyde Iron Works, has lately publifhed in the Philofophical Magazine, an account of certain proceffes, with a view to prove " That the experiment performed at the Polytechnic School refpecting the converfion of iron into fteel, by means of the diamond, was not conclufive." The manner in which Mr. Mufhet's experiments were conducted, appeared to me to be liable to fome objections; and it was therefore neceffary to afcertain, how far the diamond had contributed to the formation of the fteel in the laft experiment, by expofing part of the iron of which the cylinder was made to the fame degree of heat in fimilar circumftances. Accordingly, a portion of the fame iron was imbedded in fand and clay, and the apparatus was arranged, and the forge managed precifely as in the laft experiment. At the end of an hour, the crucibles being withdrawn from the

* The quartzy fand, called Lynn fand, wafhed and heated red hot--Stourbridge clay.

fire,

fire, and allowed to cool, I found that one end of the cylinder was ſlightly bliſtered ; but otherwiſe had retained its ſhape and original qualities. The temperature was 152°.

I may here once for all obſerve, that I took care to examine whether the iron made uſe of was real ſoft iron, before it was ſubjected to experiment. I likewiſe, in every inſtance, reſerved a part of the iron, as a ſtandard with which I compared that on which the experiment had been made. In this I was aſſiſted by a ſkilful workman, who in my preſence made trials of the hardneſs and malleability of the metal when heated to various degrees of temperature, and alſo when cold, after having been ignited and plunged into water. Whenever therefore, in future, I mention ſoft iron as remaining unaltered after an experiment, I wiſh it to be underſtood, that it was ſubjected to theſe proofs of its malleability, and that it had the ſame qualities with the portion of ſoft iron that had been reſerved.

This reſult affords a proof, that iron expoſed to a heat of 150° during one hour, cannot be converted into ſteel without the aid of carbon ; and that in the firſt experiment, the converſion into ſteel was ſolely to be attributed to the action of the diamond.

Mr. Muſhet's doubt, " whether the diamond afforded even one particle of carbon to the iron," is thus clearly removed. In the paper above-mentioned, he ſtates it as his opinion, founded on his own experiments, that when ſoft iron is expoſed to a high temperature, carbon diſſolved in caloric penetrates the crucibles, and the reſt of the apparatus in which it is encloſed, and converts the iron into ſteel. In order to determine how far this opinion was well founded, I made the following experiments :

My firſt object was to expoſe ſoft iron to ſtrong heat, in an apparatus which might abſolutely exclude all carbonaceous matter, except ſuch as might be ſo diſſolved. This I hoped to accompliſh by making uſe of compact crucibles, and, for greater ſecurity by the interpoſition of ſome ſubſtance between the iron and the crucibles.

On conſidering with Dr. Kennedy what ſubſtance would moſt effectually anſwer this purpoſe, we agreed that pure white felſpar (adularia) would be the beſt ; for Dr. Kennedy, in the courſe of an analyſis of this ſubſtance, having found that it began to paſs into the ſtate of glaſs about 90°, we concluded, that before the heat was of ſufficient intenſity to melt the iron, it would be ſurrounded by a vitreous maſs perfectly free from carbonaceous matter ; and, as we preſumed, impervious to the action of carbon from without. Another reaſon for uſing adularia was, that its action on crucibles is but ſlight, which would prevent any riſk of the experiment failing by their being melted.

For the proceſſes next to be deſcribed, I made ſome ſmall crucibles of the porcelain clay of Cornwall, and baked them in a ſtrong heat. Into one of theſe I put ſome felſpar in fine powder, upon which having placed a ſmall cylinder of ſoft iron, the crucible was filled with the felſpar, ſo that the iron was completely ſurrounded with it. On this crucible I luted a lid of the ſame clay, and placed it in a Heſſian crucible, which was filled up with powdered felſpar. Having alſo luted a lid on the Heſſian crucible, I attached to its outſide a ſmall clay caſe containing a pyrometer piece, and covered the whole with a coating of Stourbridge clay and quartzyſand. The crucible and its contents were then placed

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in an air furnace, and the heat was raifed gradually for an hour. During a fecond hour it was continued at its higheft pitch; after which the apparatus was removed from the fire *. The heat marked by the pyrometer was 152°.

The iron had retained its fhape perfectly, and its properties remained unchanged. The felfpar was reduced to a glafs, perfectly tranfparent and colourlefs, except that it had received a greenifh tinge where it was in contact with the iron. This, I prefume, it derived from a fmall portion of oxide formed on the furface of the iron, by the air unavoidably included in the crucibles.

Wifhing to exclude the felfpar from the iron, I varied the procefs by placing a piece of iron alone in a cornifh clay crucible, luting on a lid, and imbedding this in another crucible filled with felfpar. The heat was continued for an hour and a half. I found that the melted felfpar had penetrated into the interior crucible, and that part of it had reached the iron which ftill remained unaltered.

In order effectually to prevent the felfpar from coming into contact with the metal, I next put a piece of iron into one of the fmall cornifh clay crucibles, and inverted another of a larger fize over it. The two crucibles were luted together with a mixture of Cornifh clay baked in a heat of 160°, and a little of the fame clay in its raw ftate. I placed them in a Heffian crucible, furrounded them with fragments of felfpar, and luted on a lid as before. The heat was managed as in the former experiment, and continued in its greateft intenfity for an hour and a half. I found the fmall crucibles intire, and imbedded in the glafs of the felfpar. The oxide of the iron had acted on the interior crucible, and formed a fmall quantity of brownifh glafs. The iron retained its fhape and foftnefs, and poffeffed none of the properties of fteel.

Mr. Mufhet is of opinion, that carbon diffolved in caloric, penetrates but flowly through earthy bodies. It therefore occurred, that he might object to thefe refults as unfatisfactory, becaufe I had not allowed fufficient time for the carbon to find its way through the thick mafs of crucibles and felfpar in which the iron was imbedded. This I hoped to obviate by the following experiments.

Having placed in a Heffian crucible a piece of foft iron, weighing about half a pound troy, I furrounded it with fand. This crucible was included in two others of the fame kind, and the fpace between each being filled with fand, lids were luted on, and the whole was placed in the reverberatory at Mr. Barker's Iron Foundery, and allowed to remain in its utmoft heat for fix hours. When taken out, I found that the afhes carried up by the flame had vitrified a great part of the crucibles, fo as to form two fmall apertures in the interior crucible which contained the iron; but to my furprize I found the iron unaltered, except that it was flightly oxidated on the furface. The pyrometer was deftroyed.

In a fecond experiment with another piece of iron, I filled the interftices between the crucibles with lute made of fand and clay. The heat was continued for four hours only,

* In all thefe experiments the fuel was coak.

at the end of which I found the outer crucible vitrified, but the interior ones intire. The pyrometer measured 153°, and the iron remained unchanged.

Finding that under the circumstances above-mentioned no carbon penetrated to the iron, I now resolved to repeat Mr. Mushet's first experiment, following exactly the process which he has described; and as he does not inform us what kind of crucibles he used, I took English crucibles, which are less compact than the Hessian.

Mr. Mushet says, that he used sand, obtained by pounding the stone of which the furnace hearths at Clyde Iron Works are made. I am unacquainted with this particular stone; but it is well known, that many sand-stones contain coaly matter. I therefore made use of Lynn sand.

Into an English crucible I put three small cylinders of soft iron, and on this luted a lid made of sand, with a very small proportion of clay. I placed this in a larger crucible containing some sand, and covered it up with the sand, till the outer crucible was almost full. Having adjusted a lid to it, I placed the whole in the air furnace. The fire was managed as in the former experiments, and the greatest heat continued for one hour and a half, when the crucibles were taken out entire.

Part of the interior crucible, and the oxide of the iron, occasioned by the included air had entered into fusion, and formed a small quantity of brownish glass. The three pieces of iron adhered slightly together, and on those parts which had not been in contact with the glass, there was a thin film of oxide, which easily scaled off: but the shape of the cylinders was not altered. On separating them, I found that they retained all the qualities of soft iron. Their softness must therefore have been such as to unite them by a process similar to that of welding.

As Mr. Mushet informs us, that in one of his experiments his crucible was cracked, I conceived that carbon might have found access to the iron through the aperture in the form of carbonic acid, of flame, or of smoke. In order, therefore, to ascertain how far the process was affected by this circumstance, I followed the detail of his experiment precisely; using a cracked crucible, and sand pounded from a sand stone, in which, however, I could discover no traces of coaly matter. The heat in this experiment was continued for an hour and a half. The iron remained unchanged.

With the same view I made a second experiment, which I imagined would be still more decisive. A small shallow crucible containing a cylinder of soft iron, similar to those formerly used, was placed in a larger one, through the sides of which I had bored three holes. In this instance, no sand was introduced into either of them. Having adjusted a lid to the outer crucible, I exposed the whole to the greatest heat that the forge could produce for forty-five minutes; after which the iron was found not to have received any alteration in its qualities.

In the experiments already described, the heat generally exceeded 150, but was not of sufficient intensity to melt the iron, and the results would scarcely have satisfied me, had I

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not found that foft iron, in one folid piece, in fragments, or in filings, may be perfectly fufed without undergoing any change in its properties.

A cylindrical piece of foft iron, about an inch in length, and half an inch in diameter, after being placed on its end in a Heffian crucible, and furrounded with quartzy fand, was expofed to a ftrong heat in a furnace. The upper half of the cylinder loft its fhape and funk down: but the iron retained its original properties.

The fufibility of foft iron is proved in a more fatisfactory manner by the following experiments: five thin flat pieces of foft iron, making when laid together half a cubic inch, foftened into one folid mafs, which ftill poffeffed all the properties of malleable iron.

I took two Cornifh clay crucibles, and in one placed a fmall cylindrical piece of foft iron, which weighed only twenty-five grains, and in the other the fame weight of filings of foft iron. Both were placed, together with a pyrometer, in a Heffian crucible, on which a lid was luted, and the apparatus was expofed to the heat of the forge for three quarters of an hour. Both the cylinder and the filings were melted into buttons. The heat was 158°.

Wifhing to afcertain whether oxide of iron would unite with foft metallic iron when in fufion, and render it brittle, I mixed fome iron filings with a tenth part, by weight, of fcales of iron from a blackfmith's anvil, in the ftate of fine powder, and put the mixture into a Heffian crucible, included within a larger, which I placed in the forge, together with another containing fome of the fame filings without oxide, and a pyrometer. The heat was applied for three quarters of an hour, after which I found a folid button in each of the crucibles. The fufion was fo complete, that in the crucible containing the mixture of filings and oxide, fome of the melted iron had penetrated through a very fmall hole, and remained between the interior crucible and the exterior. The heat indicated by the pyrometer was the fame as in the laft experiment, 158°. The workman with whom I tried the malleability of thefe two buttons, faid, he could difcover no difference between them, and that he thought they confifted of the fofteft and moft ductile iron he had ever examined.

As the iron was here found to be melted at 158°, and as in preceding experiments in the furnace, and alfo in Mr. Barker's reverberatory, pieces of iron did not melt at 153°, it appears that the heat requifite for reducing foft ductile iron to fufion, exceeds 153°, but is not higher than 158°.

From the refult of the foregoing experiments, I hope it is evident, that foft iron cannot be converted into fteel, by carbon penetrating from the fuel through the crucibles. As Mr. Mufhet has not proved by humid analyfis, that his refults contained carbon, I am inclined to think, that in certain circumftances, iron may perhaps be combined with earths, fo as to form a compound in fome degree refembling fteel. The experiments publifhed by Clouet feem to afford grounds for this fuppofition. One of thefe I have repeated, namely, the fufion of iron filings with a mixture of carbonate of lime, and powdered crucibles, and obtained a button which became very hard, when heated red hot and

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plunged into water. But I did not succeed when I employed pieces of nails instead of filings; for the nails remained unmelted and soft. I propose to make some experiments on the fusion of iron with earths, and to analyse the products, in order to learn whether such a combination can take place,

I shall conclude by relating a new experiment, to which I have subjected the diamond, by which the identity of this substance with carbon is still farther confirmed.

Having prepared some pure oxide of iron from a solution of the sulphate, by precipitation with caustic ammonia, I mixed a small quantity of it with one fourth of its weight of diamond powder, prepared in the following manner:

The diamond being reduced to powder in a steel mortar, was boiled in muriatic acid, to dissolve the iron which might have been abraded from it. After proper edulcoration, it was heated in a muffle to burn off the carbon of the steel which remained after treatment with the acid, and which rendered the powder of a grey colour. I observed the coaly matter take fire at the edge of the heap of powder next the strongest heat, and gradually spread itself, till at last the whole appeared as if burning. The glow through the powder ceased soon after, and on removing it I found it perfectly clean and white. From the diminution of the original weight of the diamond, I found that a part of it had also been consumed.

The mixture of oxide and diamond powder was put into a Cornish clay crucible, and exposed to a pretty strong heat for half an hour, after which the oxide was found to be reduced into a metallic button of cast iron.

Another portion of the oxide of iron used in this experiment, was not reduced when placed in the same circumstances without the diamond.

III.

Account of certain Phenomena observed in the Air Vault of the Furnaces of the Devon Iron Works *; together with some practical Remarks on the Management of Blast Furnaces: By Mr. JOHN ROEBUCK, in a Letter to Sir JAMES HALL, Bart †.

S I R,

I HAVE examined my memorandums, concerning the observations I made on the condensed air in the *air vault* of the Devon iron works, near Alloa ; and, according to your request, I now transmit you an account of them ; and also of an experiment I made, when

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* These iron works are on the banks of the river Devon, which runs into the Frith of Forth near Alloa. They are 3 miles from Alloa, and 8 from Stirling.

† Communicated by him to the Royal Society of Edinburgh, and inserted in the fifth Vol. of their Transactions.

It was omitted to be mentioned, that Sir James Hall's paper in our two last Numbers is inserted in the same volume.

<div align="right">a partner</div>

a partner and manager of thefe works, in order to increafe the produce of blaft furnaces.

The two blaft furnaces at Devon are of large dimenfions, each being 44 feet high, and about 13 feet wide in the bofhes, or wideft part, and are formed on a fteep bank, by two pits funk in a very folid ftratum of coarfe grained freeftone.

Thefe pits were afterwards fhaped and lined in the ufual manner of blaft furnaces, with common bricks and fire bricks, and the hearth was laid with large blocks of the ftone that had been dug out, and which ferve the purpofe of fire ftones. At the back of the two furnaces, next the bank, the air vault is excavated, and formed by a mine drove in the folid rock, diftant from the furnaces about 16 feet. The bottom of the air vault is only about 4 feet higher than the level of the bottom of the furnaces. This vault has an aperture at one end to receive the air from the blowing machine, and has two at the oppofite end, one of which receives the eduction pipe, and the other is a door to give admittance occafionally into the vault. As the rock is extremely clofe and folid, the vault is dry, except that a little water ouzes very gently from the fide next the bank in fmall drops, and does not appear to exceed an Englifh pint in 24 hours.

Thefe furnaces are provided with air, or blaft, as it is termed, by the means of a fire-engine of the old, or Newcomen's conftruction. The diameter of the fteam cylinder is 48¾ inches; and the fquare area of its pifton being about 1866½ fquare inches, the power of this fort of engine cannot be rated at more than 7 lb. to the fquare inch, amounting in all to about 13062 lb. This power was employed to work an air pump, or blowing cylinder, of 78 inches diameter, and about 7 feet long. The number of fquare inches on the pifton of the air pump is 4778, and therefore this area, being multiplied by 2¾, will produce 13139, being a refiftance that nearly balances the above-rated power, and fhows that the air, which was expelled from the air pump, could not be condenfed more in the ordinary way of working, than with a compreffing power of about 2¾ lb. on each fquare inch. As the engine was not regulated, at firft, to make a longer ftroke than about 4 feet 8 inches, only one furnace being ufed, the quantity of air expelled at each ftroke of the machine was about 155 cubic feet, which it difcharged through a valve into the air vault, about 16 times in a minute. When two furnaces afterwards were blown, the engine was regulated to work much quicker, and with a longer ftroke. The air vault is 72 feet long, 14 feet wide, and 13 feet high; and contains upwards of 13,000 cubic feet, or above 80 times the contents of the air pump. The top, fides, and bottom of this vault, where the leaft fiffure could be difcovered in the beds of the rock, were carefully caulked with oakum, and afterwards plaftered, and then covered with pitch and paper. The intention of blowing into the vault is to equalize the blaft, or render it uniform, which it effects more completely than any machinery ever yet contrived for the fame purpofe. The air is conducted from the vault by the eduction pipe, of 16 inches diameter, into an iron box or wind cheft, and from this it goes off to each furnace, in two fmaller pipes that terminate in nozles, or blow-pipes, of only 2½ to 3¾ inch diameter, at the tweer of the furface.

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When the furface was put in blaft, after having been filled with coakes, and gently heated for more than fix weeks, the keepers allowed it to have but little blaft at firft, giving it a fmall blow-pipe of about $2\frac{1}{4}$ inch diameter, and likewife letting off a very confiderable quantity of air, at the efcape, or fafety valve on the top of the iron wind cheft, as it is a received though erroneous opinion among them, that the blaft muft be let on very gradually for feveral months. From the conftruction of this valve, it was impoffible to afcertain the exact proportion of the blaft they thus parted with, but I believe it was very confiderable. The confequence was, that the furnace, after it had been in blaft for feveral days, never feemed to arrive at its proper degree of heat, but was always black and cold about the tweer in the hearth, and appeared in danger of choking, or gobbing, as it is termed.

After various experiments tried in vain, by the keepers and the company's engineer, and others, (indeed they tried every thing, except giving the furnace a greater quantity of air, which, as I afterwards afcertained, was all that it wanted), they concluded, that the air vault was the caufe of the whole mifchief; and, to confirm their opinion, they faid they had now difcovered that water was, in confiderable quantities, driven out of the air vault through the blow-pipe, which cooled the furnace; and they infifted, that the power of the engine was fuch as to force water out of the folid rock; fo that this method of equalizing the blaft never would fucceed. The other managing partner was fo much alarmed by thefe reprefentations, that he began to confult with the engineer, and others, about finding a fubftitute for the air vault at any expence.

As the plan of the blowing apparatus had been adopted at my recommendation, and was now fo loudly condemned on account of the water, I had other motives, than mere intereft, for trying to become better acquainted with the phænomena attending it. I accordingly determined to go into the air vault, and to remain inclofed in the condenfed air while the engine was blowing the furnace. It is an experiment that perhaps never was made before, as there never exifted fuch an opportunity. I could not perfuade the engineer, or any other of the operative people about the work, to be my companions, as they imagined that there was much danger in the experiment. Mr. Neil Ryrie, however, one of the clerks of the Devon company, had fufficient confidence in my reprefentations to venture himfelf along with me.

The machine had been ftopped about two hours previous to our entering the vault, and we found a dampnefs and miftinefs in it, which difappeared foon after the door was fhut faft upon us, and the engine began to work in its ufual manner. After four or five ftrokes of the engine, we both experienced a fingular fenfation in our ears, as if they were ftopped by the fingers, which continued as long as we remained in the condenfed air. Our breathing was not in the leaft affected. I had no thermometer with me, but the temperature of the air felt to us the fame as that without the vault. Sound was much magnified, as we perceived, when we talked to each other, or ftruck any thing; particularly, the noife of the air efcaping at the blow-pipe, or wafte valve, was very loud, and feemed

to

to return back to us. There was no appearance of wind to difturb the flame of our candles; on the contrary, I was furprifed to find, that when we put one of them into the eduction pipe, which conveys the wind from the vault to the furnaces, it was not blown out. There was not the fmalleft appearance of any drops of water iffuing out of this pipe. The ouzing and dropping of water from the fide of the rock, next the bank, feemed the fame as before the condenfation was made in the vault. In fhort, every thing appeared, in other refpects, the fame as when we were in the common atmofphere. Having remained about an hour in the condenfed air, and fatisfied ourfelves that no water, during that time, that we could in the leaft difcover, was agitated and forced out of the rock and vault by the power of the blaft, as was imagined and infifted on, we gave the fignal to ftop the engine. As foon as it ceafed to work, and the condenfation abated, and before the door of the vault was unfcrewed, *the whole vault, in a few feconds, became filled with a thick vapour, fo that we could hardly fee the candles at four or five yards diftance.* The door being now opened, the work people, anxious to know our fituation, and what had occurred, came into the vault, and prevented any further obfervations.

I now endeavoured to account for this curious appearance of the water, which only fhewed itfelf occafionally, in very fmall quantities, at the tweer, at a hole I ordered to be made in the bottom of the wind cheft to collect it more accurately, for it never was obferved, but either when the engine, after working flowly, was made to work quicker, or, after having been ftopped for a few minutes, was fet to work again.

I confidered the vapour which we had difcovered in the vault to arife from the moifture of the fide of the rock next the furnace, which being expelled by the great heat of the furnace, and converted into vapour, was able to force its way through the pores of the rock into the vault, but that being in a manner confined within the rock, by the preffure of the condenfed air, it found itfelf at liberty to come into the vault, only when the condenfation abated confiderably, or was totally removed by the going flow, or ftopping of the engine. It alfo occurred to me, that the air, in a ftate of condenfation, might poffibly be capable of holding a greater quantity of water in folution, which might precipitate fuddenly into vapour or mift when the condenfation abated. I imagined, therefore, that the very fmall quantities of water we at times difcovered, proceeded from nothing elfe but this vapour, in its paffage to the furnace along with the blaft, being condenfed into water, by the coolnefs of the eduction pipe and iron wind cheft. The quantity of water did not appear to amount to a gallon in twenty-four hours.

A few days after I had made this experiment, the water ceafed entirely to make its appearance, either at the tweer, or at the hole in the wind cheft; but the furnace did not come into heat for a long while after, and indeed not till the keepers let much more air into it by a larger blow-pipe, and allowed lefs air to efcape at the fafety valve. It is probable that the rock was now become perfectly dry by the continued heat of the furnace.

My experiment had the good effect to remove all the prejudices againft the plan I had adopted of blowing the furnaces, and likewife prevented the other partner from laying out

a large fum of money, by ftopping the works, and altering the blowing machinery. Indeed, it has fince been admitted, by all who have feen it at work, to be the moft fimple and effective method of equalizing the blaft of any yet put in practice.

This experiment led me, fome time afterwards, to apply a wind guage that I contrived, to afcertain precifely the ftate of the condenfation of the air thrown into the furnaces. I found that a column of quick-filver was raifed five inches, and fometimes, though feldom, fix inches, and, in the interval of the engine to receive air into the air pump, it fell only half of an inch. At this time only one furnace was worked. But when two furnaces were in blaft, the engine only raifed the mercurial guage about 4 inches, becaufe the Devon company, for certain reafons, did not, while I continued a partner, think proper to allow the blowing machinery to be completed, by the putting to work their fecond boiler of 20 feet diameter for the fire engine, according to my original defign, which, by adjufting the machinery, would have enabled us to blow two furnaces, with two boilers, with as much effect, in proportion, as one furnace with one boiler. This inftrument had the advantage of enabling the work people to difcover the real power of their blaft, and know the exact condition of the air valves, and the gearing of the blowing pifton; for if thefe were not tight, and in order, (although the engine might, to appearance, be doing well, by making the fame number of difcharges of the air pump as ufual *per* minute), yet the wind guage would not rife fo high, and would fhew that there was an imperfection fomewhere, by reafon of a quantity of air efcaping at the valves, or pifton, that could not fo eafily otherwife be known. This contrivance was confidered as of much ufe, and was afterwards always quoted in the company's journal books, to fhow the actual ftate of the blowing machine, in comparing the daily produce of the furnaces.

I hope you will not think me tedious, when I explain to you another experiment, which appears to me to be of confiderable importance to all manufacturers of caft iron.

I had reafon to conjecture, from my own obfervations on the effects of blowing machinery on blaft furnaces, as well as from the knowledge I had acquired from my father Dr. Roebuck, and from my communications with other experienced iron mafters, that a great part of the power of fuch machinery was mifapplied in general practice, by throwing air into furnaces with much greater velocity than neceffary, and that, if this velocity was, to a certain degree diminifhed, the fame power, by properly adjufting the blowing machinery, of whatever nature, would be capable of throwing into the furnace a proportionally greater quantity of air. For, " *Since the quantities of any fluid, iffuing* " *through the fame aperture, are as the fquare roots of the preffure;*" it follows, that it would require *four* times the preffure, or power, to expel *double* the quantity of air, through the fame aperture, in the fame time: but if the area of the aperture was doubled, then the quantity of air expelled by the fame power, and in the fame time, would be increafed in the ratio of the fquare root of 2 to 1, though its velocity would be diminifhed exactly in the fame proportion. Again: I confidered that the quantity and intenfity of heat, produced in blaft furnaces, and confequently its effects in increafing the produce, might be

4   only

only in proportion to the quantity of air decompofed in the procefs of combuſtion, without regard to its greater velocity; that is to fay, whether or not the fame quantity of air was forced, in the fame time, into the furnace through a fmall pipe, or through one of larger dimenfions; for, in attending to the procefs of a common air furnace for re-melting of iron, where there is a very large quantity of air admitted through the large areas between the bars, it is well known, that a much greater intenfity of heat is produced than takes place in a blaſt furnace, and yet the air does not enter into the fire through the bars with increafed denfity or great velocity. I therefore thought it probable, that in-creafing the *quantity* of air, thrown into the blaſt furnace in a confiderable degree, although the *velocity or denfity* might be much lefs, would have the effect of increafing its heat, and operations, and produce. And as, from the principles above ſtated, with regard to the machinery, I faw I could greatly increafe the quantity of air thrown into the furnace, by enlarging the diameter of the blow-pipe, and regulating the engine accordingly, without being obliged to employ more power, I was anxious to make this experiment.

A fyſtem of management, of which I did by no means approve, was adopted by the other partners of the Devon company, foon after the works were begun to be erected; and, in the profecution of it, they ordered their fecond furnace to be put in blaſt, without per-mitting thofe meafures to be taken that were neceffary to provide and maintain a fufficient ſtock of materials; and alfo without allowing their blowing machine to be completed, according to the orignal defign, by the addition of its fecond boiler. As might have been expected, a trial of feveral months to carry on two furnaces, with only half the power of ſteam that was neceffary, and an inadequate ſtock of materials, proving unfuccefsful, the company, as a remedy, inſtead of making up the above deficiencies, ordered one of the furnaces to be blown out, and ſtopped altogether. This improper meafure, however, afforded me the opportunity of immediately putting in practice the plan I have men-tioned.

When one of the furnaces was ſtopped, the other continued to be blown by a blow-pipe of 2¾ inches diameter, and the produce of the furnace, for feveral weeks thereafter, was not 20 tons of iron *per* week at an average. The engine at this time was making about 16 ſtrokes a minute, with a ſtroke of the air pump, about 4 feet 8 inches long; but when I altered the diameter of the blow-pipe, firſt to 3, and immediately after to 3¼ inches diameter, and regulated the working gears of the engine, fo as to make a ſtroke of 5 feet 2 inches long, and about 19 ſtrokes in a minute, on an average, the produce was im-mediately increafed. It continued to be, on an average of nine months immediately after this improvement, at the rate of 33 tons of iron *per* week, of as good quality as formerly; for during this period, from the 21ſt November 1795 to July 30, 1796, this one furnace yielded 1188 tons of iron. No more coals were confumed in working the blaſt engine, or other expences about the blowing machine incurred, and therefore no more power was employed to produce this great effect. It is alfo of much importance to remark, that the

Q 2

confumption of materials, from which this large produce was obtained, was by no means fo great as formerly. The furnace required very confiderably *lefs fuel, lefs ironſtone, and lefs limeſtone,* than were employed to produce the fame quantity of iron by the former method of blowing; and according to the ſtatements made out by the company's orders, as great a change was effected in the œconomical part of the bufinefs.

From the fuccefs of this experiment, fo well authenticated, and continued for feveral months, I am led to be of opinion, that all blaſt furnaces, by a proper adjuſtment of fuch machinery as they are provided with, might greatly and advantageoufly increafe their produce, by affuming this as a principle, viz. " *That with the giving power it is rather by a great* " *quantity of air thrown into the furnace, with a moderate velocity, than by a lefs quantity* " *thrown in with a greater velocity, that the greateſt benefit is derived, in the ſmelting of iron-* " *ſtones, in order to produce pig-iron.*" However, it is by experiment alone, perhaps, that we can be enabled to find out the exact relations of power, velocity, and quantity of air requiſite to produce a *maximum* of effect *.

But, an unfortunate difagreement among the partners of the Devon company, put it out of my power to make further progrefs in this matter, by laying me under the neceffity, two years ago, of withdrawing myfelf entirely from the concern.

<div align="center">

I have the honour to be, refpectfully,

SIR,

Your moſt obedient fervant,

</div>

Edinburgh, June 30, 1798.　　　　　　　　　　　　　　JOHN ROEBUCK.

To Sir James Hall, Bart.

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* If Q be the quantity of a fluid, iffuing in a given time through an aperture of the diameter D, V its velocity, and P the power by which it is forced through the aperture: then the area of that aperture being as D^2, the quantity of the fluid iffuing in the given time will be as VD^2, or $VD^2 = Q$.

Again, this quantity multiplied into its velocity, will be as the *momentum* of the fluid expelled, or as the power by which it is expelled, that is, $V^2 D^2 = P$, or $VD = \sqrt{P}$.

Here, therefore, if D is given, V is as $\sqrt{P}$, as Mr. Roebuck affirms. Alfo, becaufe $V = \dfrac{Q}{D^2}$, and alfo $V = \dfrac{\sqrt{P}}{D}$, $Q = D\sqrt{P}$, fo that, while P remains the fame, Q will increafe as D increafes, and V will diminifh in the fame ratio.

The problem, therefore, of throwing the greateſt quantity of air into the furnace, with a given power, ſtrictly ſpeaking, has no *maximum*, but the largeſt aperture of which the engine can admit muſt be the beſt. It is probable, however, that there is a certain velocity with which the air ought to enter into the furnace; this will produce a limitation of the problem, which, as Mr. Roebuck fuggeſts, is not likely to be difcovered but by experiment. J. P.

In order to illuftrate what is faid above, a ground plan of the air vault and furnaces of the Devon Iron Works is given in Plate V.; of which the explanation follows :

Explanation of Fig. 1. Plate V.

A The air vault, formed by a mine drove in the folid rock of coarfe grained freeftone.

B The blowing cylinder.

C The pipe that conveys the air from the blowing cylinder to the air vault.

D The eduction pipe that carries the air from the air vault to the iron wind cheft.

E The iron wind cheft, (about $2\frac{1}{2}$ feet cube), in which is inferted a wind-gauge, reprefented in fig. 3.

F F The two blow pipes for each furnace, which terminate in apertures of $3\frac{1}{4}$ inches diameter at the tweers of the furnaces.

G G The two blaft furnaces, placed in two pits funk in the folid rock.

H H The timps of the furnaces from whence the caft-iron is run off into the cafting room, L L.

O The door to give occafional admittance into the air vault.

M The excavation, in which is placed the blowing machine.

Explanation of Fig. 2.

A The end of the wind-guage, (about 12 inches long), which is open to the atmofphere, being half filled with quickfilver.

B The end that is inferted in the iron wind cheft, and expofed to the preffure of the condenfed air of the air vault.

IV.

Apparatus for making the Hydraulic Engine at Schemnitz work itfelf without Attendance. By Mr. JOHN WHITLEY BOSWELL.

To. Mr. NICHOLSON.

SIR,

May 7, 1800.

HAVING laft fummer met with a defcription of an engine, fimilar in its principles to the hydraulic machine of Schemnitz, (of which there is an account in the Number of your Journal *, in which you did me the honour to infert my communication of a new ventilator); I was induced to contrive a method for making it work itfelf, as well from the utility of fuch an invention, as from its being obvious at firft fight that it was very practicable.

* Vol. IV. p. 18.

From

From the intereſt you ſeem to take in the above-mentioned engine, I conclude it will not be unacceptable to you to have a deſcription of this method of ſaving the labour at preſent employed in working it, and therefore take the liberty to ſend you a drawing, in which all the veſſels are ſuppoſed to be open at one ſide, that the diſpoſition of the pipes within may be more clearly ſeen.

Simplicity of form, and facility of execution, have been chiefly aimed at in this conſtruction; and I have no doubt but it muſt perfectly anſwer the intention, or I would not trouble you with it. If, however, you ſhould be of a different opinion, it will give me pleaſure to be ſet right by you.

DESCRIPTION: Plate VI.

A. The reſervoir, or upper level of water.

B. A chamber made of ſufficient ſtrength to bear the internal preſſure of a column of water of the height of A above it, multiplied by its own baſe.

C. A chamber of the ſame ſtrength as B, but of a ſmaller ſize; it is placed at the bottom of the pit from which the water is to be raiſed, and under the level of the water.

Theſe chambers would be ſtronger with the ſame materials, if of a globular or cylindrical form; but the ſquare ſhape is uſed in the drawing, merely for the facility of repreſenting the poſition of the parts.

D. A pipe, from the reſervoir A which paſſes through the top of B, and ends near its bottom, to convey water from A to B.

E. A pipe from the top of B to the top of C, to convey air from B to C.

F. A pipe from the bottom of C to the level of the ground at the top of the pit, to carry off the water from the pit.

G. A pipe from the bottom of B to carry off the water from it.

H. A veſſel to contain the water uſed in working the cocks; it is only placed on the top of B to ſave the conſtruction of a ſtand on purpoſe for it.

I. A cock, or moveable valve, (worked by the lever there repreſented,) in the large pipe D.

K. A ſtop cock in the ſmall pipe which conveys water from D to H. Its uſe is to make the engine work faſter or ſlower, by letting water more or leſs quick into H; or to ſtop it altogether from working when required.

L. A moveable valve, or cock in the ſmall pipe L K. The lever which works it is connected by a ſtrong wire with the lever which works I, and is balanced by a weight at its oppoſite extremity, ſufficient to open both thoſe cocks and ſhut N, when not prevented by a counter weight.

N. A cock in the pipe G to open and ſhut it as wanted.

O. A ſelf moving valve in the pipe F, which permits the water to paſs upwards, but prevents its return.

P. A ſelf-moving valve at the bottom of C, which permits the water to paſs into C, but prevents any from paſſing out of it; it is furniſhed with a grating to prevent dirt getting in.

R. A

R.   A veſſel ſuſpended from the levers of I and L, capable of containing a weight of water ſufficient to ſhut them.

S.   A veſſel ſuſpended from the lever of N, it muſt contain water enough by its weight to open N : it is connected by a chain to R, to keep it down as long as N is open.

T.   A ſyphon paſſing from the bottom of H, near its upper edge, and down again to the mouth of R.

V.   A ſelf moving valve, of a ſufficient levity to riſe, when the water in B comes up to it, and cloſe the pipe E ; into which no water would elſe paſs from B.   A ball-cock, ſuch as uſed in common water ciſterns, would alſo do here.

X.   A ſyphon from the bottom of R riſing within an inch of its top, and paſſing down again to the mouth of S.

Y.   A ſmall pipe at the bottom of S ; this may have a ſtop-cock to regulate it, which when ſtopped will alſo ſtop the engine.

EXPLANATION.

The mode of this engine's working is as follows : ſuppoſe the veſſels V H R and S empty of water, and the cocks K and Y open, and the veſſel C full of water.   The weight on the lever of L will then open the cocks L and I, on which the water from A will flow into B and H.   As the water riſes in B it will force the air through E into C, which ſtrongly preſſing on the water in C will force it up through the pipe F, till the water in B riſes to the level of V and cloſes it, at which time H will be full of water, (the quantity flowing in being ſo regulated by the cock K) and the water will flow from it through the ſyphon T into the veſſel R, which as it fills ſhuts the cocks I and L, and prevents any more water coming into B and H.   When R is full, the water flows through its ſyphon X, which fills S, and by it opens N, which empties B of water, and keeps N open as long as there is any water in H.

When H is empty, B will be ſo too (being ſo regulated by the cock K) on which in a moment or two R and S will alſo be empty ; which will cauſe the cocks I and L to open, and all things be again in the ſtate firſt ſuppoſed, for a repetition of the operations deſcribed.

To ſtop the engine the cocks at K and Y ſhould be ſhut, while S is full of water.   To ſet it working they ſhould be opened, and this is all the attendance it will require.   As no one but an engineer ſhould attempt to conſtruct ſuch an engine as this, it was uſeleſs to repreſent the manner of connecting the pipes by flaches, or otherwiſe, or the proper methods of faſtening and cloſing all parts, which are all well known to ſuch as have made this art their ſtudy.   If this method of effecting the purpoſe ſhall be eſteemed worthy of being adopted by any of them, it will give me great pleaſure to have contributed to the improvement of an engine ſo uſeful, where it can be adopted.

Your very humble Servant,

JOHN WHITLEY BOSWELL.

P. S.

P. S. I beg leave to requeſt that an error of the preſs in the paper on Ventilators, may be corre&ed, page 7, line 16 and 17, for "and this way of uſing it might be made" read " and *for* this way of uſing it, *it* might be made."

I alſo will thank you to notice, that my ventilator was conſtru&ed in March 1798, before Venturi's book came to England, which I underſtand was in November 1798. Mr. Bonnycaſtle, of Woolwich, to whom I ſhewed it, and its mode of a&ion at that time, can no doubt recolle& this circumſtance.

J. W. B.

V.

On the Chemical A&ion of the different Metals upon each other at the common Temperature of the Atmoſphere, and upon the Explanation of certain Galvanic Phenomena. By M. FABBRONI *.

THE phenomenon ſpoken of by Sultzer, in his *Theorie des Plaiſirs*, publiſhed in 1767, has been claſſed among galvanic phenomena, that is to ſay, the myſterious ſenſation which is manifeſted on the tongue by the conta& of two metals in mutual conta&, which would have excited none if they had been ſeparately applied to that organ. In fa&, I have been perſuaded, that this very principle which produces an unexpe&ed taſte in this caſe, may alſo produce a convulſive contra&ion in the animal fibre, when it comes to touch the irritable and ſenſible parts uncovered at the ſame time. But ſo far from attributing theſe effe&s with all the world to an agent almoſt unknown, ſuch as the ele&ric fire, I immediately ſuppoſed them to depend upon a mere chemical operation, in the ſame manner as probably the ſenſe of taſte itſelf is produced, which renders the mechaniſm much more intelligible. I made refle&ions, and inſtituted experiments on this curious ſubje&, of which I gave an account to the Academy of Florence in 1792. The volume has not yet been printed; but I think the ſubje& is ſpoken of by Brugnatelli in his Journal. I have neither his abridgment, nor my own memoir at hand; I ſhall therefore repeat in this place nothing but what remains clearly in my recolle&ion.

I had before obſerved on many occaſions, that running mercury retains its metallic ſplendor a long time while alone and uncombined; but that its amalgam with any other metal whatever is ſoon tarniſhed or oxided, and increaſes in weight in proportion to that progreſſive change.

I have kept, for many years, fine tin without any alteration in its argentine appearance, but this was not the caſe with regard to the different alloys I had made with the ſame metal for economical ſpeculations.

* Tranſlated from the Journal de Phyſique VI. new Series, p. 348.

I had

I had feen in the mufeum at Cortona, Etrufcan infcriptions engraved upon pure lead, which are ftill in perfect prefervation, though of the moft remote antiquity; and on the contrary, I had found with furprize in the gallery of Florence, medals of lead of the different pontiffs, in which tin, and perhaps arfenic had been mixed, to render them more beautiful and folid, were intirely reduced to a white powder, or changed into oxide, notwithstanding they had been wrapped in paper, and kept in drawers.

I had likewife obferved, that the alloy made ufe of in foldering the plates of copper which cover the moveable roof of the Obfervatory at Florence, was fpeedily changed, and converted manifeftly into a white oxide at its extreme contact with that metal.

Laftly, I had learned in England, that the iron nails formerly ufed in attaching the copper to the bottoms of fhips, corroded this laft metal fo much by their contact, that the holes foon became dilated, fo as to exceed the fize of the head of the nail itfelf.

Thefe facts appeared to me to be amply fufficient to fhew, that the metals exercife a mutual action in this cafe, and that the caufe of the phenomena produced by their union, or contact, was to be attributed to this action.

It is known, that the metals are in general fufceptible of combining together by mutual folution. We may therefore form a notion, that their tendency to mutual combination begins as foon as the particles are brought into contact. It is only by virtue of the immenfe fuperiority of their force of cohefion, that they are prevented from mutual penetration and folution in the cold. Fire is neceffary to difunite and give mobility to their particles. We fee this happen in amalgams which are formed without heat; and it is known, that in the manufacture of tinned plates, the tin penetrates the iron without this laft metal being liquified. It is, probably, the fame force of cohefion which fometimes prevents the oxidable metals from attracting oxigen with fpeed. If a rapid movement tends to difaggregate the particles of mercury in the midft of water, nothing more is required to enable it to affume the principle of oxidation in a very fhort time, by attracting it from the fluid. Thefe facts, as well as many others of the fame nature, no lefs common than well known, ought to have proved to philofophers, that the metals by exercifing their mutual attractive force, muft by the fame energy diminifh their refpective powers of aggregation; that though neither of them feparately may be able feparately to attract oxigen from the atmofphere, or from water, they may acquire that power by fimple mechanical touch, as they pafs to new combinations. We might therefore fufpect, that fome, at leaft, of the effects produced on the bodies of animals, by the application of metallic coatings to the nerves and the mufcles, may be attributed to a chemical operation; to the tranfition of oxigen into a combination; to the formation of a new compound; or to the developement of a foluble or fapid tafte, which is fo perceptibly manifefted on the organ of that fenfation.

Galvani, Aldini, Volta, and other philofophers equally fkilful, who have fo fuccefsfully directed their attention to this kind of refearch, not reflecting that the chemical action exerts itfelf with the fwiftnefs of lightning, and, furprized at the fuddennefs with which the two different metals exert their effects on the animal fibre, were of opinion, that it

could only be attributed to the electric fluid. The transmission of galvanism to a distance, and by a circuit, favored their notion, which was generally received, notwithstanding the very strong objections which might, in some cases at least, be opposed to their system. In fact, some signs of electricity have been observed on the separation of two metals, which had been before placed in contact; but it is very well known, that even several chemical operations are constantly attended with a change of equilibrium in the electric fluid, and consequently by perceptible marks of electricity. Thus it is that flashes of lightning are seen in the great volcanic fusions or eruptions; and this also is one of the cases in which philosophers have taken that for the cause of these fires, which is merely one of their effects. The signs of electricity may be produced by melting a small quantity of sulphur, or of chocolate, or even by boiling or evaporating water; but assuredly these are fusions, and are not caused by the electricity. I do not pretend to exclude all electrical influence from the prodigious effects of galvanism. I propose merely to prove, that this principle is not concerned in the phenomena of Sultzer, and that various other similar facts are derived from the same source.

The metals having a mutual affinity, their particles must mutually attract each other when they are brought into contact. We cannot estimate the magnitude of this force; but I think it is sufficient to weaken the force of their aggregation, so as to dispose them to engage in new combinations, and to yield more readily to the action of the weakest solvents.

I had observed in repeating the experiment of Sultzer, that if I wiped my tongue as accurately as possible, the sensation excited by the approach of the two metals in contact was diminished, so as to be hardly distinguished. The saliva, or some other moisture, must therefore be of some importance in this phenomenon. But to ascertain the truth of my supposition, I placed in different goblets filled with water,

1. Separate pieces of metal; for example, in the one gold; in another silver; in a third copper; and in others tin, lead, &c.

2. In other similar goblets I put the same metals as before, but two pieces in each goblet, one of which metals was more, and the other less oxidable. They were separated from contact by a small slip of glass.

3. Lastly, I placed in other goblets metals of different kinds, but two in each, and in immediate contact. The two first setts exhibited no perceptible change, but in the last the most oxidable metal was visibly loaded with oxide, a few moments after having been in contact with a different metal. This oxide gradually increased, so as to hang beyond the lower metal, uniting with it in a mass, and flowing in a cascade along the whole of the sides. This phenomenon commences, though insensibly, at the very instant of contact; but I left the metals of this experiment for a considerable time, to see what would happen. I examined them at the end of a month, and found that the two metals had contracted so considerable an adhesion, that in order to detach a piece of brass about two centimeters broad from a plate of tin, a force of no less than two kilogrammes was necessary.

I afterwards

I afterwards obferved, that feveral metals were not only charged with oxide, but that fmall faline cryftals of different figures were formed. It appeared therefore to me, that an evident chemical action had taken place, and that it was unneceffary to feek farther for the nature of the new ftimulus, which in the experiment of Sultzer has been called galvanifm. It was manifeftly a combuftion or oxidation of the metal; the ftimulating principle might therefore be either the caloric, which is difengaged, or the oxigen, which paffes to new combinations; or laftly, the new metallic falt: but which of thefe it may be I have not well afcertained. I have in fome inftances coloured the water, in which I placed the metals with tournfol; but I have remarked no other effect, than a precipitation of that colouring fecula, without any change of its colour. I have remarked, that the water in which this experiment is made, contracts a flight metallic, or as I may fay, arfenical tafte; which lafts for fome time, and produces a difpofition to fpit; but it does not appear to contain enough of metal to be detected by the moft fenfible chemical re-agents. I have therefore thought it proper to limit my conclufions refpecting this phenomenon, that it is merely a flow combuftion of the metal, which muft be accompanied with an attraction of oxigen, and a developement of light and caloric. It is well known that when a metal, fuch for example as gold, is amalgamated with mercury, there is an immediate expulfion of caloric, not perhaps in confequence of the folid ftate of the mercury, but becaufe the diminution of the force of aggregation in the particles of the latter metal facilitates its combuftion. The progreffive augmentation of weight which is obfervable in amalgams, arifes only from the oxigen which they attract from the atmofphere. I attempted in vain to meafure the quantity of caloric, which is developed by the contact of two folid metals, whatever may be their weight. This quantity is too fmall and too diffufed, as it were, over a large furface, to be afcertained by inftruments of fo little delicacy as thofe we poffefs. Neverthelefs, the light which flows from this metallic combuftion, may be feen if the eye forms part of the apparatus. For example, it is only requifite to hold a piece of filver in the mouth, and apply a fmall piece of tin upon the ball of the eye; as foon as thefe two metals are made to communicate directly, or even by means of a third metal, a faint, but very diftinct light is perceived, which is not an electric fpark, nor a convulfive irritation. For though this light appears to affect the organ at the firft inftant only, becaufe the eye foon becomes accuftomed to this weak fenfation, we may be affured, that the emanation of light is continual in this cafe, by fliding alternately the tranfparent and the opaque cornea againft the metal, when it will be feen that the light is ftronger whenever the metal is touched by the more tranfparent part of the organ. And again, if this experiment be made as it ought in the dark, nothing more will be neceffary, than to attend at the time when the communications between the two metals is interrupted, and the obfcurity will be more deep and perfect, which is a proof of the conftant prefence of fome light before that interruption. I do not fpeak of that kind of flafh which fome perfons affirm they have feen, by applying the two metals fimply to the tongue and the gums, without the eye forming part of the circuit. I have not been able to verify this fact on myfelf, and have remarked, that

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feveral individuals have affirmed, that they faw what others could not perceive, and that the action in all was a convulfive fenfation, or delufive appearance, like the light which is feen when the eye is preffed with the finger, or when a blow is given in the vicinity of that organ. It appears, therefore, that the fenfation of tafte, and the emanation of light, are in this cafe the refults of a chemical operation. But thofe who have been defirous of attributing the whole to electricity, have not been in want of plaufible obfervations to juftify their hypothefis. It has been remarked, for example, that the fenfation here defcribed, is felt even when the communication is made by a chain, or a long metallic conductor. But it is known, that electricity is propagated by this means to an indefinite diftance; and I have obferved, that fix or feven metres are the extreme limit to which the manifeftation of the metallic action on the tongue or the eye can be extended. It is certainly at the precife point of contact of the two metals, that their mutual action is the ftrongeft, and it is natural to think, that the particles which are the moft affected, muft communicate from the one to the other in their vicinity, to a certain point, the difpofing force which they have received, It muft propagate itfelf with diminifhed force, like the circles impreffed in ftagnant water by the fall of a body; and the limit of its action is nearly that which I have pointed out.

By varying my experiments in feveral manners, I obferved, that if I covered the water in which the two metals in contact were placed, with a thin coat of oil, the oxidation was very flight, and ftopped at a certain point. But this affuredly did not fo happen, becaufe the intervention of a non-conducting fluid had oppofed the completion of an electric phenomenon, as it may appear at firft fight; for I attempted to keep up the communication of the water and the metals with the common ftock, by plunging a metallic conductor beneath the oil, and the combuftion was not continued more than before. The fame interruption or limitation obtains, if the free contact of the atmofphere be excluded, by means of a fmall bell glafs reverfed over mercury, which does not oppofe the paffage of electricity. Befides which, the Galvanifts believe, that their phenomena do not depend on univerfal electricity, but on the fpecific electricity, as it is called, of the different metals. If this were the cafe, it would be difficult to conceal, why the effect fhould not be produced at the very inftant of contact, in the fame manner as it happens when two charged bottles of oppofite electricities are brought into contact. And again, nothing would prevent the continuation of the phenomenon when the two metals came into contact, whatever might be the circumftances. I am well aware, however, that if it may here perhaps be objected to me, that if the two metals acquire the faculty of decompofing water by their fimple contact, and the mere difpofition of their attraction, or mutual affinity, a flight coat of oil, or inverted glafs, ought not to oppofe the continuation of their complete oxidation, while they continue furrounded by that element. I have remarked, that the free contact of the atmofphere is neceffary to this phenomenon; becaufe it is neceffary that the water fhould contain that portion of oxigen gas, which it always holds after having remained for a certain time in contact with the air. It is neceffary in my apprehenfion,

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that the hydrogen of the water fhould be in a ftate of quartation, or overcharged with oxigen, in the fame manner as gold with filver, in order that the folution or parting may take place. The contact of the atmofphere is therefore neceffary to the water in the goblet, in order that it may re-affume the neceffary ftate of quartation, by abforbing oxigen gas from the atmofphere, in proportion as the oxidation of the metal feizes the radical of that principle.

If the experiment be made in a calm air, a kind of pellicle may be obferved at the furface of the water, immediately over, and of the fame figure and fize as the metal, beneath which ftreams even indicate the points of the furface and columns of the water, through which the atmofpheric oxigen has been tranfmitted. This re-abforption is fo true, that if in fome cafes a metallic oxide, well charged with oxigen, be fubftituted inftead of the contact of the atmofphere, the combuftion of the metal under experiment is effected with facility. It is known that iron decompofes water without addition, though very flowly; but if the red oxide of lead be added at the bottom of the water, the iron becomes changed into the black oxide without decompofition of the water.

I have obtained, though after a very long time, the oxidation of tin in water in contact with filver, in a bottle of flint glafs filled with water, and almoft hermetically clofed. But I have remarked, that the lead which enters into this kind of glafs, yielded its oxigen to the tin, and became changed into a black opaque oxide, in the fame manner as happens when a bottle of flint glafs, filled with hydrogen gas, is made red hot in the midft of charcoal. The hydrogen is burned, and feizes the oxigen of the lead, which it revives, as the tin operated in the cafe before-mentioned.

It appears evident, therefore, that the experiment of Sultzer is nothing more than a combuftion or chemical operation, as is proved not only by its refult, but its duration. For electricity acts always inftantaneoufly, whereas the effects of the chemical affinities continue as long as the re-agents are not faturated. I left for a very long time in water, pieces of filver wrapped in feveral folds of tin foil. I took out fome of them at different periods, and found the progrefs of the combuftion exactly proportional to the time. In thofe which I took out the laft the tin was corroded, and pierced through all its folds, as if it had been plunged in an acid. But if other proofs be required, to fhew that electricity has no part in the phenomenon in queftion, we might vary the experiments, fo as not to hinder the effects of the electric fluid, and evidently to prove by the eyes, that the combuftion which happens depends on the difpofition of the metals, and their chemical affinity.

For example, if a piece of tin of confiderable thicknefs be placed on the eye, and touched at its oppofite furface with a bar of filver, there is no decompofition of water, nor combuftion, nor light, and yet the contact of the two metals ought to produce thefe fenfible effects, if they depended on the communication of their electricity. 2. If a piece of tin be held upon the eye, and another piece in the mouth, and a communication be made between them by a bar of filver, there is no more appearance of light than in the former experiment. 3. If a piece of gold be applied on the eye, one of filver on the tongue, and

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a communication be made with an iron key, there is no more appearance of light than in the former instances. 4. Neither does this effect take place if the iron be applied to the eye, and the tin to the tongue, with a communication between them. 5. Gold and silver separately applied to these two organs, give hardly any sensation by their contact. 6. The same event takes place if two pieces of silver be used, and the communication be made by iron. 7. And likewise if copper be placed on the eye, tin on the tongue, and a communication be made by iron. 8. Neither is the sensation more evident if the silver be placed on the eye, the gold on the tongue, and copper be the medium of communication. 9. On the contrary, a considerable light is seen if the iron touches the eye, the silver the tongue, and the copper forms the communication. 10. Or if gold be substituted instead of silver. 11. Or if the communication between the iron on the eye, and the gold on the tongue, be made by means of a silver spatula. 12. Or if the iron on the eye, and the silver on the tongue, communicate directly. 13. Or if the order of these two metals be reversed. 14. Or if gold be used instead of silver. 15. And lastly, the same light of the combustion may be seen, if instead of placing one of the metals on the tongue, both be placed on the eyes.

By these experiments, which are all that I recollect at present, and may very easily be repeated and varied in different manners, we see that it is not electricity which produces the results. For it is well known, that the electric fluid thoroughly and instantaneously penetrates all the metals which most eminently conduct it, whatever may be their situation or relative positions.

But if it be true that the water affords the oxigen to the metal in these cases, it may be demanded, what has become of its hydrogen. It must be immediately observed, that on account of the contact of the atmosphere affording oxigen gas in proportion as the metal consumes it, very little of the water must be decomposed.

I have remarked, that I left the different metals for a long time in contact together, at the end of which I examined them, and I not only found them to be abundantly oxided, but I also found regular aluminiform saline crystals, adhering particularly to the pieces of silver, and salts of a very determined figure, consisting of two tetrehedral pyramids attached at their base, which appeared to me to be hydrogenated tin.

It is already known that hydrogen dissolves several metals; for in hydrogen itself are found iron, zinc, arsenic, &c. It is known, that the amalgam of zinc and mercury contains hydrogen, which may be driven off by heat.

I must add, that sometimes instead of placing my apparatus of tin and silver in water, I have left it a long time in alcohol, and found parallellipipedic crystals upon the silver, which were very transparent, and appeared to contain copper by their light greenish colour. This copper probably came from the silver; for in general I gave the preference to crown pieces, because I observe, that the irregularities on their surface occasioned by the impression, was greatly favorable to the formation of crystals which lodged in these cavities, and round their edges. I attempted to place the same metals in ammoniac inclosed in a crystal bottle, but without any remarkable effect; probably because the combination of the

<div align="right">hydrogen</div>

hydrogen is too ftrong, and the oxigen of the atmofphere could not unite with it in the decompofition of the metal. The ammoniac merely took a flight blueifh colour, which fhews that it had diffolved a portion of copper from the filver piece.

We fee very clearly, from the refults which I have obtained from the fimple contact of the two metals, namely, the oxide and the faline cryftals, that the fenfations experienced on the tongue, and on the eye, are the confequence of a chemical operation : and it therefore appears to me, that it is to thefe new compounds, or their elements, that we are to attribute that myfterious ftimulus, which produces convulfive motions of the animal fibre in a great part, at leaft, of the phenomena of galvanifm.

VI.

Report concerning the Art of making fine Cutlery.—W. N.

THE fabrication of edge tools is one of the firft arts among men in every ftate of fociety. Artizans are well aware of the neceffity, that the inftruments of their refpective trades fhould be made to poffefs the qualities adapted to the operations by which they gain their fubfiftence ; and among the various fub-divifions of labour, there is perhaps no material, upon which the fkill and judgement of practical men are more multifarioufly exercifed than fteel. The makers of files, of chiffels, of planes, faws, and the infinite variety of knives, all occupy their feveral departments feparate from each other, and poffefs their refpective degrees of celebrity among workmen, which are grounded on their knowledge of the peculiar kinds of fteel, as well as the methods of working them, which are beft fuited to the intended operations. Many of thefe methods are kept fecret ; but in general the philofophical enquirer will find the communications of operative men, to the full, as liberal and open as the circumftances of the cafe may feem to warrant. Many manufacturers have no referve with regard to the manipulations of their art, and have the fpirit to affert their claims to public encouragement, upon the open ground of the addrefs and integrity with which they conduct their profeffional labours.

Among the inftances of this kind which have occurred to me during a life of diligent enquiry, I have lately been much gratified by the ready affiftance and communications of Mr. Stodart of the Strand, which enable me at prefent to communicate my own notions on the fubject of fine cutlery, with the advantage and fupport of his fuccefsful experience ; which I fhall proceed to do without farther preface.

It appears to be at prefent generally agreed, that for all works which do not require welding, caft fteel is preferable to any other. For fine cutlery it undoubtedly is. Mr. Stodart ufes thofe bars which are marked Huntfman, but does not fuppofe it to be of a better quality than that of Walker, and other manufacturers. He complains, that it is

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much worfe in quality now than formerly, which complaint I have alfo heard from other intelligent artifts.   I did not afk him concerning the art of forging, but take it for granted, that it confifts in little more than the acquired fkill of managing the bar and the hammer, with the precautions not to injure the texture by ftrong hammering at too low a heat, or to degrade the quality of the fteel, by too much heat or expofure to the current of air from the bellows.

Cutlers do not ufe any coating to their work at the hardening heat, as the file cutters do; and indeed it feems evidently unneceffary when the article is intended to be tempered and ground.   Mr. S. agrees with me, that the beft rule is to harden as little as poffible above the ftate intended to be produced by tempering.   Work which has been overheated, has a crumbly edge, and will not afford the wire hereafter to be defcribed.   The proper heat is a cherry red vifible by day-light.   He has not found that any advantage is obtained from the ufe of falt in the water, or cooling that fluid, or from ufing mercury inftead of water; but it may be remarked, that queftions refpecting the fluid are, properly fpeaking, applicable only to files, gravers, and fuch tools as are intended to be left at the extreme of hardnefs.   Yet though Mr. Stodart did not feem to attach much value to peculiarities in the procefs of hardening, he mentioned it as the obfervation and practice of one of his workmen, that the charcoal fire fhould be made up with fhavings of leather: and upon being afked, what good he fuppofed the leather could do, this workman replied, that he could take upon him to fay, that he never had had a razor crack in the hardening fince he had ufed this method, though it was a very common accident before.

It appears to me from the confideration of other facts, that this procefs is likely to prove advantageous.   When brittle fubftances crack in cooling, it always happens from the outfide contracting and becoming too fmall to contain the interior parts.   But it is known, that hard fteel occupies more fpace than when foft, and it may eafily be inferred, that the nearer the fteel approaches to the ftate of iron, the lefs will be this increafe of dimenfions: If then we fuppofe a razor, or any other piece of fteel, to be heated in an open fire with a current of air paffing through it, the external part will by the lofs of carbon become lefs fteely than before; and when the whole piece comes to be hardened, the infide will be too large for the external part, which will probably crack.   But if the piece of fteel be wrapped up in the cementing mixture, or if the fire itfelf contain animal coal, and is put together fo as to operate in the manner of that mixture, the external part, inftead of being degraded by this heat, will be more carbonated than the internal part, in confequence of which it will be fo far from fplitting or burfting during its cooling, that it will be acted upon in a contrary direction, tending to render it more denfe and folid.

One of the greateft difficulties in hardening fteel works of any confiderable extent, more efpecially fuch articles as are formed of thin plates, or have a variety of parts of different fizes, confifts in the apparent impracticability of heating the thicker parts, before the flighter are burned away; befides which, even for a piece of uniform figure, it is no eafy matter to make up a fire which fhall give a fpeedy heat, and be nearly of the fame intenfity

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throughout. This difficulty formed a very confiderable impediment to my fuccefs in a courfe of delicate fteel work, in which I was engaged about feven years ago; but after various unfuccefsful experiments, I fucceeded in removing it by the ufe of a bath of melted lead, which for very juftifiable reafons has been kept a fecret till now. Pure lead, that is to fay, lead containing little or no tin, is ignited to a moderate rednefs, and then well ftirred. Into this the piece is plunged for a few feconds; that is to fay, until when brought near the furface that part does not appear lefs luminous than the reft. The piece is then fpeedily ftirred about in the bath, fuddenly drawn out, and plunged into a large mafs of water. In this manner a plate of fteel may be hardened fo as to be perfectly brittle, and yet continue fo found as to ring like a bell; an effect which I never could produce in any other way. Mr. Stodart has lately made trial of this method, and confiders it to be a great acquifition to the art, as in fact I found it.

The letting down, or tempering of hard fteel, is confidered as abfolutely neceffary for the production of a fine and durable edge. It has been ufual to do this by heating the hardened fteel, till its bright furface exhibits fome known colour by oxidation. The firft colour is a very faint ftraw colour, becoming deeper and deeper by increafe of heat, to a fine deep golden yellow, which changes irregularly to purple, then to an uniform blue, fucceeded by white and feveral fucceffive faint repetitions of thefe feries. It is well known, that the hardeft ftate of tempered inftruments, fuch as razors and furgeon's inftruments, is indicated by this ftraw colour; that a deeper colour is required for leather cutter's knives, and other tools that require the edge to be turned on one fide; that the blue which indicates a good temper for fprings, is almoft too foft for any cutting inftrument except faws, and fuch tools as are fharpened with a file; and that the lower ftates of hardnefs are not at all adapted to this ufe. But it is of confiderable importance, that the letting down or tempering, as well as the hardening, fhould be effected by heat equally applied, and that the temperatures, efpecially at the lower heats, where greater hardnefs is to be left, fhould be more precifely afcertained than can be done by the different fhades of oxidation. Mr. Hartley firft practifed the method of immerfing hard fteel in heated oil, or the fufible compound of lead five parts, tin three, and bifmuth eight. The temperature of either of thefe fluids may be afcertained in the ufual manner, when it does not exceed the point at which mercury boils: and by this contrivance the fame advantages are obtained in lowering the temperature of an whole inftrument, or any number of them at once, as have already been ftated in favour of my method of hardening. Oil is preferable to the fufible mixture for feveral reafons. It is cheaper; it admits of the work being feen during the immerfion by reafon of its tranfparency; and there is no occafion for any contrivance to prevent the work from floating.

I requefted Mr. Stodart to favour me with an account of the temperatures at which the feveral colours make their appearance upon hardened fteel; in compliance with which he made a feries of experiments upon furgeons needles hardened, highly polifhed, and ex-

pofed to a gradual heat while floating at the furface of the fufible mixture. The appearances are as follow :

No. 1. taken out at 430° of Fahrenheit. This temperature leaves the fteel in the moft excellent ftate for razors and fcalpals. The tarnifh, or faint yellowifh tinge it produces, is too evanefcent to be obferved without comparifon with another piece of polifhed fteel. Inftruments in this ftate retain their edge much longer than thofe upon which the actual ftraw colour has been brought, as is the common practice. Mr. S. informs me, that 430° is the loweft temperature for letting down, and that the lower degrees will not afford a firm edge.

No. 2 at 440°, and 3 at 450°. Thefe needles differ fo little in their appearance from No. 1, that it is not eafy to arrange them with certainty when mifplaced.

No. 4 has the evident tinge which workmen call pale ftraw colour. It was taken out at 460°, and has the ufual temper of penknives, razors, and other fine edge tools. It is much fofter than No. 1, as Mr. Stodart affures me, and this difference exhibits a valuable proof of the advantages of this method of tempering.

Nos. 5, 6, 7 and 8, exhibit fucceffive deeper fhades of colour, having been refpectively taken out at the temperatures 470°, 480°, 490°, and 500°. The laft is of a bright brownifh metallic yellow, very flightly inclining to purple.

No. 9 obtained an uniform deep blue at the temperature of 580°. The intermediate fhades produced on fteel by heats between 500° and 580° are yellow, brown, red, and purple, which are exhibited irregularly on different parts of the furface. As I had before feen this irregularity, particularly on the furface of a razor of Wootz*, and had found in my own experience, that the colours on different kinds of fteel do not correfpond with like degrees of temper, and probably of temperature in their production, I was defirous that fome experiments might be made upon it by the fame fkilful artift. Four beautifully polifhed blades were therefore expofed to heat on the fufible metal. The firft was taken up when it had acquired the fine yellow, or uniform deep ftraw colour. The fecond remained on the mixture till the part neareft the ftem had become purpleifh, at which period a number of fmall round fpots of a purpleifh colour appeared in the clear yellow of the blade. The third was left till the thicker parts of the blade were of a deep ruddy purple, but the concave face ftill continued yellow. This alfo acquired fpots like the other, and a flight cloudinefs. Thefe three blades were of caft fteel; the fourth, which was made out of a piece called Styrian fteel, was left upon the mixture till the red tinge had pervaded almoft the whole of its concave face. Two or three fpots appeared upon this blade, but the greater part of its furface was variegated with blue clouds, difpofed in fuch a manner as to produce thofe waving lines which in Damafcus fteel are called the water. Two refults are more immediately fuggefted by thefe facts; firft, that the irregular production of deep colour upon the furface of brightened fteel, may ferve to indicate the want of uniformity in its com-

* On Wootz, fee Pearfon in the Phil. Tranf.

pofition, as well as the method by an acid which has before been explained in this work *; and fecond, that the deep colour being obferved to come on firft at the thickeft parts, Mr. Stodart was difpofed to think, that its more fpeedy appearance was owing to thofe parts not having been hardened. But upon trial with a plate of fteel made quite hard at one end, and left foft at the other, I found that heat applied in the middle produced the regular changes at both ends precifely in the fame manner. I fuppofe, therefore, that the thicker parts finking deeper into the hot metal, experienced a ftronger re-action and better contact, which may have accelerated the communication of heat. It may be here noticed, that we found upon repeating the experiment of applying nitrous acid to bright fteel, which was hardened in part only, the black tinge appeared more fpeedily and ftrongly upon the hard parts, than the reft of the furface: a remarkable event, for the explanation of which I have no theory to offer.

Let us now fuppofe our cutting inftrument to be forged, hardened, and let down or tempered. It remains to be ground, polifhed, and fet. The grinding of fine cutlery is performed upon a grindftone of a fine clofe grit, called a Bilfon grindftone, and fold at the tool fhops in London at a moderate price. The cutlers ufe water, and do not feem to know any thing of the method by tallow †. The face of the work is rendered finer by fub-fequent grinding upon mahogany cylinders, with emery of different finenefs, or upon cy-linders faced with hard pewter, called laps, which are preferable to thofe with a wooden face. The laft polifh is given upon a cylinder faced with buff leather, to which crocus, or the red oxide of iron is applied with water. This laft operation is attended with confider-able danger of heating the work, and almoft inftantly reducing its temper along the thin edge, which at the fame time acquires the colours of oxidation.

The fetting now remains to be performed, which is a work of much delicacy and fkill: fo much fo indeed, that Mr. Stodart affures me, he cannot produce the moft exquifite and perfect edge if interrupted by converfation, or even by noifes in the ftreet. The tool is firft whetted upon a hone with oil, by rubbing it backwards and forwards. In all the pro-ceffes of grinding or wearing down the edge, but more efpecially in the fetting, the artift appears to prefer that ftroke which leads the edge according to the action of cutting, in-ftead of making the back run firft along the ftone. This proceeding is very judicious; for if there be any lump or particle of ftone, or other fubftance lying upon the face of the grinder, and the back of the tool be firft run over it, it will proceed beneath the edge, and lift it up, at the fame time producing a notch. But on the other hand, if the edge be made to move foremoft, and meet fuch a particle, it will flide beneath it and fuffer no injury. Another condition in whetting is, that the hand fhould not bear heavy: becaufe it is evident, that the fame ftone muft produce a more uniform edge if the fteel be worn away by many, than by few ftrokes. It is alfo of effential importance, that the hone itfelf fhould be of a fine texture, or that its filiccous particles fhould be very minute. Mr. Stodart informs me, that there are no certain criterions by which an excellent hone can be

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* Philofophical Journal, I. 470.   † Philofophical Journal, I. 131.

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diftinguifhed from one of ordinary value, excepting thofe derived from the actual ufe of both: that the Turkey ftone cuts faft, but is never found with a very fine grit: that the yellow hone is moft generally ufeful, and that any ftone of this kind requires to be foaked in oil, and kept wet with that fluid, or otherwife its effects will be the fame as that of a coarfer ftone under the better treatment: and laftly, that there is a green hone found in the old pavement of the ftreets of London, which is the beft material yet known for finifhing a fine edge.

The grindftone leaves a ragged edge, which it is the firft effect of whetting to reduce fo thin, that it may be bended backwards and forwards. This flexible part is called the wire, and if the whetting were to be continued too long, it would break off in pieces without regularity, leaving a finer, though ftill very imperfect edge, and tending to produce accidents while lying on the face of the ftone. The wire is taken off by raifing the face of the knife to an angle of about 50 degrees with the furface of the ftone, and giving a light ftroke edge foremoft alternately towards each end of the ftone. Thefe ftrokes produce an edge, the faces of which are inclined to each other in an angle of about 100 degrees; and to which the wire is fo flightly adherent, that it may often be taken away intire, and is eafily removed, by lightly drawing the edge along the finger nail. The edge thus cleared is generally very even: but it is too thick, and muft again be reduced by whetting. A finer wire is by this means produced, which will require to be again taken off, if for want of judgment, or delicacy of hand, the artift fhould have carried it too far. But we will fuppofe the obtufe edge to be very even, and the fecond wire to be fcarcely perceptible. In this cafe the laft edge will be very acute, but neither fo even nor fo ftrong as to be durably ufeful.

The finifh is given by two or more alternate light ftrokes with the edge flanting foremoft, and the blade of the knife raifed, fo that its plane forms an angle of about 28 degrees with the face of the ftone. This is the angle which by careful obfervation and meafurement, I find Mr. Stodart habitually ufes for the fineft furgeon's inftruments, and which he confiders as the beft for razors, and other keen cutting tools. The angle of edge is therefore about 56 degrees.

The excellence and uniformity of a fine edge may be afcertained, by its mode of operation when lightly drawn along the furface of the fkin, or leather, or any organized foft fubftance. Lancets are tried by fuffering the point to drop gently through a piece of thin foft leather. If the edge be exquifite, it will not only pafs with facility, but there will not be the leaft noife produced, any more than if it had dropped into water. This kind of edge cannot be produced, but by performing the laft two or more ftrokes on the green hone.

The operation of ftrapping is fimilar to that of grinding or whetting, and is performed by means of the angular particles of fine crocus, or other material bedded in the face of the ftrap. It requires lefs fkill than the operation of fetting, and is very apt, from the elafticity of the ftrap, to enlarge the angle of the edge, or round it too much.

Thoughts

VII.

Thoughts on Magnetism, by RICHARD KIRWAN, *Esq*; *L.L.D. F.R.S. and M.R.I.A.*

(*Concluded from page* 94.)

IF the south pole of a magnet be charged with filings of iron, and then approached to a bar of iron standing erect, part of the filings will drop off, because the poles of the same name, by exciting a contrary arrangement, weaken each other; but if the filings were suspended from the north pole of the magnet it would take up still more of the filings, as the opposite poles strengthen each other, the uppermost pole of the iron in this case becoming magnetic by position.

If the synonimous poles of two magnets of unequal powers be approached to each other, if the powers be very unequal the stronger immediately destroys the weaker, and inducing a contrary disposition attracts instead of repelling it; if the powers be less unequal it requires a longer time; so also if one be *softer* than the other. Even if their powers be equal, yet after some time the softer will yield to the harder. If both be equally hard they only *weaken* each other.

If a magnet be cut in two, in a direction parallel to the axis, the parts before conjoined will now repel each other, because they still retain two synonimous poles.

But if the magnet be cut in two in a direction perpendicular to the axis, the two ends before conjoined will now attract each other.

If a magnetic wire be twisted, its powers are so disordered that one side of the wire, in some places of it, will be attracted and the other side repelled by the same pole of the magnet.

The power of the magnets (*cæteris parib.*) is in proportion to their surfaces or as the squares of their diameters.—See *Hutton's Magnetism*, p. 72.

Communication.

When iron is applied to or brought within the sphere of activity of a particular magnet, it acquires the arrangement requisite to form the heteronymous pole, and thus becomes itself in some degree magnetic in its whole length, if this length be not totally disproportioned to the power of the particular magnet.

Hence the other end of such bar of iron acquires the the arrangement of the opposite pole, according to the laws of crystalization already laid down.

Iron becomes magnetic either by contact or proximity to a magnet, or by position, or by internal commotion.

If a bar of iron be placed in a vertical position its insensible fibrillæ gradually acquire the magnetic arrangement, so that after some years it becomes a complete magnet, its *lowest* part becoming a north pole, that is, pointing when free to the north, and the *upper* a south pole. In the S. hemisphere the under end becomes a south pole.

A bar

A bar of iron not previously magnetic does not acquire this difposition in the flighteft degree while lying in a horizontal or nearly a horizontal difpofition, but if one end of it be raifed it immediately acquires it in fome degree, as appears by approaching a magnetic needle to either end, becaufe in that direction it is then expofed to the activity of the polar ends of the great general magnet.

But if a bar of iron be heated, though only at one end, and while hot fet in a vertical or nearly a vertical pofition, it will acquire the magnetic power much more readily.

So alfo if one end of a bar of iron not magnetic be ftruck againft the ground it will become in fome degree magnetic, the lower end becoming a north pole, &c. and if afterwards the other end be ftruck in the fame manner the poles will be reverfed.

Hence it is evident that *any motion* communicated to the integrant particles of iron placed in a proper fituation helps them to affume the magnetic difpofition already impreffed upon them by the great general magnet.

If the oppofite poles of two magnets of *equal power* be approached to each other the power of both is increafed; and if one of them be *more powerful* than the other it will increafe the magnetic difpofition, and confequently the power of the weaker.

Soft iron, as its parts are moft eafily moved, receives the magnetic difpofition moft eafily, *hard* iron or tempered fteel more difficultly, and *caft* iron, as being both hard and abounding in the heterogeneous particles, moft difficultly and imperfectly.

Whatever way iron is applied to a magnet the magnetic power is diffufed in the direction of its length. Hence it fhould feem that when a bar of iron is laid on a magnet the contiguous ends of the iron become poles of the *fame name* with thofe of the magnet to which they are contiguous, and hence may be derived the power of *armed magnets*, for the furfaces of the *armour* immediately beneath thofe of the magnet imprefs a direction oppofite to their own on thofe of the magnet, and confequently rectify fuch furfaces of the magnet as may have been inaccurately directed, and thus ftrengthen it.

To communicate the magnetic power to iron by friction againft a magnet, it is neceffary that its pole fhould flide along the magnet feveral times in the fame direction, for if the directions be alternately oppofed the powers received will fuccefively deftroy each other.

A fynonimous pole is formed at the end at which the friction begins, to that of the magnet applied, and an oppofite at that at which it terminates.

Appropriation to Iron.

It has of old been obferved that the magnetic phænomena were peculiar to iron, and the reafons why they are fo have been already affigned, but of late fome femi-metals have been obferved to partake of thefe properties, as Nickel, Kobalt and Manganefe; this has been thought to arife from a mixture of ferruginous particles from which they can be fcarcely freed, and with refpect to Manganefe, and in many cafes of the others alfo, this feems to hold true; but with refpect to Nickel, and in fome inftances of the others alfo, the magnetic properties they difcover feem to me to proceed from their great attraction to

iron,

iron, particularly when their particles are duly arranged, for then they are expofed to the power of the great general magnet, which acts on them in proportion to this arrangement and their affinity to iron.

Of Inclination and Declination.

Thefe phænomena, which are fo different in different parts of the globe, and even in different feafons and hours of the day, not being as yet noted with fufficient certainty and precifion, I fhall for the prefent decline entering into their explanation.

VIII.

On the Fetid Gas of Drains.

SIR,

London, May 16, 1800.

THERE is a phenomenon which occurs too frequently for the comfort and eafe of many of the inhabitants of this and other towns which have fubterraneous drains, and has not been explained as I can yet find by reading or enquiry. Previous to, or about the time of the late change from fair to frequent and continued rains, the apertures of the common fewers, or great public drains communicating with the ftreets as well as with houfes, privies, &c. emitted a well-known and abominably difgufting fmell, which is often perceived upon change of weather. It is alfo obfervable in fome houfes that this fmell is always more or lefs predominant when the windows are fhut in at night. Perhaps I may be troubling you upon a very well-known fubject, when I requeft you will explain thefe facts, and point out the remedy; but I am fure, however, that many of your readers would be glad to have your thoughts upon it, as well as

Your obliged reader,

S. S.

The fubject to which the enquiries of S. S. are directed is, in fact, but little underftood. The emiffion of fetid gas from the drains is found to take place when the barometer falls; and perhaps alfo upon changes of the thermometer, though I am difpofed to think this laft is not the cafe. The offenfive gas which thus rifes from its fubterraneous fituation confifts, probably, for the moft part of fulphurated hydrogen, together with humid effluvia and putrefcent matter. Houfes which are fubject to this infection are remarkable for the fpeed with which filver plate becomes blackened, even when the prefence of the gas is not per-ceptible to the inhabitants; and they are alfo characteriftically remarkable for difeafe and
mortality.

mortality. I do not know of any experiments which have been strictly instituted to shew that the laws of the elasticity and consequent expansion of the gases under variations of pressure are different, and in what direction and quantity. But it is evident, in the present case, that when the superincumbent pressure of the air above us is diminished, the gas in the drains expands more than the common air near the surface of the earth: so that the former is made to ascend into the apartments of houses and the streets. The action of the fires in dwelling houses causing an ascending current up the chimneys, there must of course be a supply at the doors, windows, and other openings: but when these openings are most effectually shut, as is the case at night, they will less readily supply the air, and a larger portion will be pressed in from the drains. It is certain, nevertheless, that this pressure, as far as it regards the openings into a dwelling house, is in all cases very small. Indeed, there is no probability that it should equal the pressure of half an inch of water. The remedy for this serious and very disgusting inconvenience is, therefore, very easy and simple, and likewise very well-known, though much less so than could be wished when we reflect on the number of houses which are thus incommoded. I do not, therefore, think that the want of novelty ought to be considered as a sufficient reason why I should hesitate to describe for the use of my present correspondent, and others in the like want of information, the very common apparatus which bricklayers call a stink trap.

Let us suppose a drain, or subterraneous gutter, to communicate from beneath the pavement of a kitchen to the principal drain in the street, and that there is a grate, or a set of holes, in the pavement for the purpose of suffering waste water to flow off. These same holes will admit the noxious vapours of the drain into the house. But if any contrivance could be made that the drain should suffer water to run out, and at the same time prevent air from returning back, we should evidently be in possession of a remedy for the subject of complaint. A variety of methods present themselves for the accomplishment of this. The most common is distinguished by the name given above. It consists in inclosing the drain with a flat stone, which serves as a wall, to prevent all communication as to the upper part of the cavity: but the water is suffered to pass off beneath its lower edge by sinking the floor of the drain in that particular part. The depressed part or cavity, therefore, forms a small well or pool, in which water is constantly lodged; and the edge of the stone being designedly low enough to remain constantly immersed, the gas cannot return, unless its pressure be sufficient to sink that surface of the water which is furthest from the house to a depth at least equal to that depression;—an event which never happens.

Though this contrivance is attended with a charge which is very little when counter-balanced against the evils it removes, yet as it requires the pavement to be taken up, and a few other dispositions to be made, there are sufficient motives for preferring still simpler methods if offered. One of these consists in making the cavity or depression of the floor immediately beneath the opening in the pavement, and fixing a piece of wooden or other pipe to pass from the surface of the ground into the stagnant water. It may easily be understood that a perfect closure must be made round the sides of the pipe before the grate

or

or drain ftone is laid down. Another apparatus, which is ftill fimpler in its application is caft for that purpofe at fome of our iron founderies, or may be made in wood or metal by thofe in remote fituations, who may prefer it. A bowl about fix or feven inches in diameter, or even fmaller, is caft with a flanch or flat rim, for the purpofe of fetting it in the pavement. In the bottom, or loweft part of the cavity, there is a round hole, defended by a fhort upright pipe, through which water, if poured into the bowl, would pafs as foon as the bowl is filled to the top of the pipe. A perforated cover, with holes in it, is made fo as to fit in the cavity of the bowl; and there is another ftill fmaller bowl rivetted, bottom upwards, to the cover itfelf. When the cover is put on, the fmall bowl therefore furrounds the pipe, and when water is poured through the holes, it runs into the larger bowl, and a quantity always remains therein fufficient to ftand above the edge of the inverted bowl, becaufe the middle pipe rifes higher than that edge. It is fcarcely neceffary to add, that the wafte water will flow through the bowl by means of its pipe; but that the air of the drains can only have accefs to the infide of the inverted bowl, and cannot enter the houfe unlefs its preffure were fufficient to fink the furface of the included water, and raife that in the outer bowl as much as the inverted edge lies lower than the top of the pipe. The preffure is never equal to this quantity.

IX.

A Memoir, in which the Queftion is examined, whether Azote be a fimple or compound Body? By CHRISTOPHER GIRTANNER, *Doctor of Phyfic at Gottingen* *.

T HE moft celebrated chemifts have long been aware of the important part which is performed by azote in all the operations of nature. Lavoifier, Fourcroy, Berthollet, Van Mons, Guiton, Chaptal, Vauquelin, Prieftley, Van Marum, Goettling, Wiegleb, Von Hauch, Paets Van Trooftwyck, Deiman, and many other chemifts, have ftudied the nature of this fubftance with various degrees of fuccefs. To thefe united labours it is that we are indebted for our knowledge of its fingular properties, which are fo very different from the properties peculiar to other elaftic fluids.

But this fingular principle appears to be more efpecially of importance with regard to its effects in organized bodies.

In the inquiries which I have made for more than twelve years paft, on the mechanifm of life in animals and plants, I found myfelf every where ftopped by this principle fo little known. I faw it appear and difappear in my experiments, without being able to fix it, or to explain the manner in which it had been introduced into the bodies from which I had extracted it. I foon began to fufpect that azote was not a fimple, but a compound body.

* Communicated by the author to Citizen Van Mons, and publifhed in the Annales de Chimie, XXXIV. 1.

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I formed various conjectures, which experiment proved to be false, and had entirely re-nounced this research in despair, when the dispute which was agitated concerning the azote obtained from the vapour of water again excited my attention. This dispute is not yet terminated, notwithstanding what has been said for and against the change of water into gas by Goetling, Wiegleb, Von Hauch, Westrumb, Achard, Wurzer, Juch, Van Mons, Paets Van Trooftwyck, and Dieman. It appears to me that this famous dispute has much analogy with several other controversies mentioned in the annals of our science, and by which the moft important points of chemical theory have been fixed. Such was the dispute respecting the existence or non-existence of the carbonic acid in chalk, which engaged all the chemists of Europe; such was the dispute respecting the existence or non-existence of oxigen in the red oxide of mercury: a dispute in which I was myself engaged, and ill-treated by the German chemists, particularly Gren, who was easily put out of temper *.

Wiegleb, Goettling, Von Crell, maintained:

1. That the vapours of water in their passage through ignited tubes are changed into azote;

2. That this change always happens, and in all circumstances, provided that vapours of the water are brought into contact with red hot bodies;

3. That water is changed into azotic gas by combining with caloric;

4. That water is the ponderable basis of azote gas, and every other gas.

5. That, consequently, the theory of Lavoisier, is false.

The Dutch chemists, as well as Meffrs. Von Hauch, Juch, Van Mons, &c. maintained:

1. That the vapours of water in passing through red hot tubes are never, and in no case, converted into azote gas;

2. And that it is only by error that azote gas has been obtained, because it was not a product of water, but part of the atmospheric air which passed through the tubes;

3. That, consequently, the theory of Lavoisier remains unshaken, and the theory of phlogiston, or that of water being the base of all the gases, is erroneous.

I was very much interested in this dispute, of which I very attentively followed the progress. But I was sorry to observe that the spirit of party mixed in the discussion; that harshness prevailed on both sides; that the dispute was not for the acquisition of truth, but victory, and the enquirers had previously decided to find only such facts as their theories required; and, consequently, that their discernment, even in matters of fact, became impaired. Experiments were multiplied; the truth of the narratives was on both sides disputed; results were found absolutely contrary to each other; and the parties,

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* Out of all the opinions which Gren has supported, there is not one in which he has been in the right: but he possesses the merit of having written his Syftematisches Handbuch, which is an excellent compilation. The hands of Gren were of more value than his head; he compiled well, but his meditations possessed no force. G.

inftead of difcovering the truth as a common bafis of union, became more and more remote from each other in their conclufions.

For my part I was no otherwife interefted in this difpute than to fupport the interefts of truth. I am intimately perfuaded that the fyftem of Lavoifier is conformable to nature: An experiment is announced to me which is faid to overthrow this fyftem totally. Let us fee; let us examine; let us repeat; and if the fyftem be falfe, it is better to abandon it in good time, ai fonot to wait till it abandons us. Let us not be attached to fyftem, but to truth; and when nature fpeaks, let us liften to her voice in preference to Stahl or Lavoifier, Defcartes or Newton. Whatever may be the refult of our experiments, we fhall not fail to profit. The thing of which we rifk the lofs being merely error, we cannot be too defirous of undergoing it.

Thus it was that I reafoned. I had learned by the hiftory of chemiftry that in all difputes where the parties obtained oppofite refults from the fame experiments, there is an error in the manner of expreffing themfelves, and that at the bottom both parties are in the right. I was very much difpofed to think that the cafe before us was a queftion of this kind.

I, therefore, propofed to myfelf the refolution of the following queftions:

1. Are the vapours of boiling water changed into azote gas in their paffage through ignited tubes?

2. In what circumftances does this change take place?

3. What is the caufe of this production of azote gas?

4. Are the experiments contrary to the fyftem of Lavoifier, or not?

I muft firft confefs, before I enter upon this difcuffion, that the manner in which the production of azote gas had been explained, in fuppofing the external air to pafs through the tubes, did not appear to me at all fatisfactory.

I had myfelf formerly adopted this explanation *; but I very foon gave up an opinion fo contrary to every thing we know of chemiftry.

I am really concerned to fee this improbable opinion maintained by chemifts of the firft rank. But the hiftory of chemiftry fupplies us with many examples of the fame nature. Before the immortal Lavoifier had profcribed the doctrine of phlogifton, the Stahlians removed a leading difficulty of their fyftem in a manner abfolutely fimilar. The oxide of mercury was put into a crucible, which was clofed in the moft accurate manner, and then expofed to heat. Upon opening it after cooling, it was found to contain running mercury. Now a reduction of this kind not being poffible, according to the doctrine of Stahl, without the intermedium of phlogifton, the partizans of that doctrine were afked for an explanation of a fact fo contrary to it. They replied, that the phlogifton had paffed through the crucible to unite with the mercury. Even Bergmann and Scheele were content with fo abfurd an explanation †, which proves that the fpirit of fyftem mifleads the beft underftandings, and renders them ridiculous in the eyes of pofterity

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* Anfangsgrunde der Antiphlogiftifchen Chemie, fecond edition, page 89---92.

† Scheele Von luft und feuer, edition of Leonhardi, page 42.

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lefs prejudiced and more enlightened. I fhall fhew that azote does not pafs through ignited tubes, any more than phlogifton paffes through crucibles. The experiments which Prieftley has made to prove that air paffes through earthen retorts do in no refpect prove it. He faw the retorts fmoke on their outer furface: but this vapour was not, as he imagined, water paffing through: but the external furface of the earthen retort attracted water from the atmofphere.

(To be continued.)

X.

On the Power of the Oxygenated Muriatic Acid in Vegetation. By a Correfpondent.
To Mr. NICHOLSON.

SIR,

FROM a conviction that you have a fatisfaction in giving a place in your ufeful Journal, to any hints that tend to reftore an important difcovery to its rightful owner, I am induced to tranfmit to you the annexed tranfcript of part of an effay written by the late ingenious Dr. Ingenhoufz.

It was publifhed in the year 1796, in an appendix to the general report of the Board of Agriculture: and clearly demonftrates, that at that period, the Doctor not only poffeffed an idea of the power of the oxygenated muriatic acid in accelerating the germination of feeds, but had actually put it to the teft of experiment. I need hardly add, that the effects of this acid in refufcitating the dormant powers of vegetable animation, has lately been imported as a philofophical novelty from the Continent.

" As I have made mention more than once in this paper, of a letter I wrote to Sir John Sinclair, dated Dec. 2d. 1794, on different articles relating to agriculture, and among others, upon the beneficial effects of alkaline falts in promoting vegetation, and refpecting the effects of fome other falts; and as that letter makes no part of this paper, I think it proper to inform the reader, that the particularly good effect of alkaline falt was fo manifeft in my own garden, that all the gardeners who faw it thought it equal to the effects of the beft horfe dung. I repeated the application of that falt laft year (1795) at Hartford with the Hon. Baron N. Dimfdale, M. P. in his garden, and that gentleman was equally convinced as myfelf, of its manifeft good effects. We tried at the fame time, the application of different neutral falts, the particulars of which experiments I may poffibly publifh on fome future occafion. We made alfo many experiments with different folutions, and medicated liquors poured upon the ground, as well as fteeping the feeds of different grains in them. Be it fufficient to fay here, that of all the neutral falts we tried, the glauber falt did feem to be one of the beft in promoting vegetation; and that the fteeping the feeds in the oxygenated marine or muriatic acid, (which is now much employed in bleaching linen

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in an expeditious way) had a particularly beneficial effect in producing early and vigorous plants. The beneficial effects of thefe different fubftances may be eafily accounted for by an intelligent reader, according to the theory laid down in this paper."

" We were fomewhat aftonifhed that thofe feeds, *viz.* wheat, barley, rye, and oats, which had been fteeped in the above-mentioned oxygenated muriatic liquid, even during forty-eight hours, did thrive admirably well; whereas the fame feeds fteeped during fo long a time in fome of the other medicated liquids, were much hurt, or had loft their vegetative power. The fame oxygenated liquid poured upon the ground had alfo a beneficial effect."

The differtation from which the above extract is made, although evidently a hafty and unfinifhed production of its truly fcientific and refpectable author, contains fo many original hints, and ufeful views, refpecting the important object of increafing the fubfiftence of mankind, as derived from the production of farinaceous vegetable matter, that it is to be regretted, it is not known beyond the circle to which its peculiar mode of publication has neceffarily limited it.

<div style="text-align:center">

Your's

A conftant reader,

B.

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SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

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An Account of the Irides or Coronæ, which appear around and contiguous to the Bodies of the Sun, Moon, and other luminous Objects *. Octavo, 46 p. one Plate. Cadell and Davies. 1799.

T HIS work, as well as the other concerning the Inflexions of Light, noticed in the Journal of the laft month, page 78. is the production of Gibbes Walker Jordan, Efq. the initials alone of whofe name are affixed to the concluding page of each work. This contains an application of fome of the principles and difcoveries contained in that, to the explanation of the phænomena named in the title, an explanation which how greatly foever it might have been hitherto defired, was certainly unattainable before thofe difcoveries were known.

All irides, or coronæ, our author divides into four forts; 1. irides confifting of many concentric orders of colours contiguous to the fun or moon; 2. the iris of 45 degrees diameter; 3. the iris of about 84°; and 4. the iris of about 100 degrees diameter, the primary and fecondary rainbows of philofophy. After obferving the infufficiency of the principles applied by Des Cartes, Huygens, and Newton, to the explanation of thefe dif-

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* For this article I am obliged to the fame correfpondent as for the former.

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ferent appearances, and reprobating the conſtruction of frozen machineries for the purpoſe, he proceeds to ſtate the circumſtances under which the irides of the firſt ſort, the objects of immediate enquiry, are exhibited.

Theſe irides appear of one or more concentric orders and circles of colours, cloſe around and contiguous to the ſun or moon, and ſometimes around the brighteſt of the planets and fixed ſtars. The colours from the luminary outwards are greyiſh, black or blue, or faint white, ſucceeded by a broad denſe white, followed by yellow, then by red; then by violet, blue, green, yellow, red; green, diluted yellow, red; diluted green, diluted red. The ſecond order is frequently only green, yellow, red; and the third only diluted green, red. The two firſt orders are moſt frequently ſeen, the third leſs frequently, the fourth moſt rarely. The firſt may be ſeen every night, when the moon, with more than half a face, ſhines through thin white fleecy clouds, and through ſimilar clouds by receiving the ſun's image through a ſmall hole into a darkened chamber on a ſheet of white paper.

Of theſe irides the diameters vary conſiderably: that of the firſt order from one degree to $5\frac{1}{2}$, and that of the ſecond from $3\frac{1}{2}$ to $10\frac{1}{2}$ degrees. The general breadth of the firſt order of colours is rather more than 45 minutes, and thoſe of the others are ſucceſſively leſs.

In explaining theſe phænomena, icy cryſtalizations of all ſorts are rejected, and globules of water, the only regular forms of concretion which the vapours of the atmoſphere admit of, are alone to be reſorted to for the principles of exiſtence of regular and conſtant phænomena.

For explaining theſe phænomena as produced by globules of water, all the hitherto accepted principles are ſhewn to be inſufficient. The principle of parallel, or efficient rays, employed in the ſchools to account for the exhibition of the primary and ſecondary rainbows, is exceptionable in its former uſe, and is not here to be received, becauſe viſion of images is not by parallel rays, becauſe the light after paſſage, if ſufficiently ſtrong to be ſeen, would only produce a broad circle round the luminary of decaying white light.

Newton explains theſe phænomena by the fits of eaſy tranſmiſſion and reflection, and by comparing the refraction of the rays paſſing through a drop of water, to the reflection of a ſlender beam of light from the back part of a glaſs lens. The fits of eaſy tranſmiſſion and reflection have no exiſtence. The circumſtances of the refracted light in the phænomena are altogether, and eſſentially different from thoſe of the light in the experiment by reflection from a lens, and it is further ſhewn to be impoſſible by any refractions through a drop to produce appearances of the obſerved orders of colours. It is a principle derived from the doctrine of images, and uſed in explaining the primary and ſecondary rainbows, that the colours of the drop are inverted in the bow; and according to this principle, theſe irides, if formed by refractions through the drops, would have their blues external, their reds internal, contrary to obſervation. For this ſame reaſon, and from an eſſential change in the attendant circumſtances, theſe phænomena cannot be produced by the inflections obſerved in a ſlender beam of light paſſing by the edge of a ſingle body.

The

The only true principles of explanation of these phænomena, are to be found among those new observations concerning the inflections of light before spoken of.

When two bodies approach one another in a solar beam, the passing light between their edges is divided and distributed on both sides into two complete sets of fringes. *Vide* Journal, Observation ε, page 81. Each of these sets of fringes beginning from the central distance between the bodies, and advancing towards either of the bodies, is composed of the same colours and orders of colours as the observed irides. Of these the blues are least, the reds most bent, the blues furthest from, the reds nearest to the body.

Whenever therefore two globules of water in a cloud or vapour approach near each other, the light passing between them will be distributed into similar fringes, and the union of these fringes from between different drops, will produce all around the luminary circles, and orders of colours, such as by observation are really found to be exhibited.

The nearer to, or more remote the two bodies of the experiment are from each other, the more or less broad will be the fringes, and therefore the breadths also of the irides will depend upon, and vary with the distances of the globules, and thence the observed variations of breadth are easily accounted for. The breadths are varied also, and more especially in vapours of confiderable extent or thickness, by the actions of the succeeding series of globules, for although not in the same plane, bodies, and consequently these globules, act upon the light passing between them, and form fringes. *Vide* Journal, Observation η, page 81.

These principles being applied to explain the various phænomena of lights and coloured circles, exhibited in various situations by the sun and moon through the vapours of the atmosphere, they are next extended to all similar phænomena observed round other luminous objects as well as these. The flames of candles and lamps seen through steam, through the exhausted receiver, through thin flocks of wool or cotton, through stuffs of cotton, linen, silk, or wool, through the glasses of coach windows, on which small drops of water are precipitated from the breath of the inclosed persons, are surrounded with coronæ produced in a similar manner. Tapers are seen to burn blue by an eye charged with semipellucid humours arising from injury, watchfulness, and other causes producing rings of colours, sometimes one of dusky blueish green, faintly terminated with yellow and red ; and sometimes a second of dusky green and red. A phænomenon of this sort occurred to Des Cartes. A hole in a window shutter will also, under similar circumstances, produce similar appearances.

These observations are illustrated by figures of reference, and may further be confirmed by viewing a candle at the distance of ten feet or more through a thin flock of wool, or by breathing on a pane of window-glass, and looking at the image of a candle reflected obliquely from the glass through the precipitated drops of moisture, when a single halo, composed of the first order of colours terminated with yellow and red, will be distinctly seen.

All

All the circumftances of thefe phænomena being thus fully confidered and explained, and no doubt remaining of their characters and caufes, they may be called *Irides by Inflection.*

A general View of the Nature and Objects of Chemiftry, and of its Nature and Application to Arts and Manufactures.   By William Henry, Member of the Royal Medical, and Natural Hiftory Societies of Edinburgh ; of the Chemical Society of Jena in Saxony: and of the Literary and Philofophical Society of Manchefter. Octavo, 44 Pages.   Johnfon.   London.

It is ufual for thofe philofophers who undertake to explain the feveral departments of fcience by lectures, to publifh a fyllabus, or outline of their fubject in the order according to which they propofe to treat it.   Thefe fhort works, if drawn out with ability, are not only of eminent ufe to the claffes who may attend the lecturer, but alfo to the community at large, as exhibitions of the ftate of fcience, and admirable helps to the memory.   Some of thefe works have the form of a fyftematical enumeration : others that of a regular difcourfe.   The prefent work is of the latter kind, and prefents an elegant and comprehenfive view of the objects, nature, and application, of chemical fcience.

Defcription d'un Télégraphe très fimple et a la porteé de tout le Monde, avec une Planche.   A Paris, chez l'auteur, rue de la Libertè, No. 83. Pluvoife An. 8. de l'imprimerie de l'inftitut des aveugles-travailleurs, 16 Pages in 8vo. Prix 8 sols.—*Or, a Defcription of a very fimple Telegraph, practicable by any Perfon: with an Engraving.*

The telegraph propofed by this author is the human body : the arms of which are capable of forming with regard to each other, as well as to the perpendicular line of the trunk, a great number of figures, fufficiently diftinct to be eafily feen at confiderable diftances by the naked eye, or with a telefcope.   The author developes his method, and the means of carrying it into effect : and he thinks, that in this manner lines of moveable telegraphs may be formed, which may become very ufeful in war, to keep up a fpeedy and conftant communication between the different bodies of an army, or between the fixed telegraphs of other conftructions.

This memoir is part of a larger work on the prefent and other fubjects connected it, which will hereafter appear.

Magazin. Encycl.
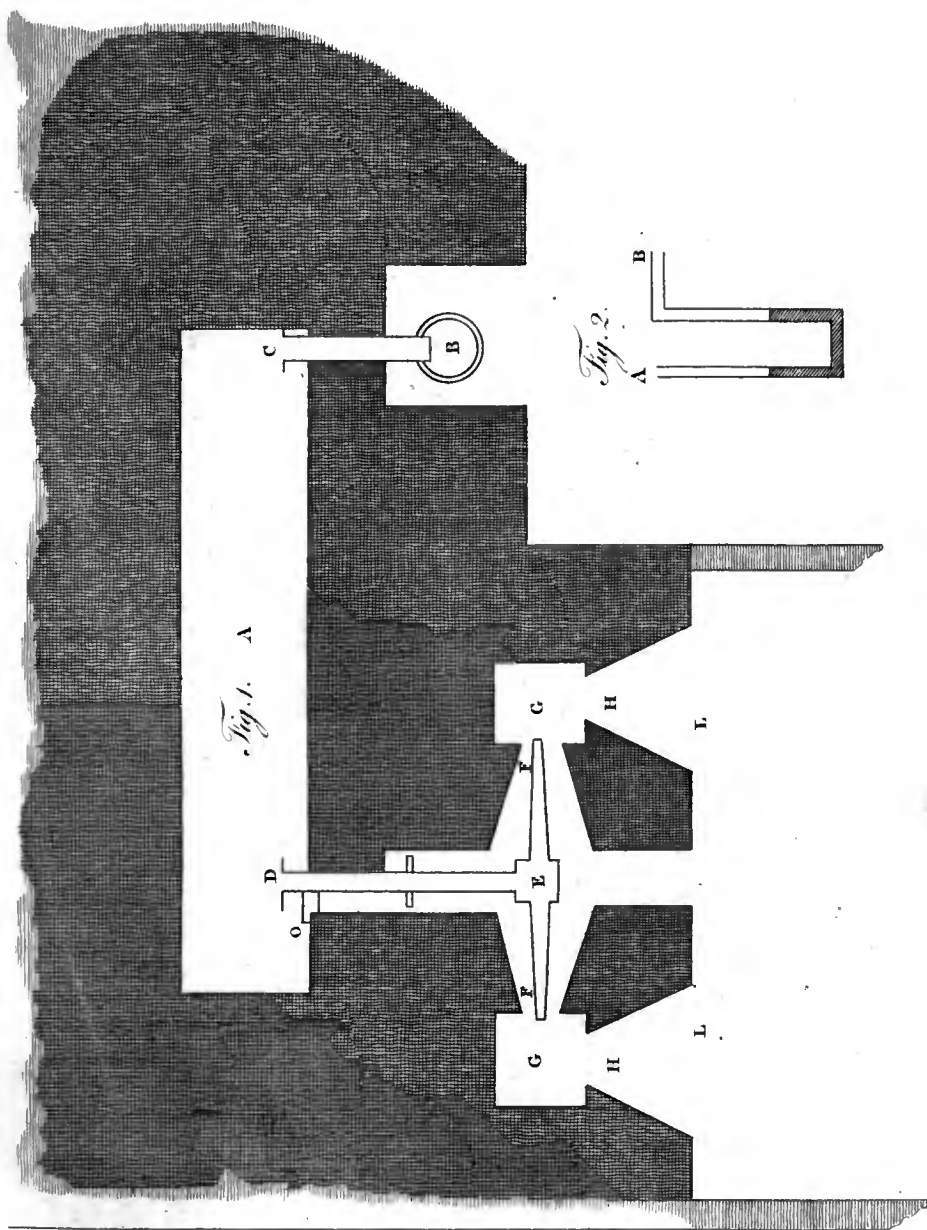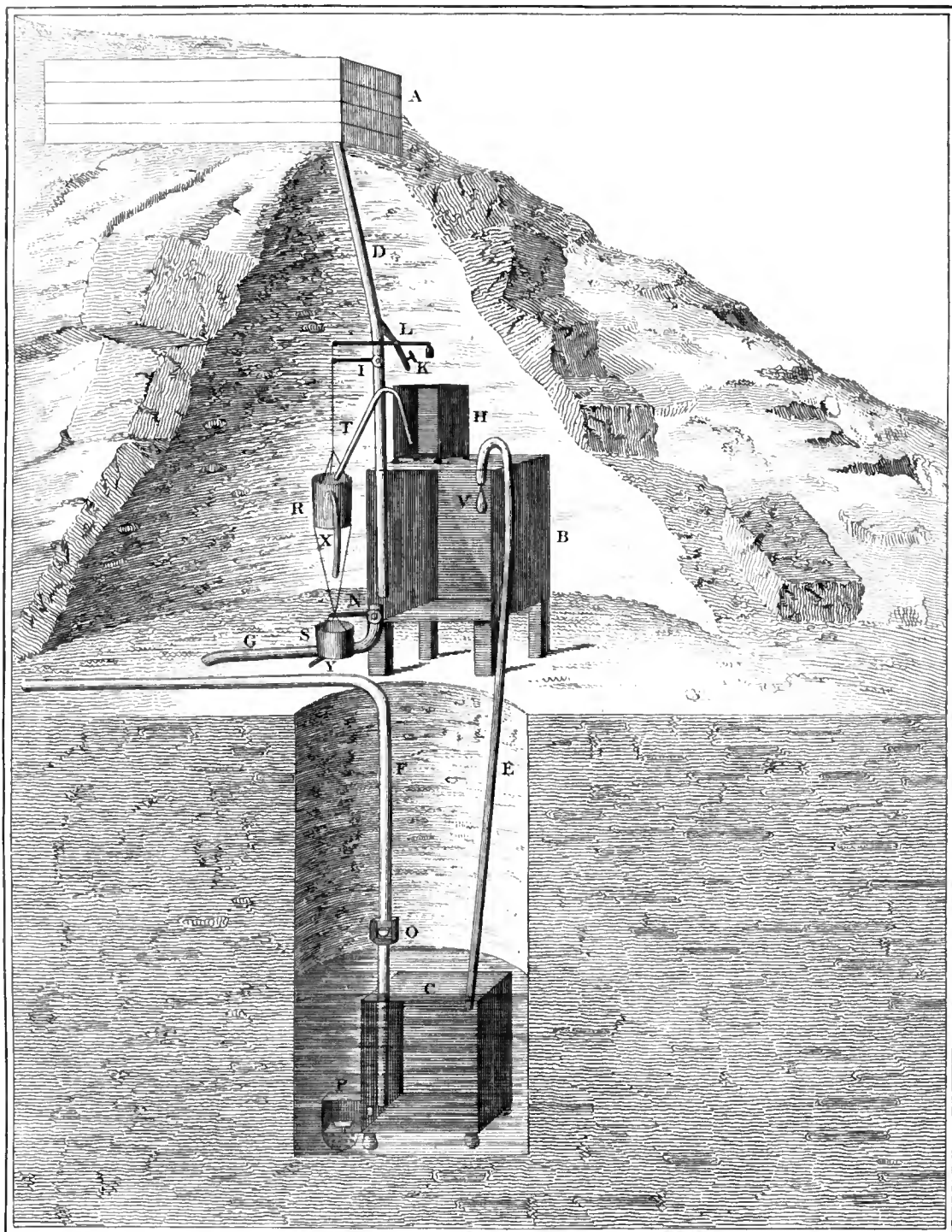
Fig. 1.

A

B

C

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Fig. 2.

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A

JOURNAL

O F

NATURAL PHILOSOPHY, CHEMISTRY,

A N D

THE ARTS.

ARTICLE I.

Defcription of a new Method of extracting Silver from Copper-Mat by means of Lead, by which the Eliquation of Black-Copper is rendered unneceffary. By the late Dr. GREN*, Prof. at Halle in Saxony.

NOTWITHSTANDING the confiderable progrefs which the metallurgical part of fcientific chemiftry has made in modern times, it muft be allowed, that its influence on the manipulations in the fmelting works has hitherto been but of little importance, and that many of thefe operations are carried on by the proceffes ufed feveral centuries ago; with all the errors which have been difcovered by fubfequent improvements in chemical knowledge. The introduction of the *prefent procefs of eliquation*, was indeed a material and great improvement on the former method of feparating filver from copper, by which the *copper-mat* † was combined with lead, and the mafs afterwards fubjected to gradual heat. For in this

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* Tranflated from *Dr. Scherer's*, fucceffor to Gren in the profefforfhip at Halle, *General Chemical Journal*, Vol. IV. page 155.

† *Copper-mat* (Kupfer-rohftein) is the product obtained from pyritous copper-ores, having undergone the firft or *crude fufion*, by which the ftony matrix only has been feparated.---*Gren's Principles of Modern Chemiftry*, § 1444.---Tranfl.

method, the requifite or precife proportion of lead to copper, was totally unknown. As ingenious, however, as the modern eliquating procefs may appear in a chemical view, it is ftill in its own nature attended with many confiderable imperfections; namely, a great lofs of time, of fuel, and efpecially of lead. Thefe imperfections are infeparable from this procefs, and render it fo very expenfive, as to be totally inapplicable in the operations with *black copper* *, which contains lefs than four ounces of filver. It is well known that *Born* has recommended the amalgamation of the copper-mat as a more advantageous method to extract its filver. But even though the amalgamating procefs, of which the efficacy and utility is manifeft in the richer filver ores, had not been proved by experience to be totally impracticable with refpect to fuch products as contain lefs filver, for example the copper-mat; yet the operation is rendered precarious, and its eftablifhment becomes an object of rifque from the circumftance, that the material requifite, is in the hands of a poffeffor, who may raife its price at his own arbitrary choice.

My refearches into the copper-mat, and its conftituent parts, as well as a more accurate ætiology of the whole of the ufual procefs, of extracting from it the copper and filver in a feparate ftate; together with the inquiries into the mutual powers of chemical attraction of the fulphur and the metals contained in it, have fuggefted to me a new and profitable method of feparating the filver from copper-mat by means of lead, by which the whole procefs of eliquating the black copper is rendered unneceffary. In this method, not only the lofs of lead by oxidation, which renders the eliquating procefs fo expenfive, but likewife all the other charges of this procefs are avoided; as for inftance, thofe of the building, and the requifite furnaces and edifices, the neceffary fuel, &c. My method is no lefs fimple than eafy; it is certain and effectual; and (what principally recommends it) it requires no particular edifice; but only a flight alteration in the form of the ufual furnace with a concave floor (*Krummofen*). It is founded on invariable chemical, hydroftatic principles; and is found to be practically applicable and effectual in all the experiments which I have repeatedly made in the fmall way, and which, in this cafe, cannot but be the fame as thofe in the large way.

Principles, on which the new Method is grounded.

1. Copper-mat confifts of fulphur, copper, iron, and filver; fo that thefe metals are completely diffolved by the fulphur.

2. Sulphur can diffolve in fufion only a determinate quantity of reguline metals previous to its faturation.

3. Sulphur has a ftronger chemical attraction for *iron*, than for copper, lead, and filver.

4. Sulphur likewife attracts *copper* more ftrongly, than lead and filver.

5. And laftly, the chemical affinity of fulphur with *lead* is greater, than its affinity with

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* *Black copper* is the metal, which is obtained by a fecond fufion from copper ores.----Tranfl.

filver.

filver. Hence the graduated arrangement of the chemical attractions of fulphur, with re-
fpect to the metals mentioned, is as follows :

SULPHUR,	LEAD,
IRON,	SILVER.
COPPER,	

6. When the fulphur is faturated with copper, it can no longer diffolve any portion of lead and filver.

7. Silver and lead have a very ftrong chemical attraction for each other.

Inferences from thefe fundamental Principles.

From thefe affertions, unqueftionably proved by experience, it follows, that if *copper-mat,* the fulphur of which (the proportion having been fo much altered by roafting, as to be fatu-rated with copper, is made to be penetrated by lead in fufion, the lead muft then take up all the filver which it contains, and in fuch a manner, that no part of the lead can be diffolved by the fulphur contained in the copper-mat.

But it would be very wrong to conclude, that the filver can be completely extracted from this roafted copper-mat, merely by fubjecting the lead, together with the copper-mat, to a melting heat. For the lead, on account of its eafier fufibility, would melt fooner than the copper-mat, fink down to the hearth of the furnace, and by its greater fpecific gravity always remain beneath the fufed copper-mat; and confequently it would deprive this laft of its filver merely at the furface, with which it is in contact, and no farther. If, there-fore, it be wifhed to obtain the intended object, it will be neceffary that the fluid copper-mat fhould be forced to rife through the lead in fufion, and to penetrate it entirely.

It is on this circumftance, that the characteriftic and moft effential part of my new method depends.

It confifts in the management, by which the copper-mat (duly roafted, and in which the fulphur is faturated with the copper) is made to rife immediately, and during its fufion through the melted lead. I might call this new method an *amalgamation of fufed copper-mat with fufed lead.*

The poffibility of effecting the conditions, requifite for this purpofe, appears very eafily from *hydroftatical* principles.—Let the fpace of the outward floor, or hearth B, Fig. 1. Plate VIII. be connected with the cavity of the furnace A, by means of a hole (*a*). Now if there be only melted lead in the furnace, it will ftand on the outward hearth, equally as high as in the furnace itfelf. Suppofe its horizontal furface to be as marked (*b c*); then if the melted copper-mat in the interior furnace A, which is of lefs fpecific gravity, be above that height, it will rife by the laws of the *equilibrium* in the outward hearth B.

The fecond chief condition is obtained, by an eafy management of the fire employed to effect the fufion. This melting furnace has a recurved bottom, and is provided with an outward hearth, (*Vorderheerd*) and a draining hearth (*Stichheerd*). Its effential form of conftruction is fufficiently fhewn in the drawing, before referred to. In this manner the

hearth

hearth of the furnace lies partly within and partly without; but it is not neceffary, that the projecting part of its hearth fhould be of confiderable breadth. The aperture (*a*), by which the exterior part of the furnace communicates with the interior, fhould be placed immediately above the floor or bottom of the hearth, or which is ftill better in that bottom itfelf. And the outer hearth of the furnace fhould have an incifion with a gutter, through which the melted copper-mat, rifing above the lead flows off continually, and is collected in a pot (vortiegel) placed on the floor of the fmelting-houfe. There is, befides, on the fide of the outward hearth, another adjoining hearth, into which the lead of the former, when fufficiently impregnated with filver may be drawn off, to be afterwards put into iron veffels. The moft convenient and advantageous dimenfions of thofe hearths, efpecially the height of the outward-hearth from the floor up to the twyer of the bellows, can indeed be afcertained by experiments only, but thefe may be eafily made without much expence.

In order to deprive the copper-mat of its filver by means of lead in this apparatus of the furnace, and to manage the fufion properly, it is required that the proportion of the fpecific gravity of the copper-mat to that of the lead fhould be known. For, as this procefs is grounded on the well known hydroftatical principle, that the heights of two fluids of different kinds, placed in connected tubes, if required to be in *æquilibrio*, muft be to each other in the inverfe ratio of their fpecific gravities, it is evident, that the melted copper-mat in the furnace, to keep the equilibrium with the fufed lead in the outward hearth, muft form a column proportionably fo much higher, as it is exceeded by the lead in fpecific gravity. It is therefore from the fpecific gravities of both thefe fubftances, that we may find how far diftant the twyer muft be from the loweft point of the hearth, and to what height the lead may be fuffered to rife in the outer hearth, with caufing the fluid copper-mat to enter into that aperture. The fpecific gravity of copper-mat is variable, according to the different degrees of its roafting. That of fufed copper-mat varies from 4.66 after the fecond roafting, to 5.20 after the third roafting.

But for the fake of greater fecurity, the leaft weight only fhould in this cafe be affumed as a ftandard. The fpecific gravity of lead, at a mean rate, is 11.35.

From thefe *data* it is obvious, that if the height of the *lead* in the outward hearth be *fix* inches, the *copper-mat* in the interior furnace, to be equiponderant, muft ftand *fourteen inches and one half* high.—If then the copper-mat, rifing through the lead in the outward hearth, covers the furface of this laft to the height of $\frac{1}{2}$ an inch, its column muft likewife be proportionally higher in the furnace, and confequently reach to 16 inches. It is therefore evident, that, when the lead rifes to 6 inches height in the outward hearth, the diftance of the twyer, or nozzle of the bellows, muft amount to at leaft 18 or 20 inches from the loweft point of the hearth.

According to the above ftated fpecific gravity of lead, one Rhinland cubic * foot of that metal weighs 750 lb. Cologne weight. When therefore 300 pounds of lead are employed

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* The Rhinland foot is reckoned at 12,396 Englifh inches, and the mark of Cologne is 3612 grains, whence the pound will be 7224 grains, or about half an ounce more than our avoirdupois pound.----N.

in this operation, their volume will amount to 760 cubic Rhinland inches; and hence if the perpendicular height of these three centners of lead shall be 6 inches in the outward hearth, it follows, that the breadth of this hearth muſt be 9 inches, and its length a little more than 14 inches.

In performing the procefs, the furnace is previouſly heated to a fufficient degree, and the lead afterwards introduced on the outer hearth; becauſe the heat is there fufficiently intenfe to fufe it. Its furface is fecured againſt oxidation, by covering it with powdered charcoal. At the fame time the furnace is charged with copper-mat, and the requifite quantity of coals, and the fufion effected by a clear fire. As the operation proceeds the lower part of the copper-mat iſſuing from the furnace through the aperture (*a*), and rifing through the body of the lead to its furface, is farther prevented from cooling, by means of fome pieces of burning wood. The beſt and moſt ufeful proportion of the coal, to that of the copper-mat in this fufion, muſt be determined by experience.

The copper-mat being now deprived of its portion of filver, and flowing into the fore-pot (vortiegel) from above the lead in the outer hearth, is once more roaſted, previous to extracting the copper. But it is to be obſerved, that by this method of diveſting it of its filver, it at the fame time undergoes a farther roaſting; on which account another part of the expence is faved in my procefs.

In the management of this procefs, it is evident, that as much copper-mat as may be thought fit, can be decompofed by the fame individual quantity of lead; and accordingly, that this laſt may be impregnated with filver at pleafure in various degrees. It is not ad-vifeable, however, to alloy the lead with too great a proportion of filver; becaufe a fmall lofs of the compound metal would then produce a more confiderable lofs of filver. But I muſt leave this to the judgment of the artiſt, and to future experience.

When the lead is to be run off, the receiver muſt be firſt duly heated, and then the tap hole opened. The whole of the lead is thus drawn off from the outer hearth of the furnace, and its furface covered with charcoal-duſt. Freſh lead may then be again conveyed, after ſtopping the tap-hole, into the outer hearth, and the operation continued as before.

Whenever the furnace requires to be cooled by the blaſt of the bellows, the *work lead* * is firſt entirely drawn off from the outer hearth, and alfo laded out of the draining hearth; after which, all the copper-mat that remains in the outward hearth, and in the furnace, is fuffered to run into the draining hearth, now cleared of the lead. But this copper-mat, not being yet deprived of the filver it contains, muſt be added at the next charge of the furnace.

The lead, which is thus alloyed with filver, is at laſt fubjected to cupellation in the ufual way. The *litharge*, which is obtained in this operation, the metal abforbed in by

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* *Work-lead*, or lead freed from copper, and mixed with filver by the operation. See *Gren's Principles of Chemiſtry*, § 1478, 1486.----Tranſl.

the

the *teſt* (heerd) and the *firſt waſte* * (abzug) need not be reduced to the reguline ſtate by a ſeparate operation ; becauſe this may be done at the ſame time, that another portion of copper-mat is cleared of its ſilver by fuſing them together with it. But as this perfection of any art can only be obtained by experience, and diſpatch in the manipulations can be · acquired by practice alone, it will be found neceſſary, even in this inſtance, to perform repeated experiments, for the purpoſe of aſcertaining the beſt dimenſions of the parts of the furnace, the moſt ſuitable proportion of the quantity of coals to be employed, and the moſt advantageous direction of the bellows pipe ; as well as the moſt convenient regulation of the blaſt, and the moſt ſuitable proportion of the copper-mat to the lead to be employed in this proceſs.

The advantages afforded by my method, compared with thoſe reſulting from the proceſs of eliquation, are evident. No lead is here loſt by combuſtion, becauſe it is accurately covered by the copper-mat, and no intenſe heat is required, becauſe theſe ſubſtances are of eaſy fuſion. A much ſmaller quantity of lead is alſo ſufficient in this proceſs, than in that of eliquation ; perhaps only a fifth part, or ſtill leſs ; whence, upon the whole, the loſs or waſte of lead, including the cupellation, is conſiderably leſs. Beſides this, my proceſs requires but little expence in the building or apparatus, and the expences of ſmelting are by· this means in part compenſated ; ſeveral fires for roaſting the copper-mat being rendered unneceſſary, and thus ſaved. From this diminution of expences in my new method, ſuch copper-mats may likewiſe be deprived of their ſilver, as when worked into black copper are ſo poor in ſilver, as not to repay the charges of eliquation.

By theſe means, therefore, when the quantity of fuſed copper-mat in the furnace A has ſufficiently increaſed, it will reach the communicating aperture (*a*), after having driven before it all the lead into the outward-hearth. Laſtly, as ſoon as the copper-mat has advanced to beneath the ſurface of the lead in the outward-hearth B, it will riſe through the lead by the laws of hydroſtatics, and penetrating its maſs, will place itſelf on the ſurface of the lead, continually accumulating there, until having reached the proper height, it can run off through the indentation cut in the rim of the fore-hearth.

In this ſimple manner, therefore, any required quantity of copper-mat may, by repeated charging of the furnace, paſs through a determined quantity of lead ; and may be thus deprived of its ſilver without loſs in burned lead, and the lead itſelf may at pleaſure be more or leſs impregnated with ſilver.

Experiments in the ſmall Way, to confirm the Succeſs of this Method.

In order to perform theſe operations in the ſmall way, as well to confirm the theory and practice, and at the ſame time to diſcover a method of making the neceſſary experiments which might enſure correſpondent performance in the large way, I proceeded in the following manner :

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* *Firſt waſte* is the droſs or ſcoria formed in the firſt part of the operation, when the cupelling is performed in the large way in the *refining furnace*. See *Gren's Principles of Chem.* §. 1486.----Tranſl.

<div align="right">A Heſſian</div>

A Heffian crucible muft be perforated at the bottom, and both its internal and external furfaces coated with charcoal-duft and loam. This crucible is then placed in another calice-formed crucible (kelchtutte), or into a black-lead crucible, properly cut for that purpofe, and likewife internally coated with powdered charcoal. In this it is faftened by means of a wedge made of clay, in fuch a manner that it cannot elevate itfelf during the experiment. A determinate quantity of lead, altogether free from filver, is then fufed in the interior crucible, through the aperture of which in the bottom it flows into the exterior veffel. The quantity of lead fhould be fufficient to occupy part of the inner crucible: And to protect it againft oxydation, its furface muft be covered with fome powdered charcoal in both veffels. Upon this fome powdered copper-mat, that has undergone the fecond or third roafting, is introduced into the inner crucible, and melted by a fire fuffi-ciently ftrong and brifk, fo that it may flow thin. The putting in of frefh copper-mat is to be continued, till the interior crucible can hold no more, when in thin fufion. The fluid copper-mat preffes the fufed lead in the inner crucible through its aperture at bottom into the outer, and at laft itfelf follows, and rifes up through the lead, and giving out its portion of filver during its tranfition. That portion only of copper-mat which remains in the inner veffel will continue to preferve its filver. The apparatus is then fuffered to become cold without agitation, and after breaking the veffels, the whole of the lead and copper-mat refting upon it in the outer crucible are to be collected, feparating them carefully from each other, and cleaning them from the adhering charcoal-duft. Their weights are next to be exactly determined; after which the *lead* is affayed for filver, and the *copper-mat*, deprived of its filver, is affayed for copper. A calculation may be made from the refults of the quantity of filver obtainable from a certain determinate quan-tity of copper.

If the copper-mat has not been duly roafted, and, confequently, ftill contains too much fulphur, it will diffolve a portion of filver in this procefs, and a lofs will be obferved in the remaining lead, which will be greater in proportion as the fulphur in the copper-mat is lefs faturated with copper. But if the copper-mat, by too much roafting, has been brought below the point of faturation of the fulphur with the copper, part of the copper will then feparate during the fufion, and, mixing with the lead, will occafion an increafe of weight.

On making experiments with the copper-mat from *Rothenburg*, I found that a mixture of that which had fuftained the *third* roafting heat, with an equal portion of that which had been fubjected to the *fecond* roafting, was the beft fuited, and of fuch a quality, that the effect was quite fatisfactory.

Out of many different experiments, which I have made in order to put my method to trial, and which afforded very uniform refults, I fhall in this place give an account only of the laft I have made.

Copper-mat from *Rothenburg*, mixed in equal parts of that of the fecond with that of the third roafting, was made to pafs, in the manner above explained, through 24 centners

of

of lead. The lead was again recovered with an increase of weight, not yet fully amounting to 2 *per cent.* The *work-lead* contained in the centner *two* ounces, $1\frac{7}{384}$ drachms of silver, and hence in the whole 51 *ounces,* and one and $\frac{1}{16}$ drachm. The whole quantity of the copper-mat, divested of its silver, in this operation, by means of lead, amounted to 9 centners of black copper; hence *one* centner of black copper at 110 lb. yielded 5 *ounces,* 5 and $\frac{65}{144}$ drachms of silver. When the copper, obtained from this copper-mat, was examined, it was found to contain no more than $3\frac{11}{18}$ drachms of silver, and, therefore, less than is usually the case with eliquated copper.

A more minute Explanation of this Process, as performed in the large Way.

Two conditions are essentially requisite to the management of this process. (1) The copper-mat must have been roasted in the due degree. (2) The melting-furnace must have been so constructed, that the copper-mat, while in fusion, may be enabled to pass through the body of the melted lead.

The necessity of the first condition is evident from what has been already said; namely, that when the copper-mat still contains too large a portion of sulphur not saturated with that metal, part of the lead will be dissolved by the sulphur during the fusion, and an irrecoverable loss will be thus occasioned. On the contrary, when the copper-mat has been too strongly roasted, it then no longer contains a quantity of sulphur sufficient to hold all the copper in solution. Part of the latter will, therefore, separate, and unite with the lead. But it is better to fall into the last error than the first; because the copper can be again recovered, when, as must be done of course, the scoriæ and litharge produced in the refining of the lead impregnated with copper, together with the metal imbibed by the ashes of the test, are again reduced to the reguline state; in which state they are conveyed again into the furnace with a copper-mat less roasted. The sulphur of this last again separates the copper from the reduced lead. But for the accomplishment of this condition it is necessary, that by small experiments, easy to be made, and in the manner indicated, it should be first ascertained, what degree of roasting of the copper-mat may be the most suitable to the operation. And with this view the portion to be subjected to the trial should be so managed, that an uniform degree of roasting may be kept up. According to my experiments in the small way, it seems, that copper-mat, which has been roasted the third time in the usual manner, and has once more passed by itself through the melting furnace, is apparently the best qualified for this purpose, if *pure* lead be made use of in divesting it of its silver. For though this last should happen to take up some of copper in the process, yet the copper may be recovered from it, by adding copper-mat of the second roasting to the litharge, with the first waste and metallic portion absorbed by the test at the time when these products of the refinery are afterwards reduced.

We see, therefore, that my method saves: (1) The expences of building and maintaining the house appropriated to the process of eliquation; (2) The wages expended on this account; (3) The additional charges for the fuel in the operations of eliquating, second

eliquating

eliquating (darren)* and melting the lead with the black-copper, in which it is contained ; (4) All the lofs of lead, occafioned by its fufion with the copper at its eliquation, its refining, and the fcratch-work with the brufh in cleaning ; (5) Likewife the lofs of lead poor in filver, which is obtained by eliquation from copper, and ought not to be enriched with filver in that degree, as may be done according to my method. And if it fhould be found advifeable to *refine* the *work-lead* directly in the fmelting houfe, there would then (6) be faved the charges of tranfporting the black copper to the houfe, in which it is to be eliquated.

To this may be added, that by my method of proceeding the quality of the copper is improved, which is always the chief object of the fmelting houfe. The copper obtained by eliquation is well known to contain lead, and this proportion of lead imparts to it a noxious property, diminifhes its ductility, and even that of the brafs, prepared from it. This contamination of the copper with lead cannot take place in my procefs ; becaufe by that management no lead can combine with the copper of the copper-mat made ufe of, when the fulphur of this laft has been fufficiently diminifhed in its quantity by the roafting.

II.

Obfervations on the Proofs of the Huttonian Theory of the Earth, adduced by Sir James Hall, Bart. By R. KIRWAN, ESQ. Communicated by the Author.

(*Concluded from page* 102.)

EXPERIMENT THE FIRST.

GRUNSTEN, a compound of felfpar and hornblend intimately mixed with each other, was the fubject of this experiment.

Its *colour*, black, or greenifh black, intermixed with pale reddifh brown ; both the felfpar and hornblend imperfectly and confufedly cryftalized in minute grains ; the *fracture* partly ftriated and partly foliated. *Luftre* moderate : its hardnefs 7, or almoft 8. Gives an earthy fmell when breathed upon, and frequently contains fmall fpecks of pyrites. Ibid. p. 7.

This fubftance he vitrified by a ftrong heat and fubfequent rapid cooling, p. 9. A fragment of the glafs thus produced being introduced under a narrow muffle, and heated to 21. in one minute became fo foft as to yield readily to the preffure of an iron rod, but after a fecond minute it became quite hard, though the temperature had been ftationary. The fubftance thus hardened underwent a thorough change, it loft its vitreous character, its fracture was like that of *porcelain* (that is even) and it was fufible only in a heat of 31. page 11.

* *Gren's Principles of Chem.* §. 1490.

In another experiment, *ibid.* he found this change to take place even before the glafs was in perfect fufion. For while both ends of a fragment of this glafs were fupported on refts of clay, it was found not to fink down between them until the heat was raifed to 30. In another experiment he found the confolidation, which he (improperly, as I think) calls cryftalization, to take place even while the heat was gradually increafed, and the fubftance ftill fo vifcid as to retain the original fhape of the fragments.

In another experiment, where the glafs was flowly cooled, its texture was found compleatly to refemble that of whinftone, the *fracture* was *rough*, *ftony*, and *cryftaline*, with a number of fhining facettes interfperfed through the mafs, and a few cryftals in the cavities produced by air bubbles, p. 8.

Thefe experiments may be confidered in two points of view; firft, with refpect to phenomena of confolidation in a heat either gradually increafed *above*, or gradually diminifhed *below* the heat neceffary to foften the vitreous fubftance, the lofs of the vitreous character, and the ftony appearance, affumed through flow refrigeration.

And, in the fecond place, we may examine how far the phenomena here obferved tend to countenance the Huttonian theory either of the formation of granite, trap, or bafalt, or other ftony fubftances: in this refpect only it concerns me to examine thefe experiments, yet I cannot forbear mentioning fome few reflections on the firft.

It has been obferved by all thofe who have attended to the formation of common glafs (and is, indeed, evident from the fumes that float over its furface) that from the inftant it enters into fufion, it is in a conftant ftate of decompofition, gradually becomes lefs fufible, and increafes in denfity; the fubftances that thus efcape are, in this cafe, the faline, as Bofc D'Antic has fhewn, and Macquer alfo afferts. See 1 Bofc D'Antic, 10, and 242, 213. and hence the lofs of weight which gas thus fuffers, *ibid.* 220, and 4 Macquer 261. Macquer alfo obferved, that glafs kept too long in fufion loofes its tranfparency, and becomes opaque, becaufe the flux evaporates. And he obferves, that glaffes formed of argill, lime, and gypfum, are particularly fubject to this accident. Lavoifier noticed the fame phenomenon during the fufion of felfpar even by oxygen air, namely, that the longer it was kept in fufion, the more infufible it became. Mem. Par. 1783, p. 577, which he imputed to the volatility of one of fome or other of its ingredients. And he afterwards found occafion to extend the fame remark to fteatites, and alfo to a mixture of equal parts of quartz and calcareous fpar. This increafed in fufibility of certain fubftances by a gradually increafed or continued heat, is not, therefore, a new difcovery having been already noticed: but Sir James Hall has confiderably enlarged it, by fhewing that the ftones he operated upon had re-affumed their ftony appearance, after having been in a vitreous ftate; this appearance, if I underftand him rightly, they have affumed only in confequence of flow cooling, and not merely by a heat either ftationary or gradually increafed; confolidation only being the effect of fuch treatment.

This confolidation Sir James calls *cryftalization*, a term which feems to me highly improper; for, according to every fenfe in which this term has ever been employed, whether

that

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that operation was perfect or confused, it denotes at least an union of particles previously difperfed through a liquid medium, they muft, therefore, be at liberty to move through this medium in order to coalefce, and re-unite to each other (if both they and the medium itfelf coalefce and confolidate), this action is called *coagulation*, as happens in what was called *offa Helmontii*, and the jelly formed by the *liquor filicum*; but in Sir James's experiment we find the confolidation to take place in a fragment of glafs, which ftill retained its folid ftate, and, confequently, the particles were not at liberty to move towards each other. This confolidation muft, therefore, evidently have arifen from fome internal change in the conftitution of the glaffes in which it was obferved. What thefe changes may have been I fhall now examine.

In the firft place, it is highly probable that filex, argil, and lime, and flightly oxygenated calx of iron, whatever be their affinity to each other when duly proportioned, require, like all folids, to abforb in their paffage to a liquid ftate a certain portion of latent heat; but when in fufion, and the particles of each chemically united, they require a higher degree of heat to keep them in fufion; their elective affinities promoting fufion before the union, and impeding it after the union is formed; it is thus that iron and platina, metals feparately highly infufible, contribute to each others fufion; but when fufed, become ftill more infufible, as appears by Rinm. §. 135. Sulphur and lead are feparately and eafily fufible, but when united, their fufion becomes much more difficult. Again, Dr. Kennedy has difcovered that all thefe whins contain 10 per cent. foda; and Vauquelin has lately difcovered tartarin in felfpar: in the high heats to which thefe ftones are expofed in order to vitrify them, may it not be fuppofed that thefe falts are, in fome meafure, volatized, and the compounds thus rendered lefs fufible? Though in an high heat rapidly produced, they may ftill be fufible as a fmaller proportion of foda will in that circumftance fuffice to that effect.

The next circumftance to be accounted for, is the faxification or *ftony* appearance affumed by the vitrified ftones when flowly cooled, by far the moft curious fact, for which we are indebted to the ingenuity of Sir James. To account for this change, it is proper to remark, that though whins are faid to be vitrified in a high degree of heat, yet this is not rigoroufly true, for in that cafe they fhould afford a tranfparent glafs, whofe fracture would be perfectly polifhed with a ftrong luftre, as we fee that of common glafs, whereas, in truth, they melt only into an *enamel*, nearly approaching to the perfect vitreous ftate: even the bottles made of them are nothing more; and hence their fuperior hardnefs. Their ingredients, therefore, are not uniformly diffufed through their whole mafs, but lie in the fame order and pofition as before fufion, and in effect they contain much more filex than can be compleatly vitrified by the fmall proportion of lime and argill that enter into their compofition, even though affifted by the foda; and in the next place we muft notice, that the affinity of foda to filex diminifhes in the fame ratio as the heat diminifhes, and, confequently, they feparate, if the heat be not fo fuddenly diminifhed as to impede all motion. This is evident by what happens to common glafs when flowly cooled down to the tem-

perature

perature of the atmofphere, as came to pafs in the Glafs Houfe at Leith, and conformably to this inftance the faxification might take place even after a perfect vitrefaction;—that there are unions grounded on chymical affinity, which take place to a certain degree only at certain temperatures, and are in great meafure loofened at a lower temperature, appears in the common inftance of the folution of moft falts in water, fpirit of wine, or other menftruums, greater in a high degree of heat than at a lower, and has alfo been noticed in the fufion of gold in a mafs of filver, for if the filver be very gradually cooled, the gold will feparate from it, as Homberg obferved, Mem. Par. 1713.

Now the affinity of filex to the alkali being loofened by a flight diminution of heat, the affinity of argill to the filex to which it united only as to a *compound* in the given temperature, is alfo neceffarily loofened ; that in the dry way argill unites to filex in temperatures below 150°, only in confequence of the previous union of the filex to the alkali, is clearly deduced from this fact, that if the alkali be abfent, the union will not take place in temperatures below 150, whereas it takes place by Sir James's own experiment, at temperatures below 100, when the alkali is prefent; for he found the whins fufible at 55°. It is true the whins contain lime alfo, but though the prefence of a certain proportion of lime contributes materially to the fufibility of filex and argill, yet it would be ineffectual in degrees of heat below 120°, if an alkali were not prefent to affift it, as I know by experience.

The prefence of argill contributes alfo to the diminution of the affinity of the alkali to the filiceous ingredients, as the alkali feems to have nearly as ftrong an affinity (fome think ftronger) to argill as to filex; hence it is, that all analyfts fince Bergman's time employ an alkali to loofen the intimate union of filex and argill in precious ftones.

Thofe facts being duly confidered, we fhall not be furprized at feeing the clofe vitreous texture deftroyed by the flow cooling of melted whins, (all of which contain the above ingredients) and fucceeded by the loofer texture of a mere ftony fubftance. This is the only change that takes place, if we except the minute and indeterminate cryftalizations that occur in the cavities formed by the expulfion of air, while the mafs was as yet foft ; for the facettes interfperfed through the ftone cannot be accounted cryftals, but only the rudiments of cryftalization. Thefe are formed at the inftant the affinity of the alkali is leffened, and the earths begin to affume their folid ftate. The alkali being as yet liquid, allows the earthy particles to move through it, and to form thefe incipient cryftalizations.

We are now to examine how far the ftony ftructure affumed through flow refrigeration, by ftones previoufly fufed, tends to afford any fupport to the Huttonian theory. In my opinion it affords none at all ; the utmoft effect it can produce in an unprejudiced mind, is to render the origin of whins ambiguous, by making them affume the appearance of a Neptunian origin, when in fact they owe it to fufion; but it is only an *appearance*, for natural whins are accompanied with circumftances, and contain fubftances which contradict that appearance, and prove it to be deceitful. Befides, thefe experiments have no relation whatfoever to granite, or calcareous maffes, which form the bulk of the globe, and afford

not the flighteft indication of their origin; whins, though they abound in Scotland, and fome other countries, are, in comparifon of the former, but thinly fcattered over the furface of the globe. Some refemblance betwixt them and lavas has been long noticed. I fhall now briefly mention a few of the difcriminating characters of the artificial and natural whins, which may in moft cafes prevent us from confounding them, or afcribing to them a common origin.

1. The *natural* whins, particularly Amygdaloides (vulgarly called Toadftones) frequently contain calcareous fpar and zeolyte; now as the former contains fixed air, and the latter a notable proportion of water, I hardly think Sir James, who profeffes not to agree with Dr. Hutton in all points, will allow thefe to have been vitrified or fufed.

2. The natural whins, according to Dr. Kennedy's ftatement, lofe 5 per cent. of water, and other volatile matter, when heated to rednefs. It is not faid, whether the artificial lofe any part of their weight by fuch treatment; but it is plain they would not, fince even the lavas of Catania and Piedmonte, though of ancient date, loft none, as Dr. Kennedy exprefsly notices, and has thus afforded an excellent criterion for diftinguifhing the long contefted origination of thofe fubftances.

3. As Sir James has neglected giving a *compleat* account of the external characters of the natural whins, which were the fubject of his experiments, as alfo of the regenerated, or artificial whins derived from them; and as I have not myfelf feen them, it is difficult for me to compare them with each other, and would, indeed, be impoffible, if fome account of them had not been given by Mr. Pictet, in his valuable *Journal Britannique*, copied into the 5th Vol. of the new Rozier's Journal, p. 313. It is the refult of the examination both of the natural and artificial whins by the Society of Natural Hiftory at Geneva.

As to the grunftein, No. 1. they remark, that it betrays not the leaft mark of an igneous origin, but that the whins which Sir James produced from it, had every diftinctive character of a lava, and even of a porous lava.

The Bafalt (or rather Trapp) on which the Caftle of Edinburgh ftands, is of a compact ftructure; the artificial produced from it, Sir James tells us, fo greatly refembles it both in colour and texture, that it would be difficult, or perhaps impoffible, to diftinguifh them, *but for a few minute air bubbles, diftinguifhable in the artificial.* Neptunifts will, however, confider this as a leading character of diftinction. The mineralogifts of Geneva add, that the colour of the artificial is deeper, and its hardnefs greater, than that of the natural. If the fpecific gravity, and other characters of both were given, it is probable that other differences might be perceived. It is only in thefe characters that any difference can be expected, as the internal compofition muft be the fame in both.

Of the remaining artificial whins I can give no account, their external characters having been omitted; I cannot, however, pafs over the general inferences that Sir James deduces from his experiments, namely, that " the arguments againft the fubterraneous fufion of " whinftone, derived from its ftony character, feem now to be fully refuted," for not to

repeat

repeat what has been already faid, that many of them contain fubftances whofe exiftence is incompatible with that hypothefis, I muft farther add, that the upright ftate in which many of them exift, for inftance the bafaltic pillars of Staffa, and of the Giant's Caufeway, and of many other countries, the bafis they reft on, fometimes granite, fometimes gneifs, fometimes coal or limeftone, and the total abfence of all figns of the operation of fire, forbid us to entertain any doubt of their production in the moift way. Nay, the College of Dublin now poffeffes fragments of bafaltic pillars, in which marine fhells are imbedded; if fuch evidence can be refifted, it is in vain to feek for greater.

Sir James thinks the caufe of the fluidity of lavas, which I formerly fuggefted, as ftrange and inconceivable as that of Citizen Dolomieu. Not having had the happinefs of viewing thofe ftupendous torrents, I founded my opinion on the accounts given by the moft accurate obfervers, and particularly of C. Dolomieu, who beheld, and carefully examined, every circumftance relating to them for many years. This great obferver has not thought my opinion fo inconceivable, for he has fince embraced it. " From the manner" (fays he) " in which lavas flow, it cannot be doubted, but they carry with them a fub- " ftance capable of maintaining their heat and fluidity, and contain a fubftance which " burns in contact with the atmofphere until it is confumed. This fubftance, of which " fulphur is at leaft one of the principal ingredients, if it be not the only one, bears a " ftrong refemblance in its conftitution to phofphorus, being capable of two forts of com- " buftion. This combuftion feems capable of maintaining fluidity in a bed of lava, &c." 1 New Rozier's Journ. p. 119 & 120.

Sir James fays, I have fuppofed fubftances that have left no trace of their exiftence. Other obfervers, however, difcovered thefe traces, as Dolomieu and Fabroni, in the paffages I have already quoted. Mineralogy, Vol. I. p. 397, and 1 New Roz. p. 120, 121. It is not to be expected that volatile fubftances, fuch as fulphur and petrol, fhould long remain. However, I acknowledge that the caufe of the ftony appearance which lavas after cooling exhibit, difcovered by Sir James, appears to me at prefent by far the moft probable; and that in this refpect his difcovery is of great importance to geology. But I perfift in thinking, his experiments afford no conformation of the high degrees of heat attributed to volcanos, and ftill lefs to the many hypothefes gratuitoufly heaped on each other by Dr. Hutton, or to the volcanic origin of whins or traps, for the reafons already affigned.

III.

On the Genuineness and Purity of Drugs and Medicines. By Mr. FRED. ACCUM.

(Continued from page 36.)

METALS AND METALLIC SUBSTANCES.

Mercury. Quicksilver.—Ph. L.

THE property which mercury possesses of readily uniting with most of the metals, without suffering any material change in its obvious properties, induces fraudulent dealers to adulterate this article with lead, tin, &c. If these metals are present in any confiderable quantity, the mercury will have a dull aspect, and will not run freely into round globules, but when gently moved, it forms vermicular striæ, ar tears. When agitated in a bottle, it foon becomes black, the lead and tin becoming oxidated, and may thus be separated, as has been noticed in the Philosophical Journal I. But as these metals are fo easily detected, and can only be added in a comparatively small quantity, the dealers in this article have recourse to other practices. It is a fact, that bifmuth and zinc remarkably favor the union of lead with this metallic fluid, and that even tin may then be added in a confiderable quantity. As it is impossible to free mercury from these metals completely by a mere agitation, recourse must be had to diftillation; and even then the mercury cannot be obtained perfectly pure, unless the process be very carefully managed.

Mercury free from any admixture should be totally volatile, when gently heated to the boiling point. In order to detect the presence of lead, one part must be boiled for a few minutes with twelve parts of electric acid. The fluid is then to be decanted, and examined by means of sulphuric acid. If the smaller quantity of lead be present, a few drops of this acid will render the fluid turbid, and a white precipitate will be separated. One hundred grains of this precipitate, well washed and dried, contains 72 parts of metal, and 28 of acid; the quantity of lead contained in a given quantity of mercury, may thus be accurately ascertained. But the minutest quantity of lead in the greatest quantity of mercury, can only be detected by dissolving the mercury in nitric acid, and then mixing this solution with water saturated with sulphurated hydrogenous gas. Dark brown clouds will immediately appear, and a precipitate of the fame colour will be deposited, if the fluid is suffered to rest undifturbed for a day or two, one part of lead may thus be separated from 15260 parts of mercury. The admixture of bifmuth is detected in a similar manner, by pouring the nitrous solution into diftilled water; a white precipitate will appear if this metal be present. Tin is manifested in the ufual manner, by a weak nitro-muriatic solution of gold; and zinc, like the rest of the metals, may be separated by expofing the mercury to heat.

Sweet.

Sweet Muriate of Mercury. Calomel.—Ph. L.

Calomel ought to be perfectly faturated with mercury. Compleat faturation can only be known, by boiling for a few minutes one part of calomel, and $\frac{1}{32}$ part of muriate of ammoniac in 10 parts of diftilled water. The fluid muft then be filtrated and examined by means of carbonate of pot-afh. If the calomel is well prepared, no change will take place on the addition of this re-agent; but if the preparation is imperfect, a precipitate will enfue, 47 parts of which indicate 48 of muriatic acid. 114 parts of calomel perfectly faturated with mercury, contain 97 of metal, and 17 of acid. It fhould be perfectly inodorous and taftelefs, and when rubbed in a ftone mortar with ammoniac becomes intenfely black.

Red fulphurated Oxide of Mercury. Red fulphurated Quickfilver.—Ph. L.

The cinnabar of the fhops is generally adulterated with red oxide of lead, in order to make it dry fooner when ufed for oil painting, To difcover this reprehenfible focus, one part of cinnabar is to be digefted by heat with four of acetic acid. If lead is prefent, the acid will acquire a fweetifh tafte, and by letting fall into it a few drops of fulphuric acid, fulphate of lead will inftantly be feparated. Genuine red fulphurated oxide of mercury fhould be entirely volatile in the fire, and confequently leave nothing behind it after evaporation. A compleat decompofition of its integrant parts fhould be effected, by boiling one part of it in twelve of nitro-muriatic acid, (compofed of three parts of nitric, and one of muriatic acid) and then treating the refidue in a fimilar manner, with a fufficient quantity of foda, or pot-afh, forced from carbonic acid. Cinnabar is likewife found adulterated with a mixture of chalk and dragon's blood, and cinnabar thus contaminated, is of an exceedingly fine crimfon colour. If it contains carbonate of lime, an effervefcence will enfue on muriatic acid being added, and the earth will be taken up by the acid; on adding diftilled water, and feparating the fluid, chryftals of felenite will be obtained, on adding a little fulphuric acid. The colouring matter may be compleatly extracted by digefting the refidue in ardent fpirit, and fubfequent evaporation. 100 parts of Englifh cinnabar contain generally 20 parts of fulphur, and 80 of mercury. Its fpecific gravity is then 1000.

Black Oxide of Mercury. Quickfilver with Sulphur.—Ph. L.

This mixture of quickfilver and fulphur, ought to be at leaft fo far completed, that no globules of mercury fhould be perceptible through a good microfcope. Its colour is then of a beautiful black; it is impalpable to the touch, and totally volatile by heat. It is found fophifticated with ivory black. If this be the cafe, it will leave fome afhes behind when laid on a red hot iron, or on ignited coals. If the mixture of the mercury with the fulphur has been well performed, the powder does not give a white colour to gold, when rubbed on it for any confiderable time, nor can a feparation be eafily effected by faturating it with a thick mucilage, &c. However, black oxide of mercury, which ftands thefe tefts, will not be found in commerce, nor can it reafonably be expected.

Nitrated

Nitrated Oxide of Mercury. Red Nitrated Quickfilver.—Ph. L.

Moſt of the red nitrated oxide of mercury met with in the ſhops is adulterated with red oxide of lead. This fraud may be diſcovered in the ſame manner, as in the red ſulphurated oxide of mercury. Genuine red nitrated oxide of mercury is totally volatile on being expoſed to a red-heat, and ſoluble in nitric acid, without efferveſcence, but with a developement of heat. Sulphuric acid converts it into yellow oxide of mercury (Turpeth mineral); muriatic acid forms corroſive muriate, and when completely ſaturated, ſweet muriate of mercury, or calomel *.

White Oxide of Mercury. White Calx of Quickfilver.—Ph. L.

Inſtead of pure white oxide of mercury, we frequently meet with a mixture of white oxide of mercury, and white oxide of lead, to which, not ſeldom, a conſiderable quantity of chalk is intimately mixed. White oxide of lead is diſcovered by digeſting one part of the oxide with four of acetic acid, decanting the fluid, and adding to it a ſmall quantity of ſulphuret of ammoniac, or water impregnated with ſulphurated hydrogen. An almoſt black precipitate will be formed, which on the addition of ſulphuric acid will not be re-diſſolved. The preſence of calcareous earth may be inveſtigated as directed before.

Genuine white oxide of mercury is of a ſnowy whiteneſs, taſteleſs, and inodorous; inſoluble in acids, ponderous; does not become black when rubbed with freſh prepared lime water, is totally volatiliſed by heat, and when accurately prepared, may thus be converted into calomel by mere ſublimation.

Yellow Oxide of Mercury. Vitriolated Quickfilver.—Ph. L.

Is ſeldom found adulterated. But from a careleſs and ſlovenly management during the proceſs for obtaining it, it often contains a conſiderable quantity of free ſulphuric acid. It then has a perceptible acid taſte, and diſturbs the ſolution of muriate of barytes. It ſhould be totally volatile by heat, and poſſeſs moſt of the characteriſtics of the preceding mercurial preparations.

Sweet Muriate of Mercury obtained by Precipitation. Mild Muriated Quickfilver.—Ph. L.

This preparation being preciſely the ſame as calomel, ought therefore to poſſeſs all its peculiar properties. But as it is ſpecifically lighter than calomel, it is found adulterated with ſtarch, or with white oxide of biſmuth. The firſt fraud may be detected by expoſing it in a cloſe veſſel to a red heat; as by this means the calomel will ſublime, and leave a

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* This product, if obtained according to the rules of the Royal College of Phyſicians, muſt be contaminated with a certain quantity of corroſive ſublimate, on account of the preſcribed quantity of muriatic acid to be uſed. Did the guardians of health wiſh to unite a certain quantity of this deadly poiſon with the red oxide? or did they credit the notion, that the product would have a more ſparkling appearance, which in general is looked upon as the characteriſtic of excellency, &c. ?

black

black coal behind, and the latter sophistication is detected by exposing it to heat in an open vessel, and reducing the residue by means of a little charcoal to the metallic state, for farther investigation.

Red Oxide of Mercury. Calcined Quicksilver.—Ph. L.

Is seldom found adulterated. If well prepared it is totally volatile by fire. It is soluble in sulphuric, nitric, and muriatic acid, but the acetic, oxalic, malic, and tartareous acids have no effect upon it.

Corrosive Muriate of Mercury. Muriated Quicksilver.—Ph. L.

It has been said that this product has been found adulterated with arsenic. Though this may probably seldom be the case, yet when it is suspected, one part of the corrosive sublimate may be dissolved in 24 parts of distilled water, and precipitated again by carefully adding a solution of carbonate of pot-ash. The white precipitate thus afforded, is then separated from the supernatant fluid. This fluid thus freed from all the mercury it contained, is then to be tried by means of the ammoniate of copper. A transparent blue liquid will be formed, if the corrosive muriate of mercury was free from arsenic ; but a yellowish green precipitate will appear if arsenic was present. This precipitate collected, dried, and laid on ignited coals, will soon diffuse the garlic smell peculiar to that metal. Two hundred and sixty-seven parts of this precipitate, contain one hundred and sixty-two parts of copper, and one hundred and sixty-five of arsenic.

Sixty-nine parts of corrosive muriate of mercury contain, if well prepared, generally 50 parts of mercury, 17 of solid muriatic acid, and two of water of crystalization. One part is then soluble in sixteen parts of cold water at 50°, in $3\frac{1}{2}$ of boiling water, in $2\frac{1}{2}$ of cold ardent spirit, or in a little more than its own weight of boiling ardent spirit.

Iron. Iron.—Ph. L.

The iron filings of the shops, which in general are procured from the gun-smiths, and other artificers, are never free from copper or brass. If some of the filings be dissolved in pure muriatic acid, and a polished iron be immersed into this saturated, and concentrated solution, the part of the metal in contact with the fluid, will soon become coppered, if the quantity of this metal was considerable. A smaller quantity of copper is manifested by digesting the iron filings, in water impregnated with ammoniac, which will acquire a blueish hue, if the filings contain copper. But the best way to detect the minutest quantity of copper or brass is, to dissolve one part of the suspected iron in three of nitric acid, and to decompose this solution by the addition of carbonate of pot-ash, or ammoniac freed from carbonic acid. In the first case, a greenish precipitate, and in the latter, a blue solution will be obtained, particularly if the ammoniac is added in abundance.

The practice of purifying iron filings usually made use of in the shops, by means of a magnet, is not so perfect as is generally expected. When other metals have been united

with

with the iron, as is the cafe in various folders, the mere iron filings of the compound can not be feparated in this manner. And fo likewife brafs, copper, and zinc filings, which adhere to thofe of the iron, are likewife forced to obey the magnet, and render the operation fo far ineffectual.

<div align="center">

Sulphate of Iron.   *Vitriolated Iron.*—Ph. L.

</div>

Should confift entirely of iron united to fulphuric acid. The vitriolated iron of the fhops frequently contains copper, on account of its being in general prepared, by merely re-dif-folving the common green vitriol of commerce, which abounds with copper. The beft way of detecting the prefence of copper, confifts in diffolving one part of the fulphate of iron, in three of diftilled water, precipitating this folution with carbonate of pot-afh, and then letting fall into it a little ammoniac. If the fmalleft quantity of copper is prefent, the fluid will acquire a fapphire blue colour, but no fuch effect will take place if the falt be free from copper.

The practice of obtaining this falt pure, by diffolving the common green vitriol, and fubfequent boiling with iron filings, or cryftalization in an iron veffel, is not fo fatisfactory as could be wifhed. Sulphate of zinc, which is frequently found in the common fulphate of iron of commerce, does not become feparated in this manner. A direct combination is requifite for obtaining this falt perfectly pure.

<div align="right">

FRED. ACCUM.

</div>

No. 3, *Compton Street, Soho.*

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IV.

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Extracts of Letters from H. Goodwyn, *Esq. on the Unities of Weight and Measure beft adapted to the Britifh Empire; on the new Measures of France; with a Defcription of an Engine for raifing Water.*

I APPREHEND it to be an indifputable point that a correfponding unity of weight and meafure is truly defirable in this kingdom; and that an additional benefit, beyond what the French nation poffefs from their new metrical fyftem, would arife, if that unity corre-fponded alfo with the general method in which the comparative fpecific gravities of bodies are expreffed with us, namely, by an unit, or by one thoufandth part. Now, Sir, it appears to me that we are not only in poffeffion, but in the conftant ufe of one, both for weight and meafure, as invariable as that now eftablifhed in France. I allude to the foot meafure and avoirdupoife, or (if I may be allowed the more appropriate expreffion) decade ounce weight. When I add, what I am perfuaded, by your valuable publications, you well know, that the decade ounce weight, of pure rain, or diftilled, water at 60° of heat, is generally allowed to be equal in bulk to the $\frac{1}{1000}$th part of the cubic foot; and alfo ob-

<div align="center">Y 2</div>

<div align="right">ferve</div>

ferve that by adding only 44.3511 parts out of 10000; or about $\frac{1}{225}$th part, to the content of our prefent *Winchefter meafure*, that *it* would then contain exactly 10 cubic feet. Thefe tranfitions from the prefent to fuch an improved fyftem, would be " a confum-" mation devoutly to be wifhed," and moft ardently do I wifh it to be legalized.

A ftandard meafure for the purpofes of trade, in particular, as well as for others, that would uniformly give an accurate refult, and could be eafily made, examined and afcertained, by common mechanics, which neither our prefent liquid or dry meafures evidently can, would furely be an acquifition of great value. Such an one, I humbly prefume, would be the following :—A fquare pyramid, whofe perpendicular height is exactly thrice the length of the fide of the bafe : for fuch an one, and every fection of it, made by a plane parallel to its bafe, would, in the firft inftance poffefs, and in every fubdivifion, retain thefe remarkable properties*.

1ft. Similar comparative dimenfions to thofe above given, for the original pyramid, *i. e.* every fmaller pyramid, formed by the above-mentioned parallel fection, would have its perpendicular height thrice the length of the fide of its bafe, and

2dly. The length of the fide of each bafe will always indicate, or equal the cube root of the folid content of the pyramid *e. g.* If the length of the fide of the bafe be 3. the folid content will be the cube of 3. viz. $3 \times 3 \times 3 = 27$.

Mr. Locke has fomewhere in his writings ftated it as wrong for one man to pull down the fuperftructure of another without building or erecting a better in its ftead. I fome time paft took the liberty to point out to you an error in the comparative tables of Englifh and French meafures in your Journal ; and upon Mr. Locke's principle I feel it juft to give you my calculation of the general heads of comparative tables between fuch a fyftem as is advanced in the former part of this paper, and that I apprehend to be now eftablifhed in France, premifing, that I take the length of the métre from your Journal, vol. III. page

	pieds.	pou.	lignes.
283, at	3	0	11.296,

and I take the comparative length of the Englifh, with the French foot, from data in the Connoiffance des Temps for 1795, and from the Philofophical Tranfactions for 1768, page 326.

By thefe references it will appear that the French foot is to the Englifh

<p style="text-align:center">as 1. : 1.065752004, &c.</p>

Confequently,

French foot.		Englifh decades, or 10ths of an Englifh foot.
1.	=	10.65752004

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* I have been many years in the habit of ufing a pyramid meafure to examine corn ; and am perfectly convinced that fuch a one will indicate a far more accurate refult than can arife from the manner in which corn is meafured by the bufhel.----G.

Comparative Tables, English with French.

LONG MEASURE.

Long decade.		Métre.	Métre.		Long decades.
1	=	0.03047983 ferè	1	=	{ 32.808583358, &c. or inches 39.3703 }

SQUARE MEASURE.

Square decades.		Ares.	Ares.		Square decades.
1	=	0.0000092902 ferè	1	=	{ 107640.3142 or sqr. inches 155002.052448. }

CUBE MEASURE.

Cube decades.		Litres.	Litre.		Cube decades.
1	=	0.02831637 ferè	1	=	{ 35.3152622, &c. or cubic inches 61.0247727. }

WEIGHTS.

Avoird. or decade oz.		Grammes.	Gramme.		Decade oz.
1	=	28.31637 ferè	1	=	{ 0.03531526, &c. or grains or 15.45042625. }

Long, Square, or Cube. } decades are reduced to { Long, Square, or Cube. } English inches by multiplying by } 1.2 1.44 1.728

And decade ounces are reduced to grains,

containing, { 7000. or 5760. } to the lb. { Avoird. Troy } by

multiplying the ounce by 437.5 = the number of grains in an avoirdupoise ounce.

East Smithfield, April 25, 1800.

Description of an Engine for raising Water.

THE hydrostatic machine here described was invented by me some years ago. It has been seen but by few: Mr. Watt, of Birmingham, Mr. Rennie, the civil engineer, Mr. More, the late secretary to the Arts and Sciences, Mr. George Adams, late of Fleet-street, who has made handsome mention of it in his Lectures. I have no doubt but you can readily point out many uses to which it may be applicable.

May 17, 1800.   H. G.

A Section

A Section and Deſcription of a Machine that will raiſe a Body of Water to any Height, not ex-
ceeding the Height of a Column that will counterbalance the Preſſure of the Atmoſphere (ſay
30 Feet) by the Deſcent of Part of the ſame Body of Water, through a ſomewhat greater
Height, and aided by the Preſſure of the Atmoſphere.

Let A	Plate VIII. figure 2, be a ſphere of copper, or other metal, about 1 foot 6 inches in diameter.
B	Another ſphere about 2 feet 6 inches in diameter.
C	A reſervoir of water kept conſtantly ſupplied.
D	A glaſs cap about 6 inches long, fixed on the top of A, for the purpoſe of ſeeing when the water begins to fill and has filled A.
E	The upper reſervoir into which A is to be emptied.
1,1,	A pipe, about 0.5 inches in diameter, fixed into the top of B, and riſing upwards to within about an inch of the top of D.
2	A pipe of the ſame diameter, a few feet longer than 1,1, fixed into the bottom of B, and extending downwards in a perpendicular or inclined direction.
3	A pipe of one inch and a half in diameter, fixed to bottom of A, and extending upwards through it, to within about two inches of the top of D. *Vide* fig. 3.
4,4,	A pipe about half an inch diameter fixed into the top of B, and extending upwards, through, and fixed to bottom of C.
5	A pipe of ſame diameter fixed to top of B, and terminating in and fixed to bottom of C.
6	A pipe of ſame diameter fixed into bottom of A.
7	A trumpet mouth pipe fixed to bottom of pipe 3, and extending downwards, to within about an inch of bottom of reſervoir C.

a, b, c, & *d* Are cocks fixed to the pipes.

The ſpheres, pipes, cocks, and joints, muſt all be air tight.

In order to raiſe water from reſervoir C into reſervoir E, all the cocks being turned off, proceed thus : turn on the cocks *b* and *c,* in order to fill B, and when B is full, turn off the cocks *b* and *c,* and turn on the cock *d.* The water will then begin to run from the ſphere B, and will, by means of its communication through the pipe 1, 1, with the upper ſphere A, rarify the air therein, and the atmoſphere at the ſame time preſſing on the water in C, will cauſe it to riſe through the trumpet mouth at 7, of pipe 3, and by falling over the top of it at D to fill the ſphere A. When A is full, which may be ſeen through the glaſs cap D, turn off *d,* and turn on *a, b,* and *c,* by which means A will empty into the reſervoir E, and B will be repleniſhed for another operation. Then turn off the cocks *a, b,* and *c,* and turn on the cock *d,* and repeat the operation of raiſing the water into A. But if it be re-

quired

quired to raife *one body* of water from refervoir C into refervoir E, by means of the defcent of *another body* of water through B, a communication muft be made into B, independant of the pipe 5 and cock *c*, viz. through a pipe, cock, and funnel (or refervoir) reprefented by the dotted lines, communicating with B, near the pipe and cock 5 and *c*, and then they are to be ufed in lieu of pipe 5 and cock *c*.

V.

A Memoir, in which the Queſtion is examined, whether Azote be a ſimple or compound Body?
By CHRISTOPHER GIRTANNER, *Doctor of Phyſic at Gottingen.*

(Concluded from page 140.)

I PROPOSE, in a particular work upon azote, to give a detail of the numerous experiments which I had made to difcover its nature : I fhall confine myfelf at prefent to give a general outline of thefe experiments, as well as of the inductions which I have thought might be drawn from them.

I think myfelf intitled, therefore, to affirm without felf deception, that azote gas is obtained.

1. When water is boiled in an earthen retort not glazed within, and the vapour is made to pafs through a tube of glafs, or other material;

2. When water is boiled in a retort of glafs containing clay or alumine, and the vapour is made to pafs through a tube of glafs, or other material;

3. When water is boiled alone in a retort of glafs, and the vapour is made to pafs through a tube of pipe clay;

4. When water is boiled alone in a glafs retort, and the vapour is made to pafs through a glafs tube containing clay or alumine;

5. When an earthen tube is filled with water, and this tube is included in a larger tube of glafs with fand between them, and this tube of glafs in tube of iron alfo with fand between them, and the whole is expofed to ignition * ;

6. When water is boiled in a glafs retort containing lime, and the vapour is paffed through a tube of glafs or other material ;

7. When water is boiled in a glafs retort containing pounded quartz, or filex, and the vapour is paffed through a tube of glafs, or other material ;

8. Experiment 1 fucceeds equally when the earthen retort is coated *externally* with a metallic glaze :

* This experiment, which I have not repeated, is given by Prieftley, in his experiments and obfervations on different kinds of air. Birmingham, 1790, vol. ii, page 418.

9. Experi-

9. Experiment 3 fucceeds equally when the earthen tube is coated *externally* with a metallic glaze :

10. Experiment 4 fucceeds equally when the tube is filled with lime, or pounded glafs.

The vapours of water only without azote gas are obtained,

1. When water is boiled in a glafs retort, and the fteam is paffed through tubes of glafs or porcelain ;

2. When water is boiled in an earthen retort, internally coated with a metallic glaze, and the vapours are paffed through tubes of glafs or porcelain ;

3. When water is boiled in a glafs retort filled with pounded glafs, and the vapours are paffed through the tubes of glafs or porcelain :

GENERAL REMARKS.

In order to obtain azote gas in confiderable quantity, the water muft be gently evaporated by a very moderate heat, and care muft be taken not to raife it.

It may be obferved, that in all thefe experiments without exception, that as foon as the laft drops of water is evaporated, the azote gas ceafes to be produced, notwithftanding the continuance of the heat.

RESULTS OF THE EXPERIMENTS.

Such are the facts, and the fimple enumeration of experiments, independent of all fyftem, explanation, and theory. And it follows,

1. That Meffrs. Wiegleb and Goettling were right in affirming, that water is changed into azote gas by the action of heat ;

2. That they were wrong in maintaining that this change takes place always, and in all circumftances, provided the vapours of water be in contact with ignited bodies ;

3. That Mr. Wiegleb has not proved what he advanced, namely, that the change of water into gas is owing to caloric, and that water is the bafis of every gas whatever.

4. That the Dutch chemifts were wrong in advancing, that the change of water into azote gas never happens in any cafe, and that the gas obtained is owing to the atmofpheric air having paffed through the retorts and the tubes ;

5. That there are circumftances in which water is changed into azote gas, and others in which it is not, and that it is eafy to reconcile the two oppofite parties.

The change of water into azote gas by the action of caloric and the earths being affirmed, it remains for us to difcover the folution of this problem.

I have obferved, as well as Ingenhoufz, Von Homboldt, and Van Mons, that the earths when moiftened, have the property of abforbing the oxigen of the atmofphere at the ordinary temperature. I have alfo obferved what Von Homboldt has not remarked, that they abforb oxigen in lefs time, and greater quantity, when they are heated. I found by other experiments, that the earths alfo take oxigen from water, but that it is neceffary for this purpofe, that the temperature fhould be more elevated than that of the atmofphere.

Clay,

Clay, loamy earth, or alumine, become charged with oxigen with the moft avidity, and at a temperature far below that of boiling water. Lime requires a more elevated temperature, and does not abforb fo much oxigen. Silex requires to be ignited before it will abforb oxigen, but then it feizes it rapidly enough. Baked clay alfo unites with oxigen, but requires a ftill more elevated temperature. Glazed clay does not abforb oxygen, becaufe the glaze being a metallic glafs, has no action upon that fubftance.

Having made thefe obfervations, I did not find much difficulty in explaining the phenomena of the change of water into azote gas. Recollecting the hypothefis which was thrown out by Mr. Mayer fome years ago, on the nature of azote gas, which he fuppofes to be compofed of oxigen and hydrogen, or that it is water changed into gas *, I adopted fimilar ideas refpecting the nature of this gas. Mr. Mayer having given his hypothefis merely as a conjecture, unfupported by any chemical experiment whatever, I propofed to fupply what was wanting in the eftablifhment of his conclufion.

Admitting this hypothefis, and reflecting on the fingular property which the earths poffefs of abforbing oxigen from water, I explained the before related experiments without difficulty. They are the confequence of a double affinity. The oxigen of the water partly unites with the earth, and converts it into an earthy oxide ; the reft of the oxigen uniting with the hydrogen combines with the caloric, and forms azote gas ; whence it follows, that *azote is water deprived of part of its oxigen.*

Several experiments prefent themfelves in fupport of this affertion. I fhall confine myfelf to mentioning fome of them, and fhall fpeak of the others in a fecond memoir, when I fhall have repeated them.

1. Let tubes of porcelain be procured and tried, by paffing the vapour of boiling water through them, which will be condenfed at the other extremity without the fmalleft particle of gas, excepting the air which is contained in the retort. After having in this manner afcertained that the water is not changed into gas in thefe tubes, in order to obviate the objections of thofe who imagine, that the external air paffes through thefe tubes; let one of them be filled with tin filings and placed in a fire, taking care to keep it up: after which the vapours of water are to be paffed through.

The pneumatic apparatus will be found to contain azote gas mixed with oxigen gas. The tin is changed into oxide, which oxide paffes under the pneumatic apparatus with the azote gas, and the vapours of the water.

This experiment, which was made by Mr. Van Hauch, is eafily explained according to my theory. The tin takes only part of the oxigen from the water. The reft unites with hydrogen and forms azote.

2. When the vapours of water are paffed over lead in the fame manner as in the foregoing experiment, a fimilar refult is obtained according to Mr. Van Hauch. At the commencement of the operation, the oxide of lead paffes into the inverted receiver along

* Gren's Journal der Phyfik, vol. v. page 382.

with the gas, and the watery vapours; after which the metal itself appears in the form of an extremely fine powder. The proportion of azote gas to that of the oxigen obtained is as 64 to 36.

3. When the vapours of water are passed in the same manner through a tube filled with antimony, a mixture of azote and oxigen gas is obtained, in the proportion of 89 parts of the former to 11 of the latter.

4. A porcelain tube was filled with the black oxide of manganese. The tube was exposed to a very strong fire for two hours, till the oxide had intirely ceased to afford oxigen gas. The vapours of water were then passed over this oxide thus deprived of the greater part of its oxigen. Another product of oxigen gas of considerable purity, was first obtained, and afterwards azote gas.

This experiment of Mr. Van Hauch is very instructive. The manganese first absorbed the hydrogen with avidity, and afterwards part of the oxigen of the water.

5. Through the same tube filled with the same manganese, as had served for experiment 4, the vapours of water were passed. Azote gas was obtained. The fire was kept up for near three hours, and the disengagement of azote continued as long as the vapours were passed through. It ceased when these vapours were stopped, but the disengagement of azote began again as soon as the water was again made to boil, and the vapours began to pass over the manganese. This experiment was repeated for six following days during three hours each day, and always with the same success. When the operation was ended, the manganese was found adhering to the porcelain, so that it could not be separated.

6. Dr. Pearson, in his analysis of water by the electric spark always obtained azote gas, besides the two gases which compose water.

7. The same Dr. Pearson, by burning a mixture of oxigen and hydrogen gases in a tube hermetically closed, obtained water and azote gas.

8. Dr. Priestley observed, that oxigen gas, which remains for a time in contact with the purest distilled water, becomes partly changed into azote. I verified this observation, and found as much as 0,1 of azote gas in the oxigen gas.

9. A mixture of hydrogen gas and nitrous gas, which remains for a time in contact with water, no longer burns, but is found to be changed into atmospheric air. I explain this fact by supposing, that the hydrogen has combined with part of the oxigen of the nitrous gas, or of the water, and has become changed into azote gas. This experiment, however, which was made by Mr. Link, did not succeed with me

10. Dr. Priestley found that hydrogen gas, which he had kept for a long time in contact with water, was intirely changed into azote gas. This experiment does not always succeed. It succeeded four times with Priestley. It is no doubt necessary that the water should contain oxigen gas.

11. By burning together a mixture of eleven cubic inches of hydrogen gas, and one cubic inch of oxigen gas, azote gas is obtained. This experiment of Mr. Yelin did not succeed with me.

12. When

12. When the vapours of water are paffed through a gun-barrel, which has already been ufed a number of times in this kind of experiment, and is intirely oxided within, hydrogen gas is no longer obtained, but azote; the iron being no longer capable of combining with all the oxigen which the water prefents to it. This experiment of Mr. Yeling has been confirmed to me by Mr. Mayer.

13. An experiment made by Mr. Lampadius, appears to me to afford very fatisfactory proof that oxigen is contained in azote gas. He fufed arfenic in the pureft azote prepared by the combuftion of phofphorus. The metal was fublimed, and after the experiment, he found that it was partly changed into oxide of arfenic.

14. When in the combuftion of the two gafes to produce water, the quantity of hydrogen is too great, a product of nitric acid is obtained.

15. The following experiment defcribed by Scheele, appears to me to be a new proof that azote is nothing elfe but an oxide of hydrogen. Thus it is that this famous chemift expreffes himfelf, in his treatife on Air and Fire, "I filled a bladder with air obtained " from iron filings, diffolved in vitriolic acid, and I refpired this air. After twenty in- " fpirations I found myfelf obliged to ftop. When I had recovered, I emptied my lungs " as much as poffible, and again refpired the inflammable air. After ten infpirations I " could proceed no farther. The air upon examination was no longer inflammable, and " produced no cloud in lime water; in a word it was corrupted air" (azote gas).

(To be continued.)

VI.

Further Remarks on the Preparation of Pruffiate of Pot-afh—Method of purifying Cauftic and carbonated Alcalis from Sulphate of Pot-afh. By Mr. WILLIAM HENRY.

Manchefter, June 16, 1800.

To Mr. NICHOLSON.

S I R,

IN your Journal for April laft (p. 30), I communicated a new method of preparing the pruffiate of pot-afh. The only objection I am aware of, that can be urged againft this procefs, is the expenditure of a confiderable quantity of pruffiated barytes, a falt which it may not be in the power of every one to prepare. As the fole advantage, obtained by its employment, is the decompofition, by double affinity, of fulphate of pot-afh; it has fince occurred to me, that a confiderable faving of pruffiated barytes would be gained, by firft faturating the alcali, in the common way, with Pruffian blue, and afterwards digefting the impure pruffiate of pot-afh, thus obtained, with pruffiate of barytes. To a folution of common pruffiate of pot-afh, which immediately precipitated muriate of barytes, I added,

therefore,

therefore, a fmall quantity of the barytic pruffiate, and applied a gentle heat for a few minutes. The folution, when filtered, no longer precipitated muriate of barytes (except after fome time in the manner I have pointed out, p. 32.) The purer the alcali, employed in making the pruffiate of pot-afh, the lefs, it is obvious, will be the proportion of pruffiated barytes required.

I have lately contrived a method of preparing pure and carbonated alkalis, which has many advantages over the common ones. Indeed the formation of a carbonated alcali, perfectly free from fulphate of pot-afh, is a problem of confiderable difficulty, except by very expenfive proceffes. Cryftalization is incompetent to feparate the whole of the fulphate. The lixivium of the afhes of tartar, and the alcali prepared from nitre, I have never found free from fulphate of pot-afh. By faturating the alcali with acetous acid, and then adding acetite of barytes, a pure acetite of alcali may be obtained, which, when decompofed by heat, affords a pure carbonate. But this mode is troublefome and coftly. I recommend the following, as a cheap and eafy procefs :

Render the alcali, whether vegetable, mineral, or volatile, perfectly cauftic by quick-lime ; and to the clear folution add, by degrees, a warm folution of pure barytic earth, till the precipitation ceafes. The barytes feizes the fulphuric acid ; and leaves the alcali pure, which may, if required, be afterwards faturated with carbonic acid, in any of the common modes. During its reftoration to a mild ftate, any barytes, that may remain in excefs, is alfo precipitated. If the carbonic acid, employed for faturation, be obtained from carbonate of lime by fulphuric acid, the gas fhould be previoufly paffed through a folution of carbonate of pot-afh, in order to feparate any fulphuric acid, that may happen to be attached to it. A carbonate of alcali thus prepared, when faturated with acetous acid, gives no figns of fulphuric acid, on adding acetite of barytes.

Cauftic alcali, however, purified in the above mode, ftill retains a fmall portion of lime ; for, on examining the precipitate very attentively, I found no lime in its compofition, which might perhaps have been expected in it, from fome late experiments of M. M. Guyton, Kirwan, and Hatchett, on the affinity of earths in the humid way. If a cauftic alcali be required, perfectly free from lime, as may fometimes happen in nice experiments, the alcali may be deprived of its carbonic acid, entirely from the beginning, by pure barytes.

I am, Sir,

Your obedient humble Servant,

WILLIAM HENRY.

VII.

On a New Fulminating Mercury. By EDWARD HOWARD, *Efq. F. R. S* [*].

SECTION I.

THE mercurial preparations which fulminate, when mixed with fulphur, and gradually expofed to a gentle heat, are well known to chemifts : they were difcovered, and have been fully defcribed, by Mr. Bayen †.

M M. Brugnatelli and Van Mons have likewife produced fulminations by concuffion, as well with nitrate of mercury and phofphorus, as with phofphorus and moft other nitrates ‡. Cinnabar likewife is amongft the fubftances which, according to MM. Fourcroy and Vauquelin, detonate by concuffion with oxymuriate of pot-afh §.

Mr. Ameilon had, according to Mr. Berthollet, obferved, that the precipitate obtained from nitrate of mercury by oxalic acid, fufes with a hiffing noife ‖.

SECTION II.

But mercury, and moft if not all its oxides, may, by treatment with nitric acid and alcohol, be converted into a whitifh cryftallized powder, poffeffing all the inflammable properties of gunpowder, as well as many peculiar to itfelf.

I was led to this difcovery, by a late affertion, that hydrogen is the bafis of the muriatic acid : it induced me to attempt to combine different fubftances with hydrogen and oxygen. With this view, I mixed fuch fubftances with alcohol and nitric acid, as I thought might

* Philof. Tranf. 1800, p. 204.

† *Opufcules Chimiques de Bayen*, tom. i. p. 346, and note in p. 344.

‡ *Annales de Chimie*, tom. xxvii. p. 74 and 79. (or this Journal, II. 468.)

§ Ibid. tom. xxi. p. 238. (or this Journal, I. 168.)

‖ This faft has been mifreprefented, in the introduction to a work intitled The Chemical Principles of the Metallic Arts, by W. Richardfon, Surgeon, F.A.S. Sc. (page lvii.) The author, fpeaking of the acid of forrel, fays, " Klaproth of Berlin precipitated a nitrous folution of mercury with acid of wood- " forrel, neutralized with vegetable alkali. The white precipitate, well wafhed and dried, produced a ful- " minating noife, not inferior to that of fulminating gold. Acid of fugar, perfectly neutralized by vege- " table alkali, produced the fame precipitate, which, on expofure to heat, exhibited the fame fulminating " power." I muft confefs, I have not been able to produce any fuch fulmination. Mr. Richardfon has moreover given this fuppofed difcovery to Mr. Klaproth ; whereas, Mr. Berthollet, when quoting the faft to which I fuppofe Mr. Richardfon intended to allude, obferves, " Qu'on avoit déjà donné le nom d'argent " fulminant au précipité du nitrate d'argent par l'acide oxalique, dans lequel M. Klaproth avoit découvert " la propriété de fufer avec vivacité lorfqu'on l'expofe à la chaleur. M. Ameilon avoit auffi, depuis " longtems, fait connoître que l'acide oxalique communiquoit cette propriété au mercure, quoique moins " fortement qu'à l'argent ; mais cet effet (he continues) eft fort éloigné de celui qu'on défigne par la " fulmination." *Annales de Chimie*, tom. i. p. 57.

(by predifpofing affinity) favour, as well as attract, an acid combination, of the hydrogen of the one, and the oxygen of the other. The pure red oxide of mercury appeared not unfit for this purpofe; it was therefore intermixed with alcohol, and upon both, nitric acid was affufed. The acid did not act upon the alcohol fo immediately as when thefe fluids are alone mixed together, but firft gradually diffolved the oxide: however, after fome minutes had elapfed, a fmell of ether was perceptible, and a white denfe fmoke, much refembling that from the *liquor fumans* of Libavius, was emitted with ebullition. The mixture then drew down a dark-coloured precipitate, which by degrees became nearly white. This precipitate I feparated by filtration: and, obferving it to be cryftallized in fmall acicular cryftals, of a faline tafte, and alfo finding a part of the mercury volatilized in the white fumes, I muft acknowledge I was not altogether without hopes that muriatic acid had been formed, and united to the mercurial oxide. I therefore, for obvious reafons, poured fulphuric acid upon the dried cryftalline mafs, when a violent effervefcence enfued, and, to my great aftonifhment, an explofion took place.

The fingularity of this explofion induced me to repeat the procefs feveral times; and, finding that I always obtained the fame kind of powder, I prepared a quantity of it, and was led to make the feries of experiments which I fhall have the honour to relate in this paper.

SECTION III.

I firft attempted to make the mercurial powder fulminate by concuffion; and for that purpofe laid about a grain of it upon a cold anvil, and ftruck it with a hammer, likewife cold: it detonated flightly, not being, as I fuppofe, ftruck with a flat blow; for, upon ufing 3 or 4 grains, a very ftunning difagreeable noife was produced, and the faces both of the hammer and the anvil were much indented.

Half a grain or a grain, if quite dry, is as much as ought to be ufed on fuch an occafion.

The fhock of an electrical battery, fent through 5 or 6 grains of the powder, produces a very fimilar effect: it feems indeed, that a ftrong electrical fhock, generally acts on fulminating fubftances like the blow of a hammer. Meffrs. Fourcroy and Vauquelin found this to be the cafe with all their mixtures of oxymuriate of potafh *.

To afcertain at what temperature the mercurial powder explodes, 2 or 3 grains of it were floated on oil, in a capfule of leaf tin; the bulb of a Fahrenheit's thermometer was made juft to touch the furface of the oil, which was then gradually heated till the powder exploded, as the mercury of the thermometer reached the 368th degree.

SECTION IV.

Defirous of comparing the ftrength of the mercurial compound with that of gunpowder, I made the following experiment, in the prefence of my friend Mr. Abernethy.

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* *Annales de Chimie,* tom. xxi. p. 239.

Finding

Finding that the powder could be fired by flint and steel, without a disagreeable noise, a common gunpowder proof, capable of containing eleven grains of fine gunpowder, was filled with it, and fired in the usual way: the report was sharp, but not loud. The person who held the instrument in his hand felt no recoil; but the explosion laid open the upper part of the barrel, nearly from the touch-hole to the muzzle, and struck off the hand of the register, the surface of which was evenly indented, to the depth of 0,1 of an inch, as if it had received the impression of a punch.

The instrument used in this experiment being familiarly known, it is therefore scarcely necessary to describe it; suffice it to say, that it was of brass, mounted with a spring register, the moveable hand of which closed up the muzzle, to receive and graduate the violence of the explosion. The barrel was half an inch in caliber, and nearly half an inch thick, except where a spring of the lock impaired half its thickness.

SECTION V.

A gun belonging to Mr. Keir, an ingenious artist of Camden-town, was next charged with 17 grains of the mercurial powder, and a leaden bullet. A block of wood was placed at about eight yards from the muzzle, to receive the ball, and the gun was fired by a fuse. No recoil seemed to have taken place; as the barrel was not moved from its position, although it was in no ways confined. The report was feeble: the bullet, Mr. Keir conceived, from the impression made upon the wood, had been projected with about half the force it would have been by an ordinary charge, or 68 grains, of the best gunpowder. We therefore recharged the gun with 34 grains of the mercurial powder: and, as the great strength of the piece removed any apprehension of danger, Mr. Keir fired it from his shoulder, aiming at the same block of wood. The report was like the first in Section IV. sharp, but not louder than might have been expected from a charge of gunpowder. Fortunately, Mr. Keir was not hurt, but the gun was burst in an extraordinary manner. The breech was what is called a patent one, of the best forged iron, consisting of a chamber 0,4 of an inch thick all round, and 0,4 of an inch in caliber; it was torn open and flawed in many directions, and the gold touch-hole driven out. The barrel, into which the breech was screwed, was 0,5 of an inch thick; it was split by a single crack three inches long, but this did not appear to me to be the immediate effect of the explosion. I think the screw of the breech, being suddenly enlarged, acted as a wedge upon the barrel. The ball missed the block of wood, and struck against a wall, which had already been the receptacle of so many bullets, that we could not satisfy ourselves about the impression made by this last.

SECTION VI.

As it was pretty plain that no gun could confine a quantity of the mercurial powder sufficient to project a bullet, with a greater force than an ordinary charge of gunpowder, I determined to try its comparative strength in another way.

I procured

3

I procured two blocks of wood, very nearly of the fame fize and ftrength, and bored them with the fame inftrument to the fame depth. The one was charged with half an ounce of the beft Dartford gunpowder, and the other with half an ounce of the mercurial powder; both were alike buried in fand, and fired by a train communicating with the powders by a fmall touch-hole. The block containing the gunpowder was fimply fplit into three pieces: that charged with the mercurial powder was burft in every direction, and the parts immediately contiguous to the powder were abfolutely pounded, yet the whole hung together, whereas the block fplit by the gunpowder had its parts fairly feparated. The fand furrounding the gunpowder was undoubtedly the moft difturbed: in fhort, the mercurial powder appeared to have acted with the greateft energy, but only within certain limits.

SECTION VII.

The effects of the mercurial powder, in the laft experiments, made me believe that it might be confined, during its explofion, in the centre of a hollow glafs globe. Having therefore provided fuch a veffel, 7 inches in diameter, and nearly half an inch thick, mounted with brafs caps, and a ftop cock, (fee Plate VII.) I placed 10 grains of the mercurial powder on very thin paper, laid an iron wire 149th of an inch thick acrofs the paper, through the midft of the powder, and, clofing the paper, tied it faft at both extremities, with filk, to the wire. As the inclofed powder was now attached to the middle of the wire, each end of which was connected with the brafs caps, the packet of powder became, by this difpofition, fixed in the centre of the globe. Such a charge of an electrical battery was then fent along the wire, as a preliminary experiment* had fhewn me would, by making the wire red-hot, inflame the powder. The glafs globe withftood the explofion, and of courfe retained whatever gafes were generated: its interior was thinly coated with quickfilver in a very divided ftate. A bent glafs tube was now fcrewed to the ftop-cock of the brafs cap, which being introduced under a glafs jar ftanding in the mercurial bath, the ftop-cock was opened. Three cubical inches of air rufhed out, and a fourth was fet at liberty when the apparatus was removed to the water-tub. The explofion being repeated, and the air all received over water, the quantity did not vary. To avoid an error from change of temperature, the glafs globe was, both before and after the explofion, immerfed in water of the fame temperature. It appears therefore, that the ten grains of powder, produced four cubical inches only of air.

To continue the comparifon between the mercurial powder and gunpowder, 10 grains of the beft Dartford gunpowder were in a fimilar manner fet fire to in the glafs globe: it remained entire. The whole of the powder did not explode, for fome complete grains were to be obferved adhering to the interior furface of the glafs. Little need be faid of the nature of the gafes generated during the combuftion of gunpowder: they muft have

* With Mr. Cuthbertfon's electrometer.

been,

been, carbonic acid gas, fulphureous acid gas, nitrogen gas, and (according to Lavoifier *)
perhaps hydrogen gas. As to the quantity of thefe gafes, it is obvious that it could not be
afcertained ; becaufe the two firft were, at leaft in part, fpeedily abforbed by the alkali of
the nitre, left pure after the decompofition of its nitric acid.

SECTION VIII.

From the experiments related in the 4th and 5th fections, in which the gunpowder
proof and the gun were burft, it might be inferred, that the aftonifhing force of the mer-
curial powder is to be attributed to the rapidity of its combuftion ; and, a train of feveral
inches in length being confumed in a fingle flafh, it is evident that its combuftion muft be
rapid. From the experiments of the 6th and 7th fections, it is fufficiently plain that this
force is reftrained to a narrow limit ; both becaufe the block of wood charged with the
mercurial powder was more fhattered than that charged with the gunpowder, whilft the
fand furrounding it was leaft difturbed ; and likewife becaufe the glafs globe withftood the
explofion of 10 grains of the powder fixed in its centre : a charge I have twice found
fufficient to deftroy old piftol barrels, which were not injured by being fired when full of
the beft gunpowder. It alfo appears, from the laft experiment, that 10 grains of the
powder, produced by ignition four cubical inches only of air ; and it is not to be fuppofed
that the generation, however rapid, of four cubical inches of air, will alone account for
the defcribed force ; neither can it be accounted for by the formation of a little water,
which, as will hereafter be fhewn, happens at the fame moment : the quantity formed
from 10 grains muft be fo trifling, that I cannot afcribe much force to the expanfion of its
vapour. The fudden vaporization of a part of the mercury, feems to me a principal caufe
of this immenfe yet limited force ; becaufe its limitation may then be explained, as it is
well known that mercury eafily parts with caloric, and requires a temperature of 600 de-
grees of Fahrenheit, to be maintained in the vaporous ftate. That the mercury is really
converted into vapour, by ignition of the powder, may be inferred from the thin coat of
divided quickfilver, which, after the explofion in the glafs globe, covered its interior
furface ; and likewife from the quickfilver with which a tallow candle, or a piece of gold,
may be evenly coated, by being held at a fmall diftance from the inflamed powder. Thefe
facts certainly render it more than probable, although they do not demonftrate, that the
mercury is volatilized ; becaufe it is not unlikely that many mercurial particles are mecha-
nically impelled againft the furface of the glafs, the gold, and the tallow.

As to the force of dilated mercury, Mr. Baumé relates a remarkable inftance of it,
as follows.

" Un alchymifte fe préfenta à Mr. Geoffroy, et l'affura qu'il avoit trouvé le moyen de
" fixer le mercure par une opération fort fimple. Il fit conftruire fix boîtes rondes en fer

* See Lavoifier, *Traité elementaire*, p. 527.

" fort

" fort épais, qui entroient les unes dans les autres; la derniere étoit affujettie par deux
" cercles de fer qui fe croifoient en angles droits. On avoit mis quelques livres de mer-
" cure dans la capacité de la première : on mit cet appareil dans un fourneau affez rempli
" de charbon pour faire rougir à blanc les boîtes de fer ; mais, lorfque la chaleur eut
" pénétré fuffifamment le mercure, les boîtes creverent, avec une telle explofion qu'il fe
" fit un bruit épouvantable : des morceaux de boîtes furent lancés avec tant de rapidité,
" qu'il y en eut qui pafferent au travers de deux planchers : d'autres firent fur la muraille
" des effets femblables à ceux des éclats de bombes *."

(*To be continued.*)

VIII.

Ancient Account of Parhelia feen in Cumberland. Communicated by Mr. H. SARGEANT.

To Mr. NICHOLSON.

S I R,

THE following is copied from a manufcript written in the beginning of the 17th cen-
tury, and now in the library belonging to the Grammar School of St. Bees. As it relates
to a phænomenon which is extremely rare, and in this inftance probably not elfewhere re-
corded, I fend it to you for infertion, if you think proper, in your valuable Journal, toge-
ther with a copy of the figure (Pl. VIII. Fig. 4.) which accompanies it.

I am, Sir,

Your humble Servant,

Whitehaven, 1*ft of June* 1800.   H. SARGEANT.

Upon the 8th day of May, anno 1597, in Copland, (a diftrict of Cumberland) was
feene aboute the funne, beinge fome two houres from fettinge, and entringe into a thick
cloude three parrhelii, or refemblances of the funne, the brighteft towards the north, the
two other, one towards the fouth, and the other towards the eafte, and in a thinne whitifh
cloude, hardly able to be difcerned, appeared like a rayne-bowe, a pretye diftance from
each of the faid parrhelii. Thefe towards the north and fouth did feeme to be direct,
ftreight, without any compaffinge or bowinge in towards the endes, but ftoode like direct
lynes from the fouth towards the north. The one of them turned with his brightneffe
towards the other, but the rayne-bowe, which was eaftward from the cafte parrhelios, did
bowe or compaffe eaftward directly from the funnwards, and from the parrhelios, which
feemed contrary to the nature of all other rayne-bowes which ever I fawe.

N. B. The writer is fuppofed to be the Rev. Mr. Copeland, a graduate of Oxford,
about the year 1600.

* *Chymie expérimentale et raifonné,* tom. ii. p. 393. Paris, 8°, 1773.

Account

IX.

Account of the new Electrical or Galvanic Apparatus of Sig. Alex. Volta, *and Experiments performed with the same.*—W. N.

FROM motives of delicacy to the inventor of the moſt curious and important combination hereafter to be deſcribed, I forbore giving an account of its conſtruction and effects in the laſt number of this Journal, though it has now been a ſubject of great attention among philoſophers for near two months. It appeared proper to avoid the publication of facts, originally flowing from the liberal communication of the worthy preſident of the Royal Society, until the paper of the inventor had been read to that learned body; and this could not be done till very lately, becauſe the latter part of his memoir did not arrive till long after the firſt four pages.

The Right Honourable Sir J. Banks, Bart. P. R. S. having favored my friend Anthony Carliſle, Eſq. with the peruſal and conſideration of theſe four pages at the latter end of laſt April, I had the pleaſure to look them over with him, immediately after which he conſtructed an inſtrument according to Sig. Volta's directions. The experiments made with this will form part of the preſent communication; but in the firſt place, I ſhall endeavour to relate the leading particulars of the communication made to the Royal Society, which no doubt will hereafter appear at large in their Tranſactions.

The portion of letter which firſt arrived from Sig. Volta, is dated from Como in the Milaneſe, March 20, 1800. This, together with the ſubſequent parts, contains a detailed account of the inſtrument, of which the following is one of the moſt convenient forms.

Take any number of plates of copper, or which is better of ſilver, and an equal number of tin, or which is much better, zinc, and a like number of diſcs, or pieces of card or leather, or cloth *, or any porous ſubſtance capable of retaining moiſture. Let theſe laſt be ſoaked in pure water, or which is better, ſalt and water, or alkaline lees. The ſilver or copper may be pieces of money †. Build up a pile of theſe pieces; namely, a piece of ſilver, a piece of zinc, and a piece of wet card: then another piece of ſilver, a piece of zinc, and a piece of wet card: and ſo forth, in the ſame order (or any other order, provided the pieces ſucceed each other in their turn) till the whole number intended to be made uſe of is builded up. The inſtrument is then completed.

In this ſtate it will afford a perpetual current of electricity, through any conductor communicating between its upper and lower plates; and if this conductor be an animal, it will receive an electrical ſhock as often as the touch is made, by which the circuit is com-

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* Woollen or linen cloth appear to be more durable, and more ſpeedily ſoaked than card.

† Moſt of our philoſophers have uſed half crowns for the ſilver plates. The zinc may be bought at 8d. per lb. at the White Lion in Foſter Lane, and caſt in moulds of ſtone or chalk. A pound makes twenty thick pieces of the diameter of half a crown, or 1.3 inches diameter.

pleted.

pleted. Thus if one hand be applied to the lower plate, and the other to the upper, the operator will receive a shock, and that as often as he pleases to lift his finger and put it down again.

This shock resembles the weak charge of a battery of immense surface, and its intensity is so low, that it cannot make its way through the dry skin. It is, therefore, necessary that a large surface of each hand should be well wetted, and a piece of metal be grasped in each, in order to make the touch, or else that the two extremities of the pile should communicate with separate vessels of water, in which the hands may be plunged.

The commotion is stronger the more numerous the pieces. Twenty pieces will give a shock in the arms, if the above precautions be attended to. One hundred pieces may be felt to the shoulders. The current of electricity acts on the animal system while the circuit is complete, as well as during the instant of commotion, and the action is abominably painful at any place where the skin is broken.

That the energy of the apparatus is the effect of an electric stream or current, is proved by the condenser with which Sig. V. ascertained the kind of the electricity and obtained its spark. He finds the action strongest, or most pungent, on wounds on the minus side of the apparatus, or where the wounds give out electricity, a fact also observable in the common electric spark.

The theory of the learned inventor, if I rightly apprehend him, is, that it is a property of such bodies as differ in their power of conducting electricity, that when they are brought into contact they will occasion a stream of the electric matter. So that if zinc and silver be made to communicate immediately by contact, there will be a place of good conducting energy; and if they be made to communicate mediately by means of water, there will be a place of inferior conducting energy: and wherever this happens there will be a stream or current produced in the general stock of electricity. This is not deduced as the consequence of other more simple facts; but is laid down as a general or simple principle grounded on the phenomena.

As the current of electricity will be resisted by the different conductors, he remarks that the metals may touch in a single point, or be soldered together; but that the humid surfaces must be more extended.

By many experiments, he finds that the consequences are the same whether the zinc and silver touch each other, or whether the communication be made by several different metals, provided the water be in contact with the zinc and the silver only.

Where zinc is used, salt water is preferable to alkaline lees, but the contrary when tin is made use of instead of the zinc.

The effect is much increased by elevation of temperature.

He was surprized to find that the galvanic flash of light was no greater with this apparatus than with a pair of plates; but it was produced when the conductor of the circuit was applied to any part of the face, or even to the breast. The strongest action was when the touching plate was held between the teeth, so as to lie upon the tongue. In this case

4   the

the lips and tongue were convulsed, the flash appeared before the eyes, and the taste was perceived in the mouth.

Two blunt probes were inserted in the ears, and the shock passed through the head, after which the communication was kept up. A peculiar sound, like crackling or boiling, was heard; but the author did not think it prudent to make this experiment repeatedly.

The sense of smell could not be excited, because, as Sig. V. remarks, this electricity cannot be made to diffuse itself in the air.

As the discs become dry, and lose their power, Sig. V. endeavoured to prevent this effect by inclosing the column in wax or pitch, and in this he has so far succeeded, that he has fitted up two columns of twenty pieces each, which have acted well for some weeks, and he hopes will for months.

The combination, which he thinks the most instructive, consists of a row of glasses or cups (not of metal) containing warm water or brine. Into each of these is plunged a plate of zinc and another of silver, not touching each other. From these plates respectively proceed tails or prolongations, which communicate with or touch the plates of the outer glasses in such a manner, that the zinc of the first cup communicates with the silver of the second; the zinc of the second with the silver of the third; the zinc of the third, &c. progressively and regularly through the whole row. The communication between the first and last glasses gives the shock, &c. The plates in the fluid are directed to be about an inch square; but the contacts above the water may be as small as the operator pleases.

Sig. Volta makes honourable mention of my conjectural theory of the torpedo *. After remarking that my inductions were the most probable that the existing theory of electricity could at that time afford, he proceeds to make various objections needless to be here detailed, and then offers his own new and striking apparatus as more nearly resembling the torpedinal organ. I need not anticipate the reader in the happy points of resemblance between their structure and effects.

Thus far I have followed this able philosopher; who, to his former researches into the nature and laws of electricity, has now added a discovery which must for ever remove the doubt whether galvanism be an electrical phenomenon. But I cannot here look back without some surprize, and observe that the chemical phenomena of galvanism, which had been much so insisted on by Fabbroni †, more especially the rapid oxidation of the zinc, should constitute no part of his numerous observations.

On the 30th of April, Mr. Carlisle had provided a pile consisting of 17 half crowns, with a like number of pieces of zinc, and of pasteboard, soaked in salt water. These were arranged in the order of silver, zinc card, &c. which order I shall denote by saying, that the silver was undermost, that is to say, under the zinc; and I make this remark because some philosophers have used the expression that the silver was undermost when they used the order of silver, card zinc, &c. which, as the reader will easily perceive, is contrary to the order here spoken of. This is of no consequence to the effect, though it is material to a

* Philosophical Journal, I. 358.   † Philosophical Journal, IV. 120.

clear

clear underſtanding of the terms we uſe. This pile gave us the ſhock as before deſcribed, and a very acute ſenſation wherever the ſkin was broken. Our firſt reſearch was directed to aſcertain that the ſhock we felt was really an electrical phenomenon. For this purpoſe the pile was placed upon Bennett's gold leaf electrometer, and a wire was then made to communicate from the top of the pile to the metallic ſtand or foot of the inſtrument. So that the circuit of the ſhock would have been through the leaves, if they had diverged. But no ſigns of electricity appeared. Recourſe was then had to the revolving doubler, deſcribed at page 95 of our preſent volume. The plate A was connected with the top of the electrometer and the ſilver end of the pile; and the plate B and ball were made to touch the top of the ſyſtem by an uninſulated braſs wire. The doubler had been previouſly cleared of electricity by twenty turns in connection with the earth. The negative divergence was produced in the electrometer. Repeated experiments of this kind ſhewed that the ſilver end was in the minus, and the zinc end in the plus ſtate.

In all theſe experiments it was obſerved, that the action of the inſtrument was freely tranſmitted through the uſual conductors of electricity, but ſtopped by glaſs and other non-conductors. Very early in this courſe, the contacts being made ſure by placing a drop of water upon the upper plate, Mr. Carliſle obſerved a diſengagement of gas round the touching wire. This gas, though very minute in quantity, evidently ſeemed to me to have the ſmell afforded by hydrogen when the wire of communication was ſteel. This, with ſome other facts, led me to propoſe to break the circuit by the ſubſtitution of a tube of water between two wires. On the 2d of May we, therefore, inſerted a braſs wire through each of two corks inſerted in a glaſs tube of half an inch internal diameter. The tube was filled with New river water, and the diſtance between the points of the wires in the water was one inch and three quarters. This compound diſcharger was applied ſo that the external ends of its wire were in contact with the two extreme plates of a pile of thirty-ſix half crowns with the correſpondent pieces of zinc and paſteboard. A fine ſtream of minute bubbles immediately began to flow from the point of the lower wire in the tube, which communicated with the ſilver, and the oppoſite point of the upper wire became tarniſhed, firſt deep orange, and then black. On reverſing the tube, the gas came from the other point, which was now loweſt, while the upper in its turn became tarniſhed and black. Reverſing the tube again, the phenomena again changed their order. In this ſtate the whole was left for two hours and a half. The upper wire gradually emitted whitiſh filmy clouds, which, towards the end of the proceſs, became of a pea green colour, and hung in perpendicular threads from the extreme half inch of the wire, the water being rendered ſemi-opaque by what fell off, and in a great part lay, of a pale green, on the lower ſurface of the tube, which, in this diſpoſition of the apparatus, was inclined about forty degrees to the horizon. The lower wire of three quarters of an inch long, conſtantly emitted gas, except when another circuit, or complete wire, was applied to the apparatus; during which time the emiſſion of gas was ſuſpended. When this laſt mentioned wire was removed, the gas re-appeared as before, not inſtantly, but after the lapſe of four beats of a half ſecond

clock

clock standing in the room. The product of gas, during the whole two hours and a half, was two-thirtieths of a cubic inch. It was then mixed with an equal quantity of common air, and exploded by the application of a lighted waxed thread.

It might seem almost unnecessary to have reversed the order of the pile in building up, as reversing the tube must have answered exactly the same purpose. We chose, however, to do this, and found that when the zinc was at the bottom, its effects were reversed, that is to say, the gas still came from the wire communicating with the silver, &c.

We had been led by our reasoning on the first appearance of hydrogen to expect a decomposition of the water; but it was with no little surprize that we found the hydrogen extricated at the contact with one wire, while the oxigen fixed itself in combination with the other wire at the distance of almost two inches. This new fact still remains to be explained, and seems to point at some general law of the agency of electricity in chemical operations. As the distance between the wires formed a striking feature in this result, it became desirable to ascertain whether it would take place to greater distances. When a tube three quarters of an inch in diameter, and thirty-six inches long, was made use of, the effect failed, though the very same wires, inserted into a shorter tube, operated very briskly. The solicitation of other objects of enquiry prevented trial being made of all the various intermediate distances; but from the general tenor of experiments, it appears to be established, that this decomposition is more effectual the less the distance between the wires, but that it ceases altogether when the wires come into contact.

May 6.—Mr. Carlisle repeated the experiment with copper wires and tincture of litmus. The oxidating wire, namely, from the zinc side, was the lowest in the tube; it changed the tincture red in about ten minutes as high as the upper extremity of the wire. The other portion remained blue. Hence it seems either an acid was formed, or that a portion of the oxigen combined with the litmus, so as produce the effect of an acid.

It may be here offered as a general remark, that the electric pile with card, or with woollen cloth, continues in order for about two days, or scarcely three; that from a series of glasses set up by Mr. Carlisle, as well as from the pile itself, it appears that the same process of decomposition of water is carried on between each pair of plates, the zinc being oxided on the wet face, and hydrogen given out; that the common salt is decomposed, and exhibits an efflorescence of soda round the edges of the pile, extruded, most probably, by the hydrogen: and that on account of the corrosion of the faces of the zinc, it is necessary to renew them previous to each construction of the pile. This may be done by scraping or grinding. I found it most convenient to lay the piece in a hole in a board, and give it a stroke with a float file, or file of which the teeth are not crossed. It might, perhaps, be less troublesome to clean them with diluted muriatic acid; but this I have not tried.

As the ample field of physiological research to which Mr. Carlisle's attention is directed, and the multiplicity of my own avocations, rendered it less convenient for us to pursue

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our enquiries together, I conftructed an apparatus for my own ufe. Zinc was laminated to the twenty-fourth part of an inch in thicknefs, and pure filver to the one-thoufandth part of an inch, that is to fay, as thin as our flatting mills can bring it.

Of thefe metals I made two fets, namely, fixteen pieces of filver of two inches in diameter, and fixteen pieces of 1.8 inch diameter, with their correfpondent plates of zinc and wetted card. The fmall pile was firft prepared, and whether it were that thefe thin pieces were more difpofed to admit the water between the metallic faces of contact, or from whatever other caufe it may have arifen, it did not appear by any experiment, that the whole fet, though fo greatly exceeding the pile of half-crowns in furface, was capable of doing more in the decompofition of water, or in communicating the fhock. But this, with other facts, feems to fhew, that the repetition of the feries is of more confequence to this action, than the enlargement of furface; and alfo that the thicknefs of the plates, though it may be attended with convenience, moft probably affords no addition to the force. I muft alfo add, that I have no reafon to recommend my pile, though at firft fight it feemed to poffefs cheapnefs and convenience. The plates of zinc are too thin to bear frequent cleaning or renewing after corrofion of the furface, and the filver, though it is fcarcely acted on in this fituation, is too thin to be conveniently wiped or handled.

The fpontaneous electricity of the doubler prefented an objection to the ftrict fidelity of its refults; whence I thought it defirable to give my pile a trial with the condenfer. The foot or ftand of my electrometer is a brafs plate truly flat, and 3.8 inches in diameter. A piece of thin Perfian filk was tied fmoothly upon the face of this plate, and it was then placed upon another brafs plate, upon which it was moved about horizontally, in order to accumulate electricity by friction; the electrometer itfelf being ufed as the handle by grafping the top. It was found that this treatment produced very weak figns of electricity when the electrometer was lifted up. The lower brafs plate was then placed on the top of the fmall pile, and the condenfing electrometer placed upon it. A communication was then made, by means of a wire from the lower or filver end of the pile to the upper plate of the condenfer, or foot of the electrometer. In this fituation it is evident, that the charge of the pile was employed in producing oppofite ftates of electricity in the condenfer, which would be fhewn when the plates came to be feparated. The wire of communication being taken away, the electrometer was lifted, and the leaves diverged and ftruck. It became neceffary, therefore, to repeat the experiment, taking care to lift the electrometer more gradually. The divergence took place as before, and it was increafed by prefenting excited fealing wax towards the bottom of the electrometer. And as the top of the pile had by compenfation diminifhed the fame divergence, it is clear that the electricity of the top of the pile, *viz.* of the zinc, was contrary to that of fealing wax; that is to fay, the zinc was in the plus ftate. After a number of repetitions of this experiment with the fame invariable refult, the pile was then carefully overfet, without difturbing the relative arrangement of its parts; fo that the zinc was now at the bottom, and the filver at the

top.

top. The electricity of the silver was then tried a number of times, by precisely the same process as before, and it exhibited an equal degree of intensity, but it was minus or negative. In one of these experiments, I certainly saw the spark at the time of completing the circuit, and afterwards with the same pile, when I was expressly looking for it. But it is less necessary to dwell on these facts, as the stronger combinations have exhibited this effect with much greater perspicuity.

The decomposition of water, and oxidation of metallic wire, gave birth to a variety of speculations and projects of experiments. Among others it became a question, what would be the habitude of metals of difficult oxidation. Two wires of platina, one of which was round, and one fortieth of an inch in diameter, and the other nearly of the same mass, but flatted to the breadth of one twenty-fifth of an inch, were inserted into a short tube of $\frac{1}{4}$ of an inch inside diameter. When placed in the circuit, the silver side gave a plentiful stream of fine bubbles, and the zinc side also a stream less plentiful. No turbidness nor oxidation, nor tarnish appeared, during the course of four hours continuance of this operation. It was natural to conjecture, that the larger stream from the silver side was hydrogen, and the smaller oxigen. Thick gold leaf was tried with the same effects. A wire of brass was then substituted instead of one of the slips of gold. When the brass was on the minus, or silver side, the two gases were extricated for two hours, without oxidation as before; but when the brass was, by reversing the tube brought to the plus side, it became oxided in the same manner as if both the wires had been brass. When the slips of gold were long subjected to this action, the extremity of the slip communicating with the zinc, acquired a coppery or purpleish tinge, which was deepest near the end. Whether this arose from oxidation of the gold, or of the copper, of which gold leaf contains about a seventieth part, cannot from this experiment be decided.

The simple decomposition of water by platina wires without oxidation, offered a means of obtaining the gases separate from each other. With this intention, Mr. Carlisle's pile of thirty-six was combined with my two sets of sixteen repetitions. His pile was built with the zinc uppermost, and mine in the reverse order; so that by connecting the upper plates the whole constituted one range, and the communications could be made from the bottom of the one to the bottom of the other. The two platina wires were made to protrude out of two separate tubes, each containing a little water, and through the opposite corks of each were passed copper wires of communication. These tubes were slightly greased on the outside to prevent their becoming damp; and in this state the extremities, armed with the platina, were plunged in a shallow glass vessel of water, in which two small inverted vessels, quite full of water, were so disposed, that the platina of one tube was beneath one vessel, and the platina of the other tube was beneath the other, the distance between their extremities being about two inches. The copper wires of these tubes respectively were made to communicate with the extremities of the intire pile of sixty-eight sets. A cloud of gas arose from each wire, but most from the silver, or minus side. Bub-

bles were extricated from all parts of the water, and adhered to the whole internal surface of the veffels. The procefs was continued for thirteen hours, after which the wires were difengaged, and the gafes decanted into feparate bottles.　On meafuring the quantities, which was done by weighing the bottles, it was found, that the quantities of water difplaced by the gafes, were refpectively, 72 grains by the gas from the zinc fide, and 142 grains by the gas from the filver fide; fo that the whole volume of gas was 1.17 cubic inches, or near an inch and a quarter.　Thefe are nearly the proportions in bulk, of what are ftated to be the component parts of water.　The gas from the zinc fide, being tried with one meafure of nitrous gas, contracted to 1.25, and did not contract more by the addition of another meafure; the gas from the filver fide by the fame treatment contracted to 1.6.　The air of the room, on trial, contracted to 1.28.　From the fmallnefs of the quantity no attempt was made to detonate the air from the zinc fide, but a portion of that from the filver fide, being mixed with one third of atmofpheric air, gave a loud detonation.

Upon the above it may be remarked, that it does not feem probable that oxigen was afforded by both wires, but that they were mixed by the circumftances of the experiment. For the gafes being extricated in extremely minute bubbles beneath the inverted veffels, caufed a flow afcending current confifting of water mixed with thofe bubbles, many of which were undoubtedly too fmall to be difcerned.　This afcending current gave out as much of its gas at the top of the veffel, as had time to conglomerate; but the extremely minute bubbles would return in the defcending current, and be repeatedly carried up before this effect could take place.　Such a continual circulation, or ftream, the lower part of which paffed down into the faucer, muft at length have occafioned the whole mafs of fluid to become replete with thefe minute bubbles, which would break at the open furface, and be loft, or attach themfelves to the fides of the veffel, as was feen to be the cafe.　What proportion may have thus difappeared is uncertain; but it is highly probable, that one confequence of the imperfections of our apparatus, was to occafion both the inverted veffels to become receptacles for the gafes from both wires indifcriminately; though moft plentifully in each, from the wire immediately beneath its mouth.　If this reafoning may be admitted for the prefent, till the experiment is repeated in clofed veffels, it will be fair to reckon the whole diminution on both the quantities.　The whole diminution was 1.15, whence it would follow, that the purity of the oxigen eftimated in Prieftley's manner, would be expreffed by the number 0.85.

On account of the length of this communication, I fhall at prefent forbear to enter into any confiderations of theory, but fhall conclude with a concife mention of the effects of a pile of one hundred half crowns, and a chemical incident, which appears to be the moft remarkable of thofe which I have yet obferved.

The pile was fet up with pieces of green woollen cloth foaked in falt water.　It gave fevere fhocks, which were felt as high as the fhoulders.　The tranfition was much lefs forcible through a number of perfons, but it was very perceptible through nine.　The

fpark

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spark was frequently visible when the discharge was made in the dark, and a gleam of light was also, in some instances, seen about the middle of the column at the instant of the explosion. The assistants were of opinion that they heard the spark.

The extrication of the gases was rapid and plentiful by means of this apparatus. When copper wires were used for the broken circuit, with muriatic acid diluted with 100 parts of water in the tube, no gas, nor the least circulation of the fluid was perceived, when the distance of the wires was two inches. A short tube, with two copper wires very near each other in common water, was made part of the circuit, and shewed by the usual phænomena, that the stream of electricity was rapidly passing. The wires in the muriatic acid were then slided within a third of an inch of each other. For the sake of brevity, I avoid enumerating the effects which took place during several hours, and simply state; that the minus wire gave out some hydrogen during an hour, while the plus wire was corroded, and exhibited no oxide; but a deposition of copper was formed round the minus, or lower wire, which began at its lower end: that no gas whatever appeared in this tube during two hours, though the deposition was going on, and the small tube shewed the continuance of the electric stream; and that the deposition at the end of four hours formed a ramified metallic vegetation, nine or ten times the bulk of the wire it surrounded.

In this experiment it appeared, that the influence of electricity increasing the oxidability of the upper wire, and affording nascent hydrogen from the lower, caused the latter to act as the precipitant of a solution of one and the same metal.

We are in want of a measure of the intensity of the action of these machines. Will this be derived from the quantities of water decomposed, or of gas extricated under like circumstances in given times? Or from any change of temperature? Or what other commensurate incident?—Mr. Carlisle has not found that the water in the tube, while under this agency, did produce the slightest effect on a very small and delicate thermometer.

X.

Some Experiments and Observations on Galvanic Electricity. By Mr. W. CRUICKSHANK, Woolwich. Communicated by the Author.

IN subjecting a number of fluids to the action of galvanism, several facts have been discovered, which to me, at least, are perfectly new, and which appear to throw some light on the nature and powers of this new influence.

I shall, therefore, without any further apology, give a brief detail of some of the most important, hoping that they may prove acceptable to those who are employed in the same pursuits.

I shall

I shall not give any particular account of the apparatus employed, being a pile, and not differing materially from that in use. I shall only just observe, that it consisted of plates of zinc and silver, of about 1.6 inches square, and that the number of each employed in the following experiments varied from 40 to 100, according to the power required.

I found that a solution of the muriate of ammonia answered better for moistening the interposed papers than common water.

When the machine was in full action, sparks, which were perfectly visible in the day time, could be taken at pleasure, by making a communication in the usual way between the extremities of the pile, and a small report or snap could be heard; the shock given at that time was very strong, and a gold leaf electrometer, placed in the circle of communication, was very sensibly affected: these circumstances, some of which I believe have been already ascertained by Messrs. Nicholson and Carlisle, shew the strong resemblance of this influence to electricity. These gentlemen have likewise discovered that galvanism decomposes water with much greater facility than electricity, but with phenomena somewhat different.

Experiment 1.—A quantity of common water was introduced into a glass tube, being confined at each end by corks, but perfectly at one by a cement of rosin and bees'-wax: pieces of silver wire were passed through the corks, and brought within an inch of each other in the fluid, their other extremities being at the same time connected with these of the machine or pile, one with the lower zinc plate, and the other with the upper silver plate. In future, to avoid circumlocution, I shall call the wire attached to the silver plate, the silver wire, and the other the zinc wire. The tube was then placed upright in a cup containing water, with the uncemented end downwards. As soon as the communication was made between the extremities of the pile by the wires, a quantity of small air bubbles began to ascend from the end of the wire connected with the silver, as observed by Messrs. Nicholson and Carlisle; but a white cloud at the same time made its appearance at the one proceeding from the zinc, or the zinc wire. This cloud gradually increased, and assumed a darker colour, and at last it became purple, or even black. A very few air bubbles were likewise collected upon and ascended from this wire, but when the machine was in full force, a considerable stream could be observed.

The gas was collected, and found to be a mixture of hydrogene and oxygene, in the proportion of three parts of the former to one of the latter. No great dependence, however, was placed upon this in point of accuracy. The zinc wire was found to be much corroded, and looked as if a considerable portion of it had been dissolved. As the cloud which was formed around this wire became purple on exposure to the light, I suspected it might be luna cornea, or muriate of silver proceeding from the silver, which had been somehow dissolved, and afterwards precipitated in this state, by the muriatic salts in the common water. This led to the following experiments:

Experiment 2. The glass tube was now filled with distilled water, to which a little tincture of litmus was added, when the communication was made by the wires as in the former.

mer.

mer experiment, a quantity of gas arofe from both wires, but in the greateft quantity from that connected with the filver. In a few minutes a fine red line, extending fome way upwards, was perceived at the extremity of the zinc wire; this increafed, and in a fhort time the whole fluid below the point of this wire became red; the fluid, however, above the filver wire, looked of a deeper blue than before, the flight tinge of purple being deftroyed.

Experiment 3. I next filled the tube with diftilled water, tinged with the tincture of Brazil wood; it was no fooner placed in the circle of communication, than the fluid furrounding the filver wire, particularly towards its extremity, became purple, and this tinge increafed fo faft, that the whole fluid furrounding this wire, and occupying the upper part of the tube, foon affumed as deep a colour, as could be produced by ammonia.

The portion of the fluid in contact with the zinc wire became very pale, and almoft colourlefs, nor could the purple tinge extend below its upper extremity. From thefe experiments it would appear, that an acid, probably the nitrous, is produced at the wire proceeding from the zinc, and an alkali, probably ammonia, at that in contact with the filver. Thefe facts fufficiently explain the action upon the filver wire, and the nature of the whitifh cloud proceeding from it, and afterwards becoming purple. When lime-water was employed inftead of common, or diftilled water, the wire was likewife acted upon, but in a lefs degree, and the cloud had at firft an olive colour, exactly refembling the precipitate of filver by lime-water.

The quantity of filver diffolved or corroded, if I may ufe the expreffion, in thefe experiments, was very confiderable, and where common, or diftilled water had been employed, a fmall portion of it remained in folution, which was difcovered by the addition of the muriatic acid. Indeed a much larger quantity would probably have been fufpended, had it not been for the alkali generated at the fame time, and which manifeftly produced a precipitate at, or near, the upper extremity of the zinc wire, where after a certain time, a dark zone or ftratum was always formed.

Experiment 4. It is a well known fact, that hydrogene gas when heated, or in its nafcent ftate, reduces the calces of the metals; I expected, therefore, that by filling the glafs tube with a metallic folution, I might be enabled to feparate the hydrogene from the oxigen gas, and thus procure the latter in its fimple or pure ftate. With this view the tube was filled with a folution of the acetite of lead, to which an excefs of acid was added, to counteract the effects of the alkali. When the communication was made in the ufual way, no gas could be perceived, but after a minute or two, fome fine metallic needles were perceived at the extremity of the wire connected with the filver. Thefe foon increafed, and affumed the form of a feather, or rather, that of the cryftals of the muriate of ammonia. The lead thus precipitated was perfectly in its metallic ftate, and very brilliant; a little gas efcaped from the wire connected with the zinc, and it was confiderably corroded as ufual.

A folution of the fulphate of copper was next employed, and with the fame refult, the

4.   copper

copper being precipitated in its metallic form by the wire connected with the filver. In this inftance the metal did not cryftallize, but formed a kind of button at the end of the wire, which adhered fo completely to the filver, that it was found impoffible to feparate it.

The moft beautiful precipitate, however, was that of filver from its folution in the nitrous acid. In this cafe, the metal fhot into fine needle-like cryftals articulated, or joined to each other, as in the Arbor Dianæ.

What became of the oxygene gas ufually produced in thefe experiments?

Experiment 5. A quantity of pure water mixed with diftilled vinegar was introduced into the tube, and placed in the circle of communication; fome gas was difengaged from the filver wire, but no cloud appeared at the extremity of the zinc. After fome time, however, a quantity of metallic filver was precipitated by the filver wire, and this precipitate at laft became very copious; a perfectly fimilar effect was produced, when the tube was filled with very dilute fulphuric acid; in thefe cafes the precipitated filver had the appearance of fhining fcales, like that thrown down by copper in the ufual way. It may be proper to obferve, that in all thefe precipitations and reductions, nothing but wires of pure filver were employed. The refults in this laft experiment were exactly what was expected; the vinegar prevented the alkali from precipitating the filver, diffolved by the generated acid; in confequence of which, when a fufficient quantity of the metal was taken up, it was again thrown down by the filver wire in its metallic form.

Experiment 6. A folution of the muriate of ammonia being introduced into the tube, and expofed to this influence, a little gas was difengaged from the filver wire, while the zinc one was incrufted with a fubftance which foon became black, and was found to be luna cornea. The liquor which remained in the tube after the operation had been finifhed, was highly alkaline, and fmelled ftrongly of ammonia; common falt was decompofed in a fimilar manner. This experiment accounts for the decompofition of the muriate of foda and ammonia, which always takes place when the papers in the pile are moiftened with a folution of thefe falts.

A folution of the nitrate of magnefia appeared to be likewife decompofed by this procefs, for after fome time, a white powder refembling magnefia, was precipitated on the furface of the filver wire; very little gas was difengaged.

Experiment 7. In order to afcertain how far this influence might be carried, provided the circle of communication was complete, two tubes were employed, and connected by a filver wire paffing through corks as in the figure, where A, B, reprefent the two tubes, and C the connection wire; the tubes were filled with water and fecured by corks; two other wires D and E, being then paffed through thefe corks, the arc D was connected with the filver, and the other with the zinc, at the extremity of the pile. A quantity of gas as ufual was difengaged at the extremity of the filver wire D in A, and the portion of the connecting wire C in the fame tube was partly diffolved, and as mentioned in experiment 1; but the other portion of the fame wire in the tube B gave out gas, while the communicating zinc

zinc wire E was corroded. And I make no doubt that a fimilar effect would be produced, if any number of tubes were connected in a fimilar manner, by which means a large quantity of gas might be procured in a fhort time.

Befides filver wires, I likewife employed thofe of copper or iron, and it did not appear that thefe were more corroded or acted upon than the filver; indeed, in fome of the above experiments, not lefs than half, or three quarters of an inch of the wire was intirely confumed. The copper wire connected with the zinc gives out a greenifh blue fubftance refembling the nitrate of copper with excefs of the metal, or when part of the acid has been expelled by heat, &c. In examining the gas which was procured at different times, I always found it mixed with a little oxygen gas, but fometimes this did not exceed $\frac{1}{8}$ of the whole in bulk; however, I paid but little attention to this part of the procefs, for as my wires were always corroded, no conclufion with regard to the compofition of water could be drawn from it.

At prefent I fhall only further obferve, that by making the galvanic influence pafs through a quantity of diftilled water confined in a tube over mercury, for about 48 hours, a manifeft diminution of the fluid was perceived.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Magnetic Dip at Prince of Wales's Ifland.

MR. George Thomas Staunton, in a letter to his father, Sir George Staunton, Bart. dated Prince of Wales's Ifland, Nov. 4, 1799, ftates, that " our landing at this place has " enabled me to make ufe of the dipping needle, which I brought on fhore, and found " that by the means of feveral obfervations, the fouth pole of the needle dipped five de- " grees ten minutes."

Infufibility of Tungften.

ᶠ The fpecific gravity of Tungften has not been afcertained with precifion, on account of the extreme difficulty of fufing this metallic fubftance. Guyton, in a fire urged by the blaft of three pipes to 185 degrees of the pyrometer, obtained a well rounded piece of 35 grammes. But it broke in the vice, and exhibited a central portion, which was only agglutinated, and foon acquired a purple colour by expofure to the air. He found that the

well

well fufed portion had no higher fpecific gravity than 8.3406, and he concludes from the infufibility and brittlenefs of this metal, that it affords little promife of utility in the arts, except in metallic alloys, or by virtue of the property which its oxide poffeffes, of affording fixed colours, or giving fixity to the colours of vegetables.

The *Geometrie defcriptive* of Gafpard Monge is publifhed at Paris in quarto, with 25 plates, by Bernard, and likewife a new edition of his Statique.

The reference to a drawing in Mr. Cruickfhank's paper, in our prefent Number, relates to a fketch which, on account of the late reception of the copy, could not be inferted in the plate. It confifts fimply of two tubes, forming what I have called the broken circuit in the manner mentioned in my experiment, where a tube with water was connected with another containing marine acid, and both fet to work at the fame time. It feems very probable, that a large quantity of gas might be thus extricated; but from the circum-ftances of the laft mentioned experiment, it feems apparent that the power of the ap-paratus in each individual tube will be lefs the greater the number.
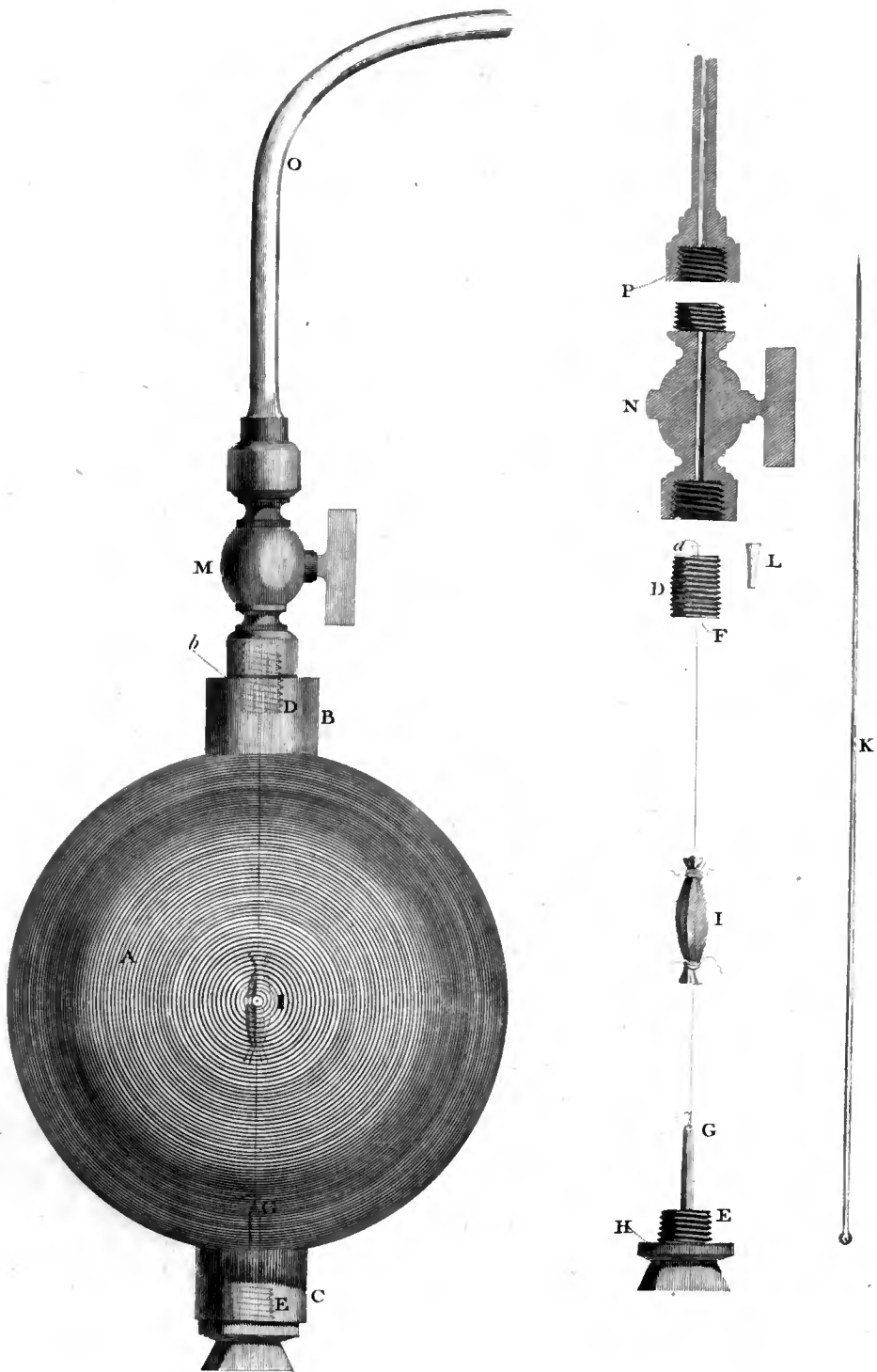
Fig. 2.
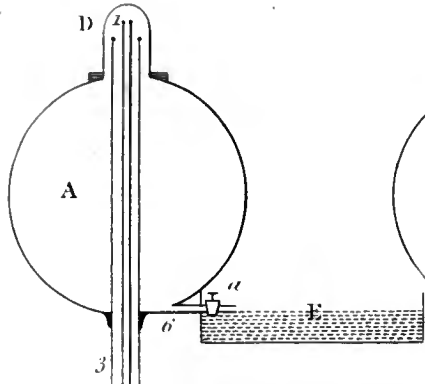
Fig. 3.
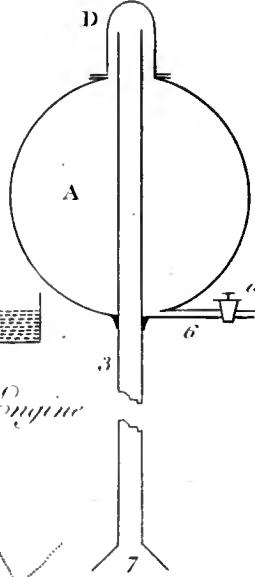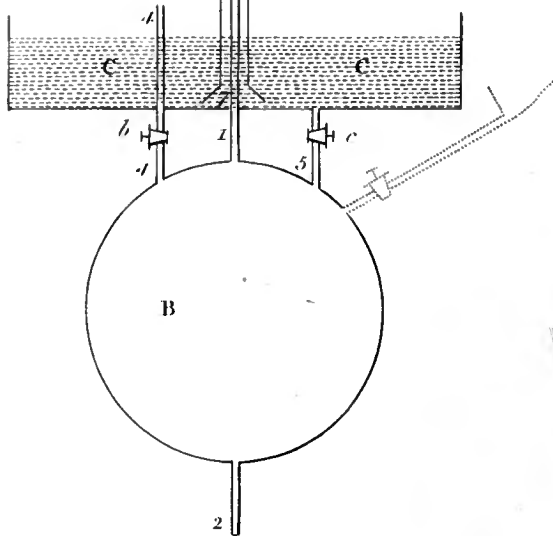
New furnace for Copper

Fig. 1.
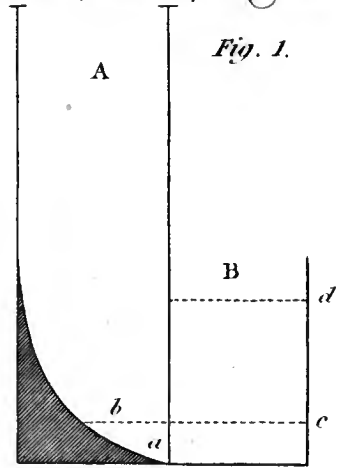
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Hydraulic Engine

Parhelia &c.

Fig. 4.

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NATURAL PHILOSOPHY, CHEMISTRY,

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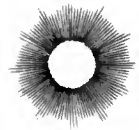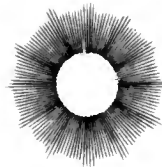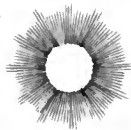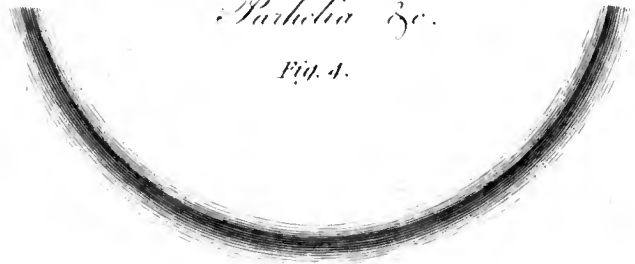
THE ARTS.

ARTICLE I.

Experiments on the Production of Air by the Freezing of Water. By JOSEPH PRIESTLEY, *LL.D. F.R.S. &c. &c. Communicated by the Author.*

IN 1793, when I was in England, I published an account of a course of experiments on *the generation of air from water*, and, after my arrival in this country, I published a sequel to them. The result of the whole was that, after all air had, in every known method, been extracted from any quantity of water, whenever any portion of it was converted into vapour, a bubble of permanent air was formed, and this was always phlogisticated. The process with the torricellian vacuum I continued some years, and found the production of air equable to the last.

The necessary inference from these experiments is either that water is convertible into phlogisticated air, or that it contains more of this air intimately combined with it than can, by this process, be extracted in any reasonable time.

Finding that no air is contained in ice that is free from visible bubbles, I thought to ascertain the truth of one of those hypotheses by exposing to cold a quantity of water from which I had, by repeated processes with the torricellian vacuum, expelled all the air that I possibly could; thinking that if it really contained no air, it would appear by the

ice being perfectly folid, fo that when it was melted, no air could be got from it. This experiment I repeated feveral times, but always found that, though the outfide of this ice was perfectly tranfparent, and free from air bubbles, the central parts were opaque; and though there were no diftinct air bubbles there, yet when it was melted, a great number iffued from it. The whole quantity of air, however, was not more than might have been produced from the fame water in the other proceffes in a reafonable time; and in them the production of air had no limit.

Difappointed in my expectation of getting by this means ice perfectly free from air (which, when a large quantity of water freezes very flowly it is eafy to do, the air contained in it retiring from that which is frozen to that which remains fluid) I diffolved ice that was perfectly tranfparent, and therefore free from air, in veffels containing mercury, and expofed it to froft a fecond time. But I always found that when the whole of it was frozen, the extreme parts were tranfparent, the central parts were opaque, and when diffolved, yielded air. Though I repeated this procefs ten or a dozen times with the fame water, always letting out the air that was procured by freezing, prefently after it was extricated, and before it could have been re-abforbed; yet on expofing it to another freezing, I never failed to get more air, and the harder the froft was, the more air I procured.

As there is an evaporation from ice no lefs than from water, the interftices made by the cryftalization of the water, when it is converted into ice, will foon be filled with vapour; and this vapour, like that which is formed by heat, becomes the bafis, I fuppofe, of a quantity of air. Since, however, the moft tranfparent ice fwims in water, this alfo muft have interftices, but they contain no air; being fuch as exift in the moft folid bodies in which (gold itfelf not excepted) the component particles are not in perfect contact, fince they are reduced into lefs dimenfions by cold.

As the veffels I made ufe of in thefe experiments were either cylindrical jars, or conical wine glaffes, and, confequently, the bubbles of air procured by freezing were expofed to a confiderable furface of water, and would in time (though not, I found, in the courfe of a day) have been abforbed by the water more free from air, I procured glafs veffels of a conical form, terminating in narrow tubes, into which the air diflodged from the ice might afcend, and not be fubject to be abforbed. I was fo fortunate as to have feveral veffels of this form, and they completely anfwered my purpofe for five or fix proceffes.

Thefe veffels were firft filled with mercury, and then I introduced into them a quantity of water freed from air by previous freezing; and when, after expofure to froft the ice was melted, the air diflodged from the ice afcended into the narrow tubes, and remained without any fenfible diminution of bulk feveral days; and every time that the water was expofed to the froft, an addition was made to it. At length, however, though the veffels were very ftrong, and contained much mercury, which, by its tendency to defcend, would give the water room to expand with the lefs danger of breaking the veffel, none of them ferved for more than the number of proceffes above-mentioned.

<div align="right">After</div>

After the breaking of my glafs veffels, I got other cylindrical ones made of *iron*, feven or eight inches in height, and near three inches wide at the bottom, the upper orifice being clofed with a cork and cement, in the centre of which was a glafs tube, the diameter of which was about the fifth of an inch. And as this glafs tube was in the greateft danger of breaking by the freezing of the water, and this had happened feveral times in the former experiments, notwithftanding all my care to guard them from the froft, I now made ufe of fnow and falt to freeze the water in the iron veffel only, placed in a veffel of mercury, having been previoufly filled as the glafs veffels had been.

The water on which I now operated was about three ounces, and it had been made as free as poffible from air by previous freezing. With this apparatus I repeated the procefs of freezing nine times, without changing the water; and the laft portion of air that I procured in this manner was as great as any of the preceding; fo that there remained no reafonable doubt, but that air might be produced from the fame water, in this manner, *ad libitum*; and having got near two inches of air in the glafs tube, I put an end to the experiment; and examining the air, I found it to be wholly *phlogifticated*, not being affected by nitrous air, and having nothing inflammable in it.

During the procefs of freezing, the air in the tube was generally compreffed into one-fifth of its ufual bulk; but when I began to thaw the ice, which I did by means of hot water in the place of the freezing mixture, it foon expanded to its former dimenfions, and no fenfible portion of it was abforbed during the whole procefs, which was about a week. Sometimes the violence of the preffure, occafioned by the expanfion of the water in freezing, would force a little water out of the veffel between the cork and the glafs tube, or the iron veffel, which prefently became ice. This I always carefully removed, and applied frefh cement to the place, to prevent the introduction of any air from without, before I began to melt the ice. And that no external air had entered, was evident both from the manner in which the air was produced after the water recovered its fluidity, and from the quality of it when examined after the procefs.

In the courfe of the experiments with the glafs veffels, a phenomenon occurred which was wholly unexpected by me, and which was very amufing. Having left the veffels filled in part with water, and in part with mercury, in the evening, I generally found them in the morning feemingly quite full of mercury, every part of the ice within the veffel being covered with it. This muft have been occafioned by a vacuum having been formed between the glafs and the ice, and into this fpace mercury had been drawn up on the principle of the capillary tube. When this was not the cafe, the interftices of the ice towards the centre were filled with thin laminæ of mercury, which alfo exhibited a curious appearance.

Sometimes, when there was no mercury between the glafs and the ice, an interftice was made between them when they were placed within the influence of the fire. In thefe circumftances I have feen the mercury drawn up to the height of feveral inches. As this fpace became enlarged by the increafe of the heat, the laminæ of mercury were con-

trafted,

tracted, till coming into the form of balls, too heavy to be supported, they fell down to the mass of mercury in the bason.

The most obvious inference from these experiments is, that water, when reduced by any means to the state of vapour, is in part converted into phlogisticated air, and that this is one of the means provided by nature to keep up the equilibrium of this constituent part of the atmosphere; as the influence of light on growing vegetables is the means of recruiting the other parts of it, viz. dephlogisticated air, and both of them are subject to absorption and diminution in several natural processes. Inflammable air, I have also shewn to be convertible into phlogisticated air; so that this is another method of supplying the atmosphere with this ingredient in its composition.

That water contains phlogiston, I have shewn to be probable from several considerations, especially that of its resembling metals in their property of being conductors of electricity. For these substances certainly contain phlogiston, if there be any such thing. Mercury also becomes super-phlogisticated by agitation in water, and this without limit, and without changing the water or the mercury; and the remaining water contains no more oxygen than before. For the air expelled from it is not more pure, but considerably less so, and it is perfectly free from acidity.

I would further observe, that these experiments, which prove the conversion of water into phlogisticated air, are inconsistent with the antiphlogistic theory, which makes water resolvable into dephlogisticated and inflammable air; but that they are highly favourable to the hypothesis of water being the basis of every kind of air, the difference between them depending upon the addition of some principles which we are not able to ascertain by weight. Also, if any species of air be entitled to the appellation of *hydrogen*, it is not inflammable, but phlogisticated air.

II.

On the Absorbent Powers of different Earths. By Mr. JOHN LESLIE. Communicated by the Author.

IN completing my researches in hygrometry, I have been led to examine the habitudes of the earthy bodies with respect to moisture. I had already discovered, that animal and vegetable matters, like the saline and deliquescent substances, attract humidity by a force altogether distinct from that of capillary absorption, and attended with diminution of volume, evolution of heat, and the other concomitants which mark a real change of constitution. And while the empire of chemical agency seemed extending on every side, it was reasonable to doubt if the earths themselves continued entirely inert and passive under the atmospherical influences. My suspicions were fully confirmed by experiment. *All the earths and stony bodies eminently attract moisture from the air, and this with different degrees*

degrees of force, modified also by temperature, which affects the measure of all combinations. The facts thus obtained, though not of a brilliant nature, appear highly instructive, and if prosecuted with attention, might throw some light on the obscure theory of vegetation, and suggest useful improvements in agriculture and rural œconomy. I give this early notice of the little which I have done, as it seems to diverge from that line of inquiry which I had prescribed to myself, and as I shall not at least for the present, pursue it farther. But I earnestly invite chemists and naturalists to resume a subject which promises such an easy and abundant harvest.

My procedure was this :—The earths, or the stony matters grosly pounded, were dried thoroughly before a good fire, and immediately introduced into phials, which were stopped up and set aside to cool. The first object was to ascertain, whether the degree of heat to which they were subjected in drying, would affect their absorbent quality. I soon perceived, however, that the results were quite regular and uniform. In fact, the powder is dried in this case not by the action of the hot air on its surface, but by the heat penetrating the mass, and communicating to the aqueous particles the disposition to assume the vaporous state, with the corresponding elasticity of steam, which beyond the boiling point increases with most rapid progression, and which enormous distensive power soon overcomes the obstinate adhesion to the earthy basis. Any heat almost between the 100th and 300th degree of the centigrade scale, will bring the substances into the proper condition. This desiccated powder is strewed on the bottom of a saucer holding an hygrometer, over which a small receiver is inverted. In a few minutes the instrument marks the highest degree of dryness produced. Here are a few of the principal results indicated by this mode, at the temperature of 16° centigrade.

Free stone	37°
Fine sea sand	40
Marble	47
Common clay	68
Sea sand, lately cultivated	72
Sandy schist	75
Bog earth	77
Rotten whinstone	78
Garden mould	80

It is remarkable that marble and quicklime produce exactly the same effect, and that in general no sensible difference is perceived between the pure earths and their carbonates. The great absorbent power of the argillaceous, compared with the silicious bodies, also deserves particular notice. But the cultivated soils possess that power in the most conspicuous degree. Garden mould stands at the top of the scale, and even sea sand, which causes a dryness of 40° only is, by a few years' tillage, rendered capable of producing that of 72°. Nor is it the operation of manure, for the single effect of this material is inferior to that of most of the earths. It seems highly probable, that the fertility of soils depends chiefly on

their

their difposition to imbibe moiſture. Manures perhaps act as ſtimulants merely, the car-
bonaceous matter of plants is derived from the atmoſphere, and the earth affords expanſion
for the fibrils of the roots, and ſupplies them with the aqueous element. Rotten whin-
ſtone conſtitutes the body of the ſoil in this fertile county, and it manifeſts among the
ſtony compounds a ſuperior degree of the power of abſorption.

Torrefaction ſeems remarkably to diminiſh the faculty of the earthy ſubſtances to attract
moiſture. Clay, roaſted in a ſtrong fire, from 68 gives only 35 degrees by the hygrome-
ter, and after being urged in a blackſmith's forge, affords no more than 8 ; whinſtone,
which in ordinary caſes has a power equal to 78°, ſhews only 23°, after expoſure to the
fierce heat of a forge. Nor is the effect occaſioned by any partial or incipient vitrification,
for ſandſtone, which has undergone a violent ignition, ſhews likewiſe a ſimilar change of
property. If bodies therefore ſuffered no alteration of contexture from the impreſſions of
the atmoſphere, geologiſts would be furniſhed with one certain criterion, to decide whe-
ther a foſſil owed its formation to the agency of fire or water. It is almoſt ſuperfluous to
obſerve, how vague and inconcluſive are the arguments uſually employed by the contending
ſects of Neptuniſts and Plutoniſts.

But although the mode of experimenting above deſcribed ſufficiently marks their diſcri-
minating qualities, it does not exhibit the full effect of the deſiccated powders ; ſince, the
hygrometer and the earth being included together, while the latter abſtracts moiſture from
the air within the receiver, the former by its wetted ball continually imparts humidity,
which notwithſtanding the extenſive ſurface of abſorption, muſt always enfeeble the per-
formance in proportion to the time of action. The moſt accurate way of experimenting,
is to throw the powders, either from the phials, or directly on its being withdrawn from
the fire, into a large flattened glaſs veſſel with a narrow neck, which is corked up for ſeveral
hours, and the condition of the included air may then be examined by letting down a ſmall
hygrometer ſuſpended by a thread from the ſtopper. For want of more ſuitable apparatus,
I uſed wine decanters, and having provided myſelf with ſpecimens of the primitive earths,
I began with determining their relative abſorbent powers. The reſults at the ſame tem-
perature, of 16° centigrade, were as follow :

Carbonate of ſtrontian	.	.	.	.	23°
Carbonate of barytes	.	.	.	.	32
Quartz	.	.	.	.	40
Marble	.	.	.	.	70
Carbonate of magneſia	.	.	.	.	75
Alumine	.	.	.	.	84

This furniſhes another proof that ſtrontianite is an independant earth, ſince it ſtands
apart from the reſt. Quartz in ſmall rounded fragments, like pigeon's eggs, picked up on
the beach, gave almoſt the ſame reſult as when reduced to a powder. In general, the only
uſe of pounding the ſtones is to accelerate their performance. It might be preſumed, that
the mixtures of thoſe earths would produce intermediate effects ; yet equal parts of ſilex

and

and alumine gave as much as the latter singly. But the *quantity* of absorption must be distinguished from its *intensity.* Thus strontianite, barytes, and silex, are quickly saturated with moisture, while magnesia and alumine continue to imbibe it for a considerable time. The effects in mixture depend on those qualities combined : after the silex has ceased to act, the alumine perseveres, and through the intermediate agency of the air, divests the former of the minute portion of humidity which it had attracted. But the compounds of the simple earths are still more remarkable in their affection to moisture, since it surpasses that of their ingredients. Thus, sea sand gives 70°; whinstone, 80°; though it consists one half of silex, and the other half of alumine, and the oxide of iron * in nearly equal parts; and pipe clay, which contains a large proportion of silex, 85°. It is evident, that the absorbent power of earths depends as much on their mechanical condition, as on the species of matter of whiah they are composed. Whatever tends to harden them diminishes their effect, and the contrary. Hence the reason why the action of fire impairs their desiccating quality. Quartz, urged in a blacksmith's forge, gives only 19°; but the same powder, after being soaked a week in water, gave 35°; and most probably it would have in time recovered its whole original power. The process by which nature gradually divides, softens, and disposes stony bodies to absorb moisture, is beautifully illustrated in the instance of whinstone or basalt. A piece of solid whinstone gave 80 degrees by the hygrometer, another piece rotten and crumbling, gave 86°; but another portion of the same rock already reduced to mould, afforded 92 †. The ameliorating effects of culture is exemplified in sea sand : fine sand caused a dryness of 70°; sand collected from the paths of a sheep walk near the beach 78°; the same sand, lately brought into cultivation 85°. Still these effects are inferior to that of garden mould, which amounts to 95°, and to which decomposed whinstone approaches the nearest. Comparing those facts with the property lately discovered by Humboldt, that the earths attract the oxygenous portion of the air, we farther perceive the intimate concatenation of changes which maintain this mundane system. The lap of nature is prepared for the reception and developement of the vegetable germs, exhibiting in a reciprocating series of growth and decay, the ever renovating forms of organic bodies through endless successions of cycles.

Sulphuric acid nearly doubles its desiccating power at every 15 degrees of elevation of temperature, that at zero being 60°. The several earths appear to follow progressions not much different.

The relative effects of different soils may be ascertained more easily, by altering the construction of the hygrometer. Connect the two balls by a long bent tube passed through two corks, and cover and wet them both. In this state they are introduced into the wine

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* The oxide of iron and zinc which I have tried, shewed less absorbent powers than the earthy bodies.

† Those specimens were taken from a remarkable spot on the summit of Largo Lau, an elegant conical mountain, which rises behind this place to the height of about 800 feet above the level of the sea. The whinstone parts into small hexagonal columns, of which one of the sides is sometimes almost effaced.

decanters.

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decanters containing the two earths to be examined; the rife or depreffion of the liquor will indicate which has the greateft abforbent power, and the meafure of its excefs. I conftructed fuch an inftrument, but have not yet pufhed the inquiry to any extent.

Before I conclude, I muft notify that the deficcating quality of flannel may be employed for the graduation of the hygrometer, inftead of that of the fulphuric acid. A large piece of flannel dried well at a fire, laid between the leaves of a book to cool, and then placed in folds within a large receiver, will produce a drynefs of 80° at the temperature of 16°. Thofe, however, who wifh to purchafe the hygrometer or photometer, may apply to Mr. Cary, optician, in the Strand.

<div align="right">JOHN LESLIE.</div>

Largo, Fifefhire, June 9, 1800.

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III.

On a New Fulminating Mercury. By EDWARD HOWARD, *Efq. F. R. S.*

(*Continued from page* 178.)

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HAD the alchemift propofed to fix water by the fame apparatus, the neft of boxes muft, I fuppofe, have likewife been ruptured; yet it does not follow that the explofion would have been fo tremendous: indeed it is probable that it would not, for if (as Mr. Kirwan remarked to me) fubftances which have the greateft fpecific gravity, have likewife the greateft attraction of cohefion, the fuppofition that the vapour of mercury exceeds in expanfive force the vapour of water, would agree with a pofition of Sir Ifaac Newton, *that thofe particles recede from one another with the greateft force, and are moft difficultly brought together, which upon contact cohere moft ftrongly* *.

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SECTION IX.

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Before I attempt to inveftigate the conftituent principles of this powder, it will be proper to defcribe the procefs and manipulations which, from frequent trials, feem to me beft calculated to produce it.

100 grains, or a greater proportional quantity, of quickfilver (not exceeding 500 grains †) are to be diffolved, with heat, in a meafured ounce and a half of nitric acid ‡. This folution being poured cold *upon* two meafured ounces of alcohol §, *previoufly* introduced into

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* Newton's Optics, p. 372, 4th Ed. Lond. 1730.

† The reafon of this limitation is not on account of any danger attending the procefs; but becaufe the quantities of nitric acid and alcohol required for more than 500 grains, would excite a degree of heat detrimental to the preparation.

‡ Of the fpecific gravity of about 1,3.

§ Of the fpecific gravity of about ,849.

<div align="right">any</div>

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any convenient glafs veffel, a moderate heat is to be applied until an effervefcence is excited. A white fume then begins to undulate on the furface of the liquor; and the powder will be gradually precipitated, upon the ceffation of action and re-action. The precipitate is to be immediately collected on a filter, well wafhed with diftilled water, and carefully dried in a heat not much exceeding that of a water bath. The immediate edulcoration of the powder is material, becaufe it is liable to the re-action of the nitric acid; and, whilft any of that acid adheres to it, it is very fubject to the influence of light. Let it alfo be cautioufly re-membered, that the mercurial folution is to be poured *upon* the alcohol.

I have recommended quickfilver to be ufed in preference to an oxide, becaufe it feems to anfwer equally, and is lefs expenfive; otherwife, not only the pure red oxide, but the red nitrous oxide, and turpeth, may be fubftituted; neither does it feem effential to attend to the precife fpecific gravity of the acid, or the alcohol. The rectified fpirit of wine and the nitrous acid of commerce, never failed, with me, to produce a fulminating mercury. It is indeed true, that the powder prepared without attention, is produced in different quantities, varies in colour, and probably in ftrength. From analogy, I am difpofed to think the whiteft is the ftrongeft; for it is well known, that black precipitates of mercury approach the neareft to the metallic ftate. The variation in quantity is remarkable; the fmalleft quantity I ever obtained from 100 grains of quickfilver being 120 grains, and the largeft 132 grains. Much depends on very minute circumftances. The greateft product feems to be obtained, when a veffel is ufed which condenfes and caufes moft ether to return into the mother liquor; befides which, care is to be had in applying the requifite heat, that a fpeedy, and not a violent action be effected. 100 grains of an oxide are not fo productive as 100 grains of quickfilver.

As to the colour, it feems to incline to black, when the action of the acid on the alcohol is moft violent, and *vice verfâ*.

SECTION X.

I need not obferve, that the gafes which were generated during the combuftion of the powder in the glafs globe, were neceffarily mixed with atmofpheric air; the facility with which the electric fluid paffes through a vacuum, made fuch a mixture unavoidable.

The cubical inch of gas received over water was not readily abforbed by it: and, as it foon extinguifhed a taper, without becoming red, or being itfelf inflamed, barytes water was let up to the three cubical inches received over mercury, when a carbonate of barytes was immediately precipitated.

The refidue of feveral explofions, after the carbonic acid had been feparated, was found, by the teft of nitrous gas, to contain nitrogen or azotic gas; which does not proceed from any decompofition of atmofpheric air, becaufe the powder may be made to explode under the exhaufted receiver of an air-pump. It is therefore manifeft, that the gafes generated during the combuftion of the fulminating mercury, confift of carbonic acid and nitrogen gafes.

SECTION XI.

The principal re-agents which decompofe the mercurial powder, are the nitric, the ful-phuric, and the muriatic acids. The nitric changes the whole into nitrous gas, carbonic acid gas, acetous acid, and nitrate of mercury. I refolved it into thefe different principles, by diftilling it pneumatically with nitric acid : this acid, upon the application of heat, foon diffolved the powder, and extricated a quantity of gas, which was found, by well known tefts, to be nitrous gas mixed with carbonic acid gas. The diftillation was carried on until gas no longer came over. The liquor of the retort was then mixed with the liquor col-lected in the receiver, and the whole faturated with pot-afh; which precipitated the mer-cury in a yellowifh-brown powder, nearly as it would have done from a folution of nitrate of mercury. This precipitate was feparated by a filter, and the filtrated liquor evaporated to a dry falt, which was wafhed with alcohol. A portion of the falt being refufed by this menftruum, it was feparated by filtration, and recognized, by all its properties, to be nitrate of pot-afh. The alcoholic liquor was likewife evaporated to a dry falt, which, upon the affufion of a little concentrate fulphuric acid, emitted acetous acid, contaminated with a feeble fmell of nitrous acid, owing to the folubility of a fmall portion of the nitre in the alcohol.

SECTION XII.

The fulphuric acid acts upon the powder in a remarkable manner, as already has been noticed. A very concentrate acid produces an explofion nearly at the inftant of contact, on account, I prefume, of the fudden and copious difengagement of caloric from a portion of the powder which is decompofed by the acid. An acid fomewhat lefs concentrate like-wife extricates a confiderable quantity of caloric, with a good deal of gas ; but, as it effects a complete decompofition, it caufes no explofion. An acid diluted with an equal quantity of water, by the aid of a little heat, feparates the gas fo much lefs rapidly, that it may with fafety be collected in a pneumatic apparatus. But, whatever be the denfity of the acid, (provided no explofion be produced,) there remains in the fulphuric liquor, after the feparation of the gas, a white uninflammable and uncryftalized powder, mixed with fome minute globules of quickfilver.

To eftimate the quantity, and obferve the nature, of this uninflammable fubftance, I treated 100 grains of the fulminating mercury with fulphuric acid a little diluted. The gas being feparated, I decanted off the liquor as it became clear, and freed the infoluble powder from acid, by edulcoration with diftilled water ; after which, I dried it, and found it weighed only 84 grains ; confequently had loft 16 grains of its original weight. Suf-pecting, from the operation of the nitric acid in the former experiment, that thefe 84 grains (with the exception of the quickfilver globules) were oxalate of mercury, I digefted them in nitrate of lime, and found my fufpicion juft. The mercury of the oxalate united to the nitric acid, and the oxalic acid to the lime. A new infoluble compound was formed ;

it weighed, when washed and dry, 48,5 grains. Carbonate of pot-ash separated the lime, and formed oxalate of pot-ash, capable of precipitating lime-water, and muriate of lime; although it had been depurated from excess of alkali, and from carbonic acid, by a previous addition of acetous acid. That the mercury of the oxalate in the 84 grains, had united to the nitric acid of the nitrate of lime, was proved by dropping muriatic acid into the liquor from which the substance demonstrated to be oxalate of lime had been separated; for a copious precipitation of calomel instantly ensued.

The sulphuric liquor, decanted from the oxalate of mercury, was now added to that with which it was edulcorated, and the whole saturated with carbonate of pot-ash. As effervescence ceased, a cloudiness and precipitation followed; and the precipitate, being collected, washed, and dried, weighed 3,4 grains: it appeared to be a carbonate of mercury. Upon evaporating a portion of the saturated sulphuric liquor, I found nothing but sulphate of pot-ash; nor had it any metallic taste. There then remains, without allowing for the weight of the carbonic acid united to the 3,4 grains, a deficit from the 100 grains of mercurial powder, of 12,6 grains, which I ascribe to the gas separated by the action of the sulphuric acid. To ascertain the quantity, and examine the nature, of the gas so separated, I introduced into a very small tubulated retort, 50 grains of the mercurial powder, and poured upon it 3 drams, by measure, of sulphuric acid, diluted with an equal quantity of water, and extricated the gas with the assistance of a gentle heat. I first received it over quicksilver, the surface of which, during the operation, partially covered itself with a little black powder *.

The gas, by different trials, amounted from 28 to 31 cubical inches; it at first appeared to be nothing but carbonic acid, as it precipitates barytes water, and extinguished a taper, without being itself inflamed, or becoming red. But, upon letting up to it liquid caustic ammoniac, there was a residue of from 5 to 7 inches of a peculiar inflammable gas, which burnt with a greenish blue flame. When I made use of the water-tub, I obtained, from the same materials, from 25 to 27 inches only of gas, although the average quantity of the peculiar inflammable gas was likewise from 5 to 7 inches; therefore, the difference of the aggregate product, over the two fluids, must have arisen from the absorption, by the water, of a part of the carbonic acid in its nascent state. The variation of the quantity of the inflammable gas, when powder from the same parcel is used, seems to depend upon the acid being a little more or less dilute.

With respect to the nature of the peculiar inflammable gas, it is plain to me, from the reasons I shall immediately adduce, that is is no other than the gas (in a pure state) into which the nitrous etherized gas can be resolved, by treatment with dilute sulphuric acid.

The Dutch chemists have shewn †, that the nitrous etherized gas can be resolved into nitrous gas, by exposure to concentrate sulphuric acid, and that, by using a dilute instead

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* I cannot account for this appearance.

† *Journal de Physique*, p. 250, October, 1794.

of a concentrate acid, a gas is obtained which enlarges the flame of a burning taper, fo much like the gafeous oxide of azote, that they miftook it for that fubftance, until they difcovered that it was permanent over water, refufed to detonate with hydrogen, and that the fallacious appearance was owing to a mixture of nitrous gas with an inflammable gas.

The inflammable gas feparated from the powder, anfwers to the defcription of the gas which at firft deceived the Dutch chemifts; 1ft, in being permanent over water; 2dly, refufing to detonate with hydrogen; and, 3dly, having the appearance of the gafeous oxide of azote, when mixed with nitrous gas.

The gas feparable by the fame acid, from nitrous etherized gas, and from the mercurial powder, have therefore the fame properties. Every chemift would thence conclude, that the nitrous etherized gas is a conftituent part of the powder, had the inflammable and nitrous gas, inftead of the inflammable and carbonic acid gas, been the mixed product extricated from it by dilute fulphuric acid.

It however appears to me, that nitrous gas was really produced by the action of the dilute fulphuric acid; and that, when produced, it united to an excefs of oxygen prefent in the oxalate of mercury.

To explain how this change might happen, I muft premife, that my experiments have fhewn me, that oxalate of mercury can exift in two, if not in three ftates.

1ft, By the difcovery of Mr. Ameilon already quoted, the precipitate obtained by oxalic acid, from nitrate of mercury, fufes with a hiffing noife. This precipitate is an oxalate of mercury, feemingly with excefs of oxygen. Mercury diffolved in fulphuric acid and precipitated by oxalic acid, and alfo the pure red oxide of mercury digefted with oxalic acid, give oxalates in the fame ftate.

2dly, Acetate of mercury precipitated by oxalic acid, although a true oxalate is formed, has no kind of inflammability. I confider it as an oxalate with lefs oxygen than thofe above-mentioned.

3dly, A folution of nitrate of mercury boiled with dulcified fpirit of nitre, gives an oxalate more inflammable than any other: perhaps it contains moft oxygen.

The oxalate of mercury remaining from the powder in the fulphuric liquor, is not only always in the fame ftate as that precipitated from acetate of mercury, entirely devoid of inflammability, but contains globules of quickfilver; confequently, it muft have parted with even more than its excefs of oxygen; and, if nitrous gas was prefent, it would of courfe feize at leaft a portion of that oxygen. It is true, that globules of quickfilver may feem incompatible with nitrous acid; but the quantity of the one may not correfpond with that of the other, or the dilution of the acid may deftroy its action.

As to the prefence of the carbonic acid, it muft have arifen either from a complete * decompofition of a part of the oxalate; or, admitting the nitrous etherized gas to be a conftituent principle of the powder, from a portion of the oxygen, not taken up by the nitrous gas, being united with the carbon of the etherized gas.

* Inflammable oxalate of mercury, made to fufe in a retort connected with the quickfilver tub, gives out carbonic acid gas.

SECTION

SECTION XIII.

The muriatic acid digested with the mercurial powder, diffolves a portion of it, without extricating any notable quantity of gas. The diffolution evaporated to a dry falt, taftes like corrofive fublimate; and the portion which the acid does not take up, is left in the ftate of an uninflammable oxalate.

SECTION XIV.

Thefe effects all tend to eftablifh the exiftence of the nitrous etherized gas, as a confti-tuent part of the powder; and likewife corroborate the explanation I have ventured to give, of the action of the fulphuric acid. Moreover, a meafured ounce and a half of ni-trous acid, holding 100 grains of mercury in folution, and 2 meafured ounces of alcohol, yield 90 cubical inches only of gas : whereas, without the intervention of mercury, they yield 210 inches. Upon the whole, I truft it will be thought reafonable to conclude, that the mercurial powder is compofed of the nitrous etherized gas, and of oxalate of mercury with excefs of oxygen.

1ft, Becaufe the nitric acid converts the mercurial powder intirely into nitrous gas, carbonic acid gas, acetous acid, and nitrate of mercury.

2dly. Becaufe the dilute fulphuric acid refolves it into an uninflammable oxalate of mer-cury, and feparates from it a gas refembling that into which the fame acid refolves the nitrous etherized gas.

3dly. Becaufe an uninflammable oxalate is likewife left, after the muriatic acid has converted a part of it into fublimate.

4thly. Becaufe it cannot be formed by boiling nitrate of mercury in dulcified fpirit of nitre; although a very inflammable oxalate is by this means produced.

5thly. Becaufe the difference of the product of gas, from the fame meafures of alcohol and nitrous acid, with and without mercury in folution, is not trifling ; and,

6thly. Becaufe nitrogen gas was generated during its combuftion in the glafs globe.

Should my conclufions be thought warranted by the reafons I have adduced, the theory of the combuftion of the mercurial powder will be obvious to every chemift. The hy-drogen of the oxalic acid, and of the etherized gas, is firft united to the oxygen of the oxalate, forming water *; the carbon is faturated with oxygen, forming carbonic acid gas ; and a part, if not the whole of the nitrogen of the etherized gas, is feparated in the ftate of nitrogen gas; both of which laft gafes, it may be recollected, were after the explofion prefent in the glafs globe. The mercury is revived, and, I prefume, thrown into vapour ; as may well be imagined, from the immenfe quantity of caloric extricated, by adding con-centrate fulphuric acid to the mercurial powder.

* Drops of water were obferved on the internal furface of the globe, the day after feveral explofions had been produced in its centre.

I will

I will not venture to ſtate with accuracy, in what proportions its conſtituent principles are combined. The affinities I have brought into play are complicated, and the conſtitution of the ſubſtances I have to deal with not fully known. But, to make round numbers, I will reſume the ſtatement, that 100 grains of the mercurial powder loſt 16 grains of its original weight, by treatment with dilute ſulphuric acid: 84 grains of mercurial oxalate, mixed with a few minute globules of quickſilver, remained undiſſolved in the acid. The ſulphuric liquor was ſaturated with carbonate of pot-aſh, and yielded 3,4 grains of carbonate of mercury. If 1,4 grain ſhould be thought a proper allowance for the weight of carbonic acid in the 3,4 grains, I will make that deduction, and add the remaining 2 grains to the 84 grains of mercurial oxalate and quickſilver; I ſhall then have,

Of oxalate and mercury　　　-　　　-　　　-　　　-　　　86 grains
and a deficit, to be aſcribed to the nitrous etherized gas and
exceſs of oxygen　　　-　　　-　　　-　　　-　　　-　　　14
　　　　　　　　　　　　　　　　　　　　　　　　　　　　100

It may perhaps be proper to proceed ſtill further, and recur to the 48,5 grains, ſeparated by nitrate of lime from the 84 grains of mercurial oxalate and globules of quickſilver, in the 11th ſection. Theſe 48,5 grains were proved to be chiefly oxalate of lime; but they likewiſe contained a minute inſeparable quantity of mercury, almoſt in the ſtate of quickſilver, formerly part of the 84 grains from which they were ſeparated. Had the 48,5 grains been pure calcareous oxolate, the quantity of pure oxalic acid in them would, according to Bergmann[*], be 23,28 grains. Hence, by omitting the 2 grains of mercury in the 3,4 grains of carbonate, 100 grains of the mercurial powder might have been ſaid to contain, of pure oxalic acid 23,28 grains; of mercury 62,72 grains; and of nitrous etherized gas and exceſs of oxygen 14 grains. But, as the 48,5 grains were not pure oxalate, inaſmuch as they contained the mercury they received from the 84 grains, from which they were generated by the nitrate of lime, ſome allowance muſt be made for the mercury ſucceſſively intermixed with the 84 grains and the 48,5 grains.

In order to make correſponding numbers, and allow for unavoidable errors, I ſhall eſtimate the quantity of that mercury to have amounted to 2 grains, which I muſt of courſe deduct from the 23,28 grains of oxalic acid. I ſhall then have the following ſtatement:

That 100 grains of the fulminating mercury ought to contain,
of pure oxalic acid　　-　　-　　-　　-　　-　　-　　21,28 grains,
of mercury formerly united to the oxalic acid　　-　　-　　60,72
of mercury diſſolved in the ſulphuric liquor　　-　　-　　2
and of mercury left in the ſulphuric liquor after the ſeparation of
　the gaſes　　-　　-　　-　　-　　-　　2
　　　　　　　　Total of mercury　　-　　-　　64,72
Of nitrous etherized gas and exceſs of oxygen　-　　-　　14,
　　　　　　　　　　　　　　　　　　　　　　　　　　100.

* Bergmann, *de Acido Saccbari*, Opuſcula. tom. i. § 6. p. 248. Leipzig, 1788.

Since

Since 100 grains of the powder feem to contain 64,72 grains of mercury, it will be immediately inquired, what becomes of 100 grains of quickfilver, when treated as directed, in the defcription of the procefs for preparing the fulminating mercury.

It has been ftated (in fection 9) that 100 grains of quickfilver produce, under different circumftances, from 120 to 132 grains of mercurial powder; and, if 100 grains of this powder contain 64,72 grains, 120 grains, or 132 grains muft, by parity of reafoning, contain 78,06 grains, or 85,47 grains; therefore, 13,34 grains, or 20,75 grains, more of the 100 grains are immediately accounted for; becaufe 64,72 grains + 1334 grains = 78,06, and 64,72 grains + 20,75 grains = 85,47 grains. The remaining deficiency of 21,94 grains, or 14,53 grains, which, with the 78,06 grains, or 85,47 grains, would complete the original 100, of quickfilver, remains partly in the liquor from which the powder is feparated, and is partly volatilized in the white denfe fumes, which in the beginning of this paper I compared to the *liquor fumans* of Libavius. The mercury cannot, in either inftance, be obtained in a form immediately indicative of its quantity; and a feries of experiments to afcertain the quantities in which many different fubftances can combine with mercury, is not my prefent object. After obferving, that the mercury left in the refiduary liquor can be precipitated in a very fubtle dark powder, by carbonate of pot-afh, I fhall content myfelf with examining the nature of the white fumes.

SECTION XV.

It is clear that thefe white fumes contain mercury: they may be wholly condenfed in a range of Woulfe's apparatus, charged with a folution of muriate of ammoniac. When the operation is over, a white powder is feen floating with ether on the faline liquor, which, if the bottles are agitated, is entirely diffolved. After the mixture has been boiled, or for fome time expofed to the atmofphere, it yields to cauftic ammoniac a precipitate, in all refpects fimilar to that which is feparated by cauftic ammoniac from corrofive fublimate.

I would infer from thefe facts, that the white denfe fumes confift of mercury, or perhaps oxide of mercury, united to the nitrous etherized gas; and that, when the muriate of ammoniac containing them is expofed to the atmofphere, or is boiled, the gas feparates from the mercury; and the excefs of nitrous acid, which always comes over with nitrous ether, decompofes the ammoniacal muriate, and forms corrofive mercurial muriate or fublimate. This theory is corroborated, by comparing the quantity of gas eftimated to be contained in the fulminating mercury, with the quantities of gas yielded from alcohol and nitrous acid, with and without mercury in folution; not to mention that more ether, as well as more gas, is produced without the intervention of mercury; and that, according to the Dutch chemifts, the product of ether, is always in the inverfe ratio to the product of nitrous etherized gas. Should a further proof be thought neceffary, of the exiftence of the nitrous etherized gas in the fulminating mercury, as well as in the white denfe fumes, it may be added, that if a mixture of alcohol and nitrous acid holding mercury in folution, be fo dilute, and expofed to a temperature fo low, that neither ether nor nitrous etherized

gas

gas are produced, the fulminating mercury, or the white fumes, will never be generated: for, under such circumstances, the mercury is precipitated chiefly in the state of an inflammable oxalate. Further, when we consider the different substances formed by an union of nitrous acid and alcohol, we are so far acquainted with all, except the ether and the nitrous etherized gas, as to create a presumption, that no others are capable of volatilizing mercury, at the very low temperature in which the white fumes exist, since during some minutes they are permanent over water of 40° Fahrenheit.

SECTION XVI.

Hitherto, as much only has been said of the gas which is separated from the mercurial powder by dilute sulphuric acid, as was necessary to identify it with that into which the same acid can resolve the nitrous etherized gas; I have further to speak of its peculiarity. *

The characteristic properties of the inflammable gas, seem to me to be the following:
1st. It does not diminish in volume, either with oxygen or nitrous gas.
2dly. It will not explode with oxygen by the electric shock, in a close vessel.
3dly. It burns like hydrocarbonate, but with a blueish green flame. And,
4thly. It is permanent over water. (Section 12.)

It is of course either not formed, or is convertible into nitrous gas, by the concentrate nitric and muriatic acids; because, by those acids, no inflammable gas was extricated from the powder.

Should this inflammable gas prove not to be a hydrocarbonate, I shall be disposed to conclude, that it has nitrogen for its basis; indeed, I am at this moment inclined to that opinion, because I find that Dr. Priestley, during his experiments on his dephlogisticated nitrous air, once produced a gas which seems to have resembled this inflammable gas, both in the mode of burning, and in the colour of the flame.

After the termination of the common solution of iron in spirit of nitre, he used heat, and got, says he, † " such a kind of air as I had brought nitrous air to be, by exposing it " to iron, or liver of sulphur; for, on the first trial, a candle burned in it with a much " enlarged flame. At another time, the application of a candle to air produced in this " manner, was attended with a real though not a loud explosion ; and, immediately after this, *a greenish coloured flame descended from the top to the bottom of the vessel* in which the air " was contained. In the next produce of air, from the same process, *the flame descended* " *blue* and very rapid, from the top to the bottom of the vessel."

These greenish and blue coloured flames, descending from the top to the bottom of the vessel, are precisely descriptive of the inflammable gas separated from the powder. If it

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* It must be first noticed, that it is never pure when obtained from the nitrous etherized gas; nor am I aware how it is to be purified, unless the nitrous gas could be taken from it, without being converted into nitrous acid; for, by that acid, it would probably be itself converted into nitrous gas.

† Priestley on Air, Vol. II. p. 55. Birmingham. 1790.

can be produced with certainty by the repetition of Dr. Prieftley's experiments, or fhould it by any means be got pure from the nitrous etherized gas, my curiofity will excite me to make it the object of future refearch; otherwife, I muft confefs, I fhall feel more difpofed to profecute other chemical fubjects: for, having reafon to think that the denfity of the acid made a variation in the product of this gas, and having never found that *any* acid, however denfe, produced an immediate explofion, I once poured 6 drams of concentrate acid upon 50 grains of the powder. An explofion, nearly at the inftant of contact, was effected: I was wounded feverely, and moft of my apparatus deftroyed. A quantity moreover of the gas I had previoufly prepared, was loft by the inadvertency of a perfon who went into my laboratory, whilft I was confined by the confequences of this difcouraging accident. But, fhould any one be defirous of giving the gas a further examination, I again repeat, that as far as I am enabled to judge, it may with fafety be prepared, by pouring 3 drams of fulphuric acid diluted with the fame quantity of water, upon 50 grains of the powder, and then applying the flame of a candle until gas begins to be extricated. The only attempt I have made to decompofe it, was by expofing it to copper and ammoniac; which, during feveral weeks, did not effect the leaft alteration.

(*To be continued.*)

IV.

Account of a Series of Experiments, undertaken with the View of decompofing the Muriatic Acid. By Mr. WILLIAM HENRY[*].

MODERN chemiftry, notwithftanding its rapid advancement during the few laft years, ftill prefents to its cultivators feveral interefting objects, both of analytic and fynthetic inquiry. Among the former, the decompofition of the muriatic and of certain other acids, holds a diftinguifhed place; for our curiofity refpecting the nature of thefe bodies, is ftrongly excited, by the influence which the difcovery would have on the general doctrines of chemical fcience, as well as on the explanation of individual facts. The theory of the formation of acids, for example, one of the moft important parts of the new fyftem of chemiftry, muft be regarded as incomplete, and liable to fubverfion, till the individual acids now alluded to have been refolved into their conftituent principles. To the beft of my knowledge, however, we are not in poffeffion of a fingle fact that gives the fmalleft infight into the conftitution of the muriatic acid; and the attempts to effect its analyfis, can only therefore be directed by the analogy of the decompofition of other bodies, which, from fimilarity of character, are arranged in the fame clafs.

[*] Philof. Tranf. 1800, page 188.

One of the firft objects, in the analyfis of a compound body, fhould be its complete feparation from all other fubftances, which, by their prefence, may tend to introduce uncertainty into the refults of the proceffes that are employed. But it is feldom that a fimplicity fo defirable can be obtained in the objects of chemical refearch; for, agreeably to a known law of affinity, the laft portions of any fubftance are feparated with peculiar difficulty; the force of attraction appearing to increafe, as we recede from the point of faturation. In a liquid ftate, the muriatic acid is a totally unfit fubject for analytic experiment; for, in the ftrongeft form under which it can be procured, it ftill contains a large proportion of water. This watery portion, befides the complexity which it introduces into the refults of experiments, prevents any combuftible fubftance that may be applied, from acting on the truly acid part; becaufe that clafs of bodies, having lefs difficulty in attracting oxygen from water than from the acid, will neceffarily take it from the former fource. The ftate of gas, therefore, is the only one in which the muriatic acid can become a proper object of analyfis.

In the feries of experiments on this gas, which I am now about to defcribe, I employed the electric fluid, as an agent much preferable to artificial heat. This mode of operating enables us to confine accurately the gafes fubmitted to experiment; the phænomena that occur during the procefs, may be diftinctly obferved; and the comparifon of the products, with the original gafes, may be inftituted with great exactnefs. The action of the electric fluid itfelf, as a decomponent, is extremely powerful; for it is capable of feparating from each other, the conftituent parts of water, of the nitric and fulphuric acids, of the volatile alkali, of nitrous gas, and of feveral other bodies, whofe components are ftrongly united. I began, therefore, with examining attentively the effects of the electric muriatic acid gas, without admixture *.

SECTION I.

On the Effects of Electricity on Muriatic Acid Gas.

When ftrong electrical fhocks were paffed through a portion of muriatic acid gas, confined in a glafs tube over mercury, the following appearances took place. The bulk of the gas, after 20 or 30 fhocks, was confiderably diminifhed; and a white depofit appeared on the inner furface of the tube, which confiderably obfcured its tranfparency. In fome inftances, both the contraction and depofit were much more remarkable than in others. The gas which iffued from muriate of foda, foon after the affufion of fulphuric acid, and while the charge was yet warm, exhibited thefe appearances in an eminent degree.

* The gafes fubmitted to the action of electricity, in the following experiments, were confined in ftraight glafs tubes of various diameters, armed at the fealed end with a conductor of gold, or of platina, but generally of the latter metal. The fhocks were as ftrong as could be given without breaking the tubes, which, notwithftanding every precaution, were often fhattered by the force of the explofion. Each meafure of gas is equal to the bulk occupied by a grain of mercury.

Of

Of this gas, 307 meafures were reduced, by 20 fhocks, to 227, or were contracted nearly one-fourth. Gas from the fame materials, after they had continued working for fome hours, was diminifhed, by fimilar treatment, only about a twelfth. Thefe effects, therefore, it feemed probable, depended in fome meafure on the prefence of moifture; and I accordingly found, that muriatic acid gas, after more than a week's expofure to muriate of lime, brought into contact with it immediately after cooling from a ftate of fufion, was fcarcely diminifhed at all; and that the depofit, though it ftill occurred, was lefs copious in quantity. This depofit was not, like corrofive fublimate, foluble in water; but had every property of the lefs faturated falt, calomel.

The mercury by which the muriatic acid was confined, was therefore evidently oxidated; and, to the combination of a part of the gas with the oxide thus produced, the diminution of bulk was doubtlefs to be afcribed. But it was uncertain from whence this oxygen was derived. It might either refult fro— the decompofition of the acid gas, or of the water chemically combined with it. The following experiments were therefore made, to determine this point.

Experiment 1. Through 1457 meafures of muriatic acid gas, 300 electrical fhocks were paffed. There remained, after the admiffion of water, 100 meafures of permanent gas, (or not quite 7 from each hundred of the original gas,) which, on trial, appeared to be purely hydrogenous.

Exper. 2. Of the gas, dried by muriate of lime, 176 meafures received 120 fhocks. The refidue of hydrogenous gas amounted to 11 meafures, or rather more than *6 per cent.*

Thefe experiments, and other fimilar ones, made on comparative portions of muriatic acid gas, in its recent ftate, and after expofure to muriate of lime, convinced me that it was impoffible, by this method, wholly to deprive the muriatic gas of water. The recent gas, however, when electrified in fmaller quantity than in experiment 1, gave a larger proportion of hydrogenous gas; which fhews, that fome portion of its moifture was removed, by expofure to muriate of lime. In order, if poffible, to procure the gas perfectly dry, another mode of preparing it was reforted to. Alum and common falt were firft well calcined, feparately, to expel their water of cryftallization, and, being then mixed, were diftilled together in an earthern retort. The gas proceeding from thefe materials, was received over dry mercury; but, though only the laft portion that came over was referved for experiment, it ftill, after the ufual electrization, afforded a product of hydrogenous gas.

In the courfe of the preceding experiments, I obferved that the diminution of the muriatic acid gas ftopped always at a certain point, beyond which it could not be carried by continuing the fhocks. Gas alfo, which had been thus treated, when transferred to another tube, and again electrified, did not exhibit any further depofit. It became interefting, therefore, to know, whether the production of hydrogenous gas had a fimilar

limitation;

limitation ; becaufe, the decifion of this queftion would go far towards afcertaining its fource. If the evolved hydrogenous gas arofe from the decompofition of the acid, it might be expected to be produced, as long as any acid remained undecompofed. But, if water were the origin of this gas, it would ceafe to be evolved, when the whole of the water contained in the gas had been refolved into its conftituent principles.

Experiments 3 and 4. Into two feparate tubes, I paffed known quantities of muriatic acid gas. Through the one portion, 200 difcharges were taken ; and through the other, 400. On comparing the quantities of hydrogenous gas produced, it proved to bear exactly the fame proportion, in each tube, to the gas originally fubmitted to experiment. Hence it may be inferred, that the hydrogenous gas, evolved by electrifying the muriatic acid, has its origin, not from the acid, but from the water which is intimately attached to it. The agency of the electric fluid appears alfo, from the following experiments, to be exerted, not only in difuniting the elements of water, but in promoting the union of the evolved oxygen with muriatic acid.

Exper. 5. A mixture of common air and muriatic acid gas, in the proportion of 143 of the former to 116 of the latter, was rapidly diminifhed by electrical fhocks; 30 of which reduced the whole to 111 *. The remainder confifted of muriatic acid and azote gafes, with a fmall proportion of oxygenous gas. The depofit formed on the tube was of the fame kind as before, but much more abundant.

Exper. 6. The fame appearances were occafioned, much more remarkably, by electrifying muriatic acid with oxygenous gas : and the contraction continued, till the mercury rofe fo as to touch the extremity of the platina conductor. At each explofion, a denfe white cloud was feen in the tube, which foon fettled on its inner furface, and was of exactly the fame chemical compofition as the one already defcribed. Nitrous gas and muriatic gas, when electrified together, underwent a fimilar change.

In order to afcertain whether the mercury by which the gafes were confined, in the above experiments, had any influence on their refults, they were repeated in an inftrument made, purpofely for the occafion, by Mr. Cuthbertfon, of London. It confifted of a glafs tube, ground at each end, with the view of receiving two ftoppers, each perforated with platina wire, which projected into the cavity of the tube. When the ftoppers were in their places, the extremities of the wires were at the diftance of about half an inch ; and, by properly difpofing the apparatus, electrical fhocks might be paffed, through any gas or mixture of gafes, with the contact only of glafs and platina.

Exper. 7. In this tube I electrified the muriatic acid gas, and then admitted to it an infufion of litmus. The fudden deftruction of its colour evinced the formation of oxygenated muriatic acid. Not the fmalleft depofit appeared on the tube.

* This experiment fuggefts an additional reafon, to that already given, for the greater diminution of the firft, than of the fubfequent portions of the muriatic acid gas ; for the former may be prefumed to have been much more adulterated than the latter, with the atmofpherical air of the veffels.

Experiments

Experiments 8 and 9. The fame phænomenon took place, when an infufion of litmus was brought into contact with a mixture of common air and muriatic acid, and of oxygenous gas and muriatic acid, after electrization in this inftrument; oxygenated muriatic acid being produced in both cafes.

The above facts prove, that the combination of oxygen with muriatic acid, in thefe experiments, is not occafioned by a pre-difpofing affinity in the mercury to combine with oxygenated muriatic acid; but that the electric fluid ferves actually as an intermedium, in combining the muriatic acid with oxygen.

From the relation of thefe experiments it appears, that not the fmalleft progrefs had been made by them, towards the decompofition of the muriatic acid. I refolved, there fore, to attempt its analyfis, in a fimilar manner, with the aid of combuftible gafes.

SECTION II.

Effects of electrifying the Muriatic Acid Gas with inflammable Subftances.

In a memoir read before the Royal Society, and inferted in their Tranfactions for 1797, I have fhewn, that when electrical fhocks are paffed repeatedly through a confined portion of carbonated hydrogenous gas, the water held in folution by the gas, is decompofed by the carbon, which forms a conftituent part of it; that carbonic acid is formed; and an addition made, of hydrogenous gas. Hence, the bulk of the carbonated hydrogen gas is confiderably enlarged by this procefs; which fhews, by its refults, that the affinity of carbon for oxygen, is rendered much more powerful and efficient by the electric fluid. I have fince found, that other oxygenated fubftances are decompofed, by electrifying them with carbonated hydrogen gas. Nitrous gas, for example, is fpeedily deftroyed by this procefs, and carbonic acid and azotic gafes are obtained.

Every attempt to decompofe the muriatic acid, muft be founded on the prefumption that it is an oxygenated fubftance; and thofe bodies promife to be the moft fuccefsful agents, that poffefs a ftrong affinity for oxygen. Now, of all known bodies, charcoal moft ftrongly attracts oxygen; and I have, therefore, repeatedly attempted the deftruction of this acid, by paffing it over red-hot charcoal. But, in a feries of experiments, which I made fome time fince, with this view, in conjunction, with Mr. Rupp, we foon found reafon to be diffatisfied with the difficulty and uncertainty of this procefs. An immenfe production of hydrogenous gas took place; but it was not eafy to determine whether it had its origin from real acid, or from water. Our experiments, however, though infufficient to furnifh decifive proof, induced us to believe that it had the latter origin.

It next occurred to me, that the comparative affinities of the muriatic radical, whatever it may be, and of charcoal, for oxygen, would be elegantly and fatisfactorily afcertained, by electrifying together the carbonated hydrogen and muriatic gafes. If the muriatic acid be capable of decompofition by carbon, it might be expected to be deftroyed by this procefs; and the exact quantity of acid decompofed, and the nature and quantity of the

products,

products, would thus be eafily determined. I electrified, therefore, the muriatic acid and carbonated hydrogen gafes, with the moſt fcrupulous attention to the phænomena and refults. That the electric fluid might not be mifapplied, in decompofing the water of the carbonated hydrogen gas, it was kept more than a week, before ufe, over quick lime, introduced to it while yet hot.

Exper. 10. Of this carbonated hydrogenous gas, 186 meafures were expanded, by 130 fhocks, to 211 ; that is the gas was increafed about $\frac{1}{8}$ its bulk.

Exper. 11. Of the fame gas, 84 meafures were mixed with 116 of muriatic acid gas, dried by muriate of lime. By 120 fhocks, the mixture was a little dilated. After the admiffion of a drop or two of water, there remained 91 meafures ; *i. e.* the addition of permanent gas was 7 meafures, or about as much as might have been expected from the muriatic gas alone.

Exper. 12. Eighty-three meafures of dry carbonated hydrogenous gas, with 89 of muriatic acid gas, received 200 fhocks. The permanent refidue, after the admiffion of water, was 101 meafures: the addition, therefore, amounted to 18. Of the added 18, 6 may be accounted for by the decompofition of the water of the muriatic gas, and 10 by that of the carbonated hydrogenous gas. There remain, therefore, only 2 meafures that can be fuppofed to be produced from the muriatic acid gas; a quantity too fmall to afford grounds for fuppofing them to arife from decompofed acid.

Exper. 13. Dry carbonated hydrogenous gas 132 meafures,

　　　　　mixed with dry muriatic gas　　108
　　　　　　　　　　　　　　　　　　　　　240
　　　　　by 200 fhocks, expanded to　　268

Part of this gas was then transferred to another tube, and the proportion of permanent gas afcertained. Through the remainder, 150 additional fhocks were paffed, before the amount of the gas thus evolved was determined. In both, it bore exactly the fame proportion to the original gas; which fhews, that by continuing the electrization, no further effects were produced.

(To be continued.)

V.

Eudiometric Obfervations. By Citizen BERTHOLLET[*].

As the air we breath is known to be compofed of oxigen and azote gas, the proportions of thefe two gafes, and the changes that may take place in the atmofphere, have been conftant objects of enquiry. But the beft method of acquiring that knowledge, or the refult on which the greateft reliance may be placed, is not yet determined.

* Extracted from the Memoirs on Egypt, and inferted in the Annales de Chimie, tom. XXXIV. p. 73.

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The property of nitrous gas to abforb oxigen gas was firft made ufe of; but it was then thought fufficient to compare the diminutions produced in the air with which the experiment was made, and the purity of any air was confidered to be proportioned to the diminution it fuffered.

Inquiries were afterwards directed to afcertain the real quantity of oxigen gas that combined with the nitrous gas; in order to determine by the abforption which took place in the two gafes when mixed together, the relative proportions of oxigen and azote gas contained in the air of the atmofphere.

But the nitrous gas does not always afford conftant refults, unlefs by carefully obferving the fame manipulations. Ingenhoufz has long fince fhewn when the proportion of the oxigen gas is to be determined, there is no eftablifhed law by which the diminution attributed to the oxigen gas can be meafured, or that of the nitrous gas which becomes concentrated with it.

I am informed by an extract from the bulletin of the Philomathic Society, that M. Humboldt has endeavoured, by fome very ingenious experiments, to obviate the uncertainty which prevails with regard to the differences found in the nitrous gas; and that he propofes, as a ftrict method of determining by this gas the exact proportion of the oxigen gas, feveral corrections of the quantities; but I fhall prove by experiments, upon which I am ftill employed with Citizen Champy the younger, that this method is founded on fuppofitions which are inadmiffible [*].

The proof by hydrogen gas, for which we are indebted to Volta, has much precifion, efpecially when it is made upon oxigen gas; but it requires a complicated apparatus, and the hydrogen gas may differ according to the quantity of carbon it holds in folution, which may caufe the refult to vary confiderably. Neverthelefs, this method may be confidered as fufficiently exact when the different airs are merely to be compared, and the fame hydrogen gas is ufed to make this comparifon. But the fame accuracy cannot be had when the abfolute quantity of oxigen is to be determined. The proportions by weight, of the oxigen and hydrogen that enter into the compofition of water, are known with enough precifion; but the relative fpecific gravities of the two gafes are not well enough determined; and they alter too much, according to the difference of the hydrogen gas, to admit of judging exactly of the diminution which takes place by the combuftion which fhould be attributed to the oxigen and to the hydrogen gas, and determine by that means the quantity of oxigen gas that is contained in the air with which the experiment is made.

The liquid fulphuret of alkali offers a double advantage in giving at the fame time the comparative ftate of the different airs which are examined, and the proportion of the oxigen gas they contains. For all the diminution muft here be attributed to the oxigen,

[*] Thefe experiments were not finifhed when I left Cairo, and I did not bring the notes of feveral refults; for which reafon I have been obliged to repeat them, and fhall foon communicate them to the world.---B.

which

which is divided between the oxigen and nitrous, or the hydrogen gas in the preceding experiments. There is no neceffity for any other corrections than thofe which the difference of temperature and the preffure of the atmofphere require between the inftant that the experiment is began, and that when the diminution of the air is meafured.

It cannot be fuppofed that the abforption of the oxigen would be incomplete when water fufficiently charged with fulphuret of alkali is ufed; for there is a great difference between the power which fulphuret exerts on the oxigen gas, and the weak action the azote in the gafeous ftate may employ on the oxigen. And if any diminution be found in the azote which is entirely feparated by this means, by afterwards mixing it with nitrous gas, I fhall fhew that this diminution ought not to be attributed to the oxigen.

It muft be admitted, that it cannot be faid that the real volume of the azote gas is that which was obtained; becaufe azote may hold fulphuret in folution, or very probably the fulphurated hydrogen which always exifts in liquid fulphuret; and it certainly has the fmell, though this fmell difappears on wafhing it in a little water without any fenfible diminution of volume, fo that the difference of volume which takes place in this folution muft be extremely fmall.

There can be no apprehenfion that the azote fhould be abforbed by the fulphuret, for if this abforption took place it would continue. But the quantity of azote remains conftantly the fame after the oxigen is abforbed.

It is therefore, poffible to determine the proportion of oxigen which is found in any quantity of air, by means of the liquid fulphuret, with all the precifion that can reafonably be expected in chemiftry.

The inconvenience of this method is, that the fulphuret acts flowly, and requires longer time, and particularly a low temperature; befides it gives no certain indication when the abforption is compleated, except the ceffation of this diminution, which requires ftill more time to be afcertained.

Guyton has propofed to ufe the dry fulphuret, by applying the heat of a candle to an apparatus he has defcribed. I have not tried this method, but it appears to me, that fo fmall a portion of matter may not come into good contact with fo large a volume of air, by which means the abforption of all the oxigen may be rendered uncertain. The experiment does not prefent any indication that proves its certainty.

I have recommended the flow combuftion of phofphorus. I firft place a cylinder of phofphorus fixed on a ftick of glafs in an upright veffel, in which the air fubjected to experiment is contained under water. If the temperature of the room be high, the veffel is immerfed in water, in order that the phofphorus may not melt. For the evaporation of the furface of the water keeps it at a temperature fome degrees below that of the atmofphere. Thus, during the experiments that I made here, the thermometer remained at about 36° degrees of the thermometer centigrade, and the bath in which the experiments were made was at fix degrees below this temperature. Immediately after the phofphorus is introduced into the air, a cloud is formed, which defcends and mixes with the water.

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When the operation is finiſhed, this cloud is not longer to be perceived. It is luminous in the dark, and as ſoon as it has diſappeared, the abſorption no longer proceeds even during the ſpace of ſeveral days. So that by this means a certain indication of the concluſion of the experiment is obtained; and if it be made in a narrow tube, it requires no more than two hours at the temperature I have mentioned *.

The air with which the experiment was made, is meaſured in a graduated tube, and when the operation is concluded, the remaining gas is meaſured in the ſame tube with the uſual precautions that the changes of temperature and preſſure of the atmoſphere require.

The diminution obtained by phoſphorus is found to be conſiderably leſs than that obtained by the ſulphuret; but the proportions are always the ſame, becauſe the phoſphorus being diſſolved in azote, as I have proved, takes the gaſeous form, as do all ſubſtances which are diſſolved in a gas. The volume of azote is thus augmented, and ſeveral experiments have ſhewn that this increaſe is about a fortieth part.

The difference that appears in the diminution between the phoſphorus and the ſulphuret cannot be cauſed by the one ſeparating the oxigen leſs exactly than the other; for the phoſphorus acts ſo ſtrongly on oxigen when it is diſſolved in azote, that it is ſufficient to paſs the phoſphorated azote through water to render it luminous, and make it burn with the oxigen it meets.

It appears to me that ſulphurated hydrogen, which is ſoluble in the azote, is capable of precipitating moſt of the phoſphorus. For if the phoſphorated azote be placed over the ſulphuret of alkali, its bulk is diminiſhed; but the total diminution is not ſo great as if the air had been firſt placed on the ſulphuret. The phoſphorated azote, which has been thus diminiſhed by ſulphuret, is no longer capable of being rendered luminous by coming in contact with oxigen gas.

If phoſphorus be introduced into the azote which is in contact with the ſulphuret, it produces no ſenſible effect; but if the azote be waſhed by being paſſed through clear water, it will be ſufficient to render the water immediately luminous, ſo that the water retains the greateſt part of the ſulphurated hydrogen, and gives a very ſmall quantity of oxigen to the azote gas, which by this operation is made capable of diſſolving phoſphorus, and of acting on the ſmalleſt quantity of oxigen gas.

I have obſerved, that the azote which has been expoſed with the ſulphuret, does not apparently loſe or acquire any thing in its bulk, when it has been paſſed through the water, and the combuſtion that takes place when phoſphorus is introduced is ſo ſmall, that it can ſcarcely be appreciated.

I, therefore, conclude, from all the preceding facts, that the method with phoſphorus is at the ſame time the moſt convenient and the moſt accurate, and that the time is not

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* Theſe experiments were repeated at a temperature from ſix to ten degrees of the thermometer centigrade, and it only required ſix or eight hours for the operation to be concluded. M. Humboldt mentions experiments which has laſted ſeveral days, after which the gas ſtill reddened with the nitrous gas. He muſt have tried very different methods to have produced ſo great a difference in the reſults.

F f inconvenient;

inconvenient; that it gives the moſt poſitive reſults with regard to comparing the airs, and requires only one correction of a fortieth part of the volume of the remaining gas, when the proportion of the oxigen gas is to be determined with the ſame accuracy as may be obtained by the ſulphuret, which appears to me to be the moſt exact approximation that can be procured by any means known [*].

Several experiments made by Citizen Champy and myſelf in the laboratory of the Inſtitute of Egypt, with the ſulphuret of alkali and phoſphorus, in which we applied the corrections ſuited to the changes of temperature, as well as for the expanſion of the azote by the phoſphorus, have proved, that the proportion of oxigen gas in atmoſpheric air in that country was at leaſt twenty two parts out of an hundred. We have not found in a great number of experiments any difference exceeding one hundredth part; and it is more natural to attribute this ſmall variation to the inevitable imperfections attendant on all chemical proceſſes, than to any real change in the ſtate of the atmoſphere.

Multiplied experiments I have made at Paris by the ſame methods, appear to me to prove, that the proportion of oxigen is there very nearly the ſame as in Egypt. I muſt, however, remark, that I have not ſo accurate a recollection of the reſults, as to affirm a perfect equality. It is, therefore, neceſſary to repeat theſe experiments at Paris, in order to eſtabliſh the compariſon beyond all doubt [†].

Several celebrated chemiſts and natural philoſophers give the oxigen gas a conſiderably higher proportion than that which I have determined; they pretend that they have found conſiderable variations in the air of different places and at different times. Humboldt has recently made the proportions of oxigen gas to vary from twenty-three to twenty-nine centimes. But I have not perceived theſe variations at the diſtance of Cairo from Paris,

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[*] Mr. Humboldt pretends, that he has formed a ternary combination of phoſphurus azote and oxigen (Ann. de Chim. 30 Thermidor, An. 6.) His opinion is founded on ſome experiments, in which he has obſerved that the phoſphorus produces unequal diminutions in the atmoſpheric air, expoſed to its action in tubes of equal ſize, and under the ſame circumſtances. The difference between the two tubes was from 115 to 156. It is on this alone that he eſtabliſhes the exiſtence of oxided phoſphuret of azote. I have ſeen no appearance of this difference in the numerous experiments I have made at Paris and at Cairo at two different periods.

[†] The experiments made at Paris gave about one two-hundreth part more of reſidue. They coincide perfectly with thoſe which I had formerly made. It appears to me likely that this ſmall difference ariſes from the air at Cairo being very dry. At the time when I operated, the air, probably, became ſaturated with water, and acquired by that means a ſlight dilation rather than a difference between the conſtituent parts of the two atmoſpheres. But the reſults prove that the proportion of oxigen gas might be determined at a little above twenty-two parts out of an hundred.

Experience will eaſily decide between the opinion of Mr. Humboldt and mine. It is only required to examine, whether it be true that phoſphorus and the liquid ſulphuret produce a uniform and conſtant reſult, as I affirm. I do not ſpeak of the vivid combuſtion of phoſphorus, with regard to which there are other conſiderations to be made. When the experiment of the ſlow action of the phoſphorus is made, the cylinder of phoſphorus ought to occupy a large part of the height of the tube in which the experiment is made, in order that all the atmoſpheric air may be ſubjected to its action.

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at very remote periods, and in very different climates. This difference may be attributed to the nitrous gas, which is ufually made ufe of. It is allowed, that experiments of this kind cannot be compared, if any alteration happens in the water made ufe of; or in the nitrous gas; or in the dimenfions of the tube; or in the manipulations. Corrections founded on fuppofitions, very far from being juftified by experiment, are very feduloufly applied, and regular methods are rejected, to which no exception has ever been made but what has been overthrown by actual obfervation. Perhaps they are found too fimple in Europe.

And, indeed, how can it be imagined that the atmofphere, which is continually agitated by motions that fuddenly remove its parts, and which renew and alter its fituations, can vary confiderably between one village and another. There is, however, an exception to be made for thofe places which are elevated above the level of the fea. The difference of fpecific gravity between oxigen and azote gas, which in the elaftic ftate exert a very weak mutual action, explain that difference which has been found in their proportions.

VI.

Analyfis of a new Variety of Lead Ore. By RICHARD CHENEVIX, Esq. *Communicated by the Author.*

THE mutual elucidation which kindred fciences receive from each other, muft ever increafe as the means of inveftigation become more accurate and extenfive, and in no inftance can this mutual connection and improvement be more ftrikingly difplayed, than when the influence of newly difcovered facts extends itfelf through the whole dominion of natural knowledge.

When Sir Ifaac Newton, by comparing the refracting power of water with that of other bodies, made the bold inference that it confifted partly of inflammable matter, and that the diamond poffeffing that power in a ftill higher degree, muft be ranked among combuftible fubftances, it was little fufpected that chemiftry would prove this not to be the vague doubt of an imagination mifled by a favourite hypothefis, but the firm affertion of genius guided by ftrong analogical conviction. The progrefs made fince that period affords numerous examples of fuch happy coincidences; and it cannot fail to be highly fatisfactory, when two fciences, tending to the fame point by different means, concur in affording fimilar refults. The Abbé Hauy, on comparifon of the cryftalline form of the emerald and the beril, after Monf. Vauquelin had juft difcovered his new earth in the latter, did not hefitate to pronounce his ftrong fufpicion of its being a conftituent part of both; and analyfis proved the validity of his opinion. I am happy that I have it in my power to furnifh another example of the accuracy of thofe principles, to which he has fo particularly

devoted

devoted his attention; and to call in the aid of chemiſtry to ſhew with what certitude the penetrating eye of the able cryſtallographer can, from external ſtructure, arrive at an intimate knowledge of pure mineralogical ſubſtances.

In the valuable collection of the Right Honourable Charles Greville, there are many different varieties of carbonated lead, acknowledged and claſſed as ſuch from their external characters: one, however, which, as far as I can learn, does not exiſt elſewhere, excited ſome doubt in the mind of Monſ. le Comte de Bournon, from its aſſuming a peculiar modification of form, not hitherto remarked. He offered it to me for chemical examination; obſerving at the ſame time, that though thoroughly convinced it was carbonated lead, he was equally ſure it muſt be in ſome particular ſtate of combination, either in the proportion of its known principles, or by containing ſome ſubſtance not found in the other varieties. I undertook the analyſis, and to render the approximation more ſtriking, he has favoured me with the following accurate mineralogical deſcription.

Mineralogical Characters, in which it chiefly differs from ſimple Carbonated Lead, by Monſ. le Comte de Bournon.

This variety of lead ore differs from ſimple carbonated lead.

1. It is much ſofter than carbonated lead, making no impreſſion whatever on it, and being eaſily ſcratched and worn by rubbing againſt it.

2. Its ſpecific gravity is much leſs, being 60.651. Many experiments on pure cryſtals of carbonated lead give a ſpecific gravity of 72.357. Many authors fix it at 44,63, but they have undoubtedly taken the heavy ſpar for it.

3. Its fracture is glaſſy, and much like that of the precious ſtones.

4. Its form is different in many reſpects. Like moſt other natural oxides of lead, its lamellæ appear to be rectangular; but their union, which very ſeldom forms a perfect cube in the carbonated lead, produces that form more frequently in this ore. The various laws of decreaſe, which theſe lamellæ undergo, in order to produce that cube (which is very frequently lengthened, or flattened) as well as the different cryſtalline modifications, are alſo very different.

Sometimes the decreaſe is at the angles, ſo as to replace thoſe of the cube or parallelipipedon by planes of greater or leſs extent, and perpendicular to the axis of the cube. Sometimes, if we conſider the cube or parallelipipedon as a priſm, we ſhall find that the longitudinal edges of the priſm are replaced by planes perpendicular to thoſe edges.

Very frequently, the cryſtal being ſtill conſidered as a priſm, the edges of the terminating faces are replaced by planes inclined to the faces of the priſm, ſo as to form an angle of 148°, and another angle of 122°, with the terminating faces. The cryſtallization under that form has ſometimes been of ſufficient duration, to allow at the two extremities of the cryſtal a tetraedal pyramid, ſeldom complete, but which ſometimes preſerves no marks of terminating faces, but a ſmall plane replacing the ſummit of the pyramid; when complete, the oppoſite faces join at the ſummit in an angle of 64°.

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At other times, the cryftal having undergone the preceding modification, fo as to leave a great part of the cubical faces, is fubject to another, which replaces each of the longitudinal prifmatic edges by two planes, inclined upon the prifmatic faces, with which they form an angle of 161° 30′, then a tetraedral pyramid, more or lefs truncated at the fummit, feems placed on each of the longitudinal faces (the cryftals ftill confidered as prifmatic) the faces of which, taken two by two, are differently inclined ; two of them tend to meet at the fummit in an angle of 143°, the two others in an angle of 116°. Such are the different modifications of this ore, to which there is nothing fimilar among the fimple carbonated leads.

All thefe varieties are extremely beautiful and perfect in their cryftallization ; they are generally of a light ftraw colour, though fometimes of a perfectly colourlefs and clear tranfparent white, with a luftre far more brilliant than the fimple carbonated leads.

Its topographical hiftory I fhall tranfcribe in the words of Mr. Greville. " About " fourteen or fifteen years ago, I purchafed a piece of this mineral at Matlock, in Derby- " fhire ; the fhopkeeper not knowing from what mine it came, or unwilling to inform me, " I left a commiffion for him to fend me all he could procure, and repeated my order for " feveral years fucceffively : I traced fome fpecimens, which had been fold, and recovered " them. The largeft cryftal, Dr. Darwin, of Derby, gave up to me. I fuppofed, from " my enquiries, that it came from the diftrict of mines intended to be unwatered by the " Cromford level, as I was affured of my having no chance of any more till that level was " compleated ; and fince that period the water has continued in a degree, that many " mines which were then open, are now drowned."

The cryftals are upon a large facetted galena, and commonly from half an inch to an inch in length. The largeft mentioned in Mr. Greville's note, is an inch and a half long, an inch broad, and half an inch thick, of the fineft tranfparent yellow. This ore, however, does not appear to be an exclufive production of this country ; for there are two fpecimens in Mr. Greville's collection, which are in much fmaller and perfectly white cryftals, which feem perfectly the fame ; but they are not of this country, and, as I believe, come from the Hartz ; no doubt it will be found elfewhere, when the attention of naturalifts fhall have been more particularly directed to that object.

ANALYSIS.

One hundred parts, chofen from a cryftal perfectly tranfparent, regular, and pure, were reduced to powder ; during this operation they frequently caked, and offered an elaftic refiftance, not unlike muriate of ammonia in the fame circumftances ; they were thrown into a known weight of nitrous acid ; a fudden effervefcence took place, and the diminution was = 00,6, and the elaftic fluid was afterwards proved to be carbonic acid. As foon as the effervefcence had fubfided, there remained at bottom a white powder, infoluble without the affiftance of heat ; but after a flight ebullition, this refiduum was reduced to 00,1, which was fulphate of barytes, and quite foreign to the intimate nature of the ore. Into the diffolution was poured cauftic ammonia, in preference to a fixed alkali,

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because the oxide of lead is foluble in this laſt: A precipitate fell down, and the remaining liquor, filtered and neutralized, was fubmitted to the following teſts:

Nitrate of lime afforded no precipitate: the prefence, therefore, of the phofphoric, arfenic, or molybdic acids, was entirely precluded.

Nitrate of barytes afforded no precipitate, therefore no fulphuric acid was prefent.

Nitrate of lead, a flight precipitate, but quickly foluble by the addition of water. Theſe appearances convinced me that none of thoſe acids, hitherto found to mineralize lead ores were to be expected in the fpecimen now to be examined; and the properties of the precipitate cauſed by nitrate of lead, agreeing nearly with thoſe of muriate of lead, I dropped in fome nitrate of filver: a very thick and abundant precipitate took place, which had all the characteriſtics *, not to be miſtaken, of muriate of filver. It weighed 42, which, according to Bergman, contain 7 of muriatic acid; but according to Klaproth, in his analyſis of the molybdate of lead, 7 of muriatic acid faturate about 45 of oxide of lead; and again, according to Bergman, 6 of carbonic acid faturate 34 of oxide of lead, which would have left a deficit of 8. The experiment was, therefore, repeated; and being aware that ammonia could no more decompoſe the whole of the muriate of lead, than could any other alkali, the nitrate of filver was dropped immediately into the nitric diſſolution of the ore; and upon repeating this eſſay twice, 48 of muriate of filver were obtained each time, containing 8 of muriatic acid; which in combination form 59 of muriate of lead; the lead afterwards fubmitted to various proofs, was found to be perfectly pure, and the proportions may be eſtabliſhed as follows:

Oxide of lead	-	-	-	51	}	Muriate of lead.	-	-	59
Muriatic acid	-	-	-	8					
Oxide of lead	-	-	-	34	}	Carbonate of lead	-	-	40
Carbonic acid	-	-	-	6					
				99					99

With about one of heavy fpar, as before mentioned.

It does not, however, appear probable, that theſe two falts of lead exiſt feparately in the ore; its perfect cryſtallization and tranfparency fufficiently indicate it to be in the ſtate of a triple falt, or carbonated muriate of lead, combined on the above proportions, and in the following order:

Muriatic	}		8			
and	}	acids		Oxide of lead	85
Carbonic	}		6			
			14			99

The very great fcarcity of this ore prevented me from fubmitting it to more experiments; however, upon expoſing a certain quantity of it to a red heat, a thick vapour aroſe,

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* I need mention no other than this; muriate is the only falt of filver which is not foluble in almoſt every acid, and is inconteſtibly the moſt delicate and abfolute re-agent in chemiſtry; as well to demonſtrate the prefence, as to determine the quantity either of filver or muriatic acid.

which

which manifested no smell of any kind, and could not, therefore, be suspected to contain either arsenic or antimony: I suppose it to be muriate of lead, as notwithstanding its great fusibility, and its supposed fixity, it is, in fact, in some degree volatile, as I have repeatedly observed on other occasions.

No more than twenty-two parts of water are necessary to dissolve one of muriate of lead, and the dissolving power is much encreased by the presence of an acid, yet we see in the case before us it could resist the action of a much greater quantity, even when aided by a considerable portion of nitric acid in excess; such examples are not rare in natural substances, compared with the slight aggregation of those which art can produce.

The existence of a natural muriate of lead has been suspected more than once. Ferber examined a specimen found at Meis and Bleystadt, in Bohemia, which he imagined to contain muriatic acid; but the Baron de Born assures, that Klaproth, upon an accurate analysis, had determined those same grey hexaedral crystals, which Ferber had procured, to be merely phosphate of lead, a variety then well known. Monf. Sage had likewise asserted as much; but the specimens which he had tried, being further submitted to investigation by Mr. Laborie, and by some members of the Royal Academy of Paris, were found to be totally void of muriatic acid, and are now universally acknowledged as pure carbonated leads.

The authorities here mentioned are too respectable for me to discuss their merits. I shall only add, that after the experiments above described, and which were performed in the presence of Monf. de Bournon, he no longer refuses his assent to this new species of lead ore, now first proved, if not discovered, but has added it to those varieties already admitted in the system of mineralogy.

VII.

Experiments on the Chemical Effects of Galvanic Electricity. By Mr. WILLIAM HENRY.

To Mr. NICHOLSON.

SIR,

Manchester, July 20, 1800.

IN addition to the interesting facts, respecting the chemical action of galvanism, which appeared in the last Number of your Journal, I beg leave to communicate the following experiments. The apparatus, which I employed, needs no further description than that it consisted of half crowns and similar pieces of zinc, varying in number as occasion required, with pieces of woollen cloth interposed, soaked in a saturated solution of common salt. I tried muriate of lime, as a substitute for common salt, but without any better effect.

1. To

1. To the hiftory of the phenomena, attending the decompofition of water by this new influence, which have been already accurately defcribed by yourfelf, and by Meffrs. Carlifle and Cruickfhank, I have little to add. If the water be confined by mercury, in a tube with a conductor fealed hermetically into one end, a production of gas only takes place, when the conductor communicates with the filver fide of the apparatus; and if the order be inverted, *i. e.* if the mercury be connected with the filver end, and the conductor at the fealed extremity of the tube with the zinc end, of the pile, no gas whatever is produced, though the agitation of the furface of the mercury in the tube fhews that the influence is ftill tranfmitted. On paffing up a wire of any metal through the quickfilver, fo that its extremity may rife into the water, gas then begins to afcend copioufly from the lower wire. Does not this fhew, that pointed bodies are more effectual in decompofing water than bodies with rounded furfaces?

It may be worth while to obferve, that the change of the colour of litmus from blue to red, occurs in diftilled water, even after long boiling. From Dr. Pearfon's experiments*, however, it appears that air is feparated by electric fhocks from water, which has been expofed under a receiver, exhaufted by a powerful air pump.

2. Concentrated fulphuric acid was fubmitted to this influence, in a glafs tube, furnifhed with two platina conductors, the open end of the tube being immerfed in a cupful of the fame acid. Gas was produced in great plenty, one half of which was abforbed by fulphuret of potafh. The remainder was hydrogenous gas, from the decompofition of water, which even the ftrongeft fulphuric acid neceffarily contains. But as the oxygenous gas was fufficient to have faturated twice the quantity of hydrogen gas evolved, one half of the former muft have had another origin than water, and may he afcribed to the decompofition of the acid. Indeed, during the paffage of the electricity, a white cloud was obferved to furround the wire from which the gas afcended, and which was probably difoxygenated fulphur.

3. Perfectly pure and colourlefs nitric acid was rapidly decompofed in a fimilar apparatus. The acid affumed a ftraw colour, and gas was obtained, confifting of oxygenous and azotic gafes, in the proportion of 530 of the former to 151 of the latter.

4. From liquid muriatic acid, 424 parts of gas were evolved, of which 144 were oxygenous, and 280 hydrogenous. Thefe gafes had, doubtlefs, their origin from the decompofition of water. The quantity of the acid being confiderable, in proportion to the gafes produced, it did not exhibit the marks of having become oxygenated during the experiment.

5. A tube was prepared with platina conductors, covered with wax, and being filled with a faturated folution of oxygenated muriatic acid in water, the circuit was completed through it, between the two ends of the pile. The gas evolved was a mixture of oxygenous

* Phil. Tranf. 1797, or this Journal, vol. I.

and

and hydrogenous, in the proportion of 136 to 118. Now the 118 parts of hydrogen gas require 59 by meafure of oxygen gas for faturation. The remaining 77 parts of oxygen gas arofe from the de-oxygenation of the acid.

6. As no certain conclufions refpecting the compofition of the muriatic acid can be drawn from experiments, inftituted on it when condenfed by water, I was eager to try the effects of this new and powerful agent on the acid in a gafeous form. But it was neceffary to afcertain previoufly, whether the galvanic influence be capable of paffing through aëriform bodies. An argument to the contrary was furnifhed by the inefficiency of this influence in inflaming mixtures of oxygen and hydrogen gafes; for when the gafes evolved from decompofed water, have difplaced the whole of the liquid above the extremity of the upper wire, the evolved gafes do not take fire, as they do in the experiment of Dr. Deiman. Another fact, fhewing the non-tranfmiffion of this influence through air, is that when a little air is let up into an inverted tube filled with water, and furnifhed with two conductors, fo as to difplace the water a little below the extremity of the upper wire, no gas afcends, though the lower wire be connected with the filver bafe of the pile. I found, alfo, that a divifion in a piece of tin-foil pafted on glafs, even when fo fmall as not to be difcerned without a magnifier, interrupted the paffage of the galvanic fluid. But this non-conducting property might poffibly be peculiar to common air, and it was defirable, therefore, that the tranfmitting power of other gafes fhould be afcertained.

One of the moft delicate tefts I know of the effect of electricity on gafes is furnifhed by the phofphorated hydrogen gas, which, by common electrical fhocks, or even fparks, is expanded with great rapidity. I expofed, therefore, a portion of this gas over mercury, in a tube furnifhed at one end with a platina conductor, which was connected with the filver end of the pile, while the mercury that confined the gas was connected with the zinc end. But after ftanding feveral hours, no change had enfued. In a fimilar manner I expofed a mixture of muriatic acid and oxygenous gafes, which common electrical difcharges rapidly diminifh, without any contraction of bulk, or change of properties *. Neither was any permanent gas evolved by a fimilar treatment of muriatic acid gas alone. The deficiency of the property of tranfmiffion through gafes limits confiderably the ufe of galvanifm as a chemical agent, and has totally overturned my project of attempting, by its intervention, the analyfis of muriatic acid.

7. Finding it impracticable to tranfmit the influence through ammoniacal gas, I expofed to its action a portion of water perfectly faturated with this alcali. The refult, which has been confirmed by frequent repetitions, furprized me not a little. No oxygenous gas was produced; for the evolved gas was not diminifhed by fulphuret of potafh, neither did it inflame on paffing through it an electric fpark. On firing it with oxygen

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* I was mifled at firft to believe that a contraction had taken place, becaufe the fealing wax, with which the platina wires were covered, had abforbed a portion of muriatic acid gas.

G g gas,

gas, it was diminished very greatly, and a solution of sulphuret of potash, after having abstracted the excess of oxygen gas, left only a small bubble of azotic gas, which may be traced to the oxygen gas employed to effect the combustion.

8. A solution of caustic vegetable alcali, being exposed in a similar manner, the gas produced was pure hydrogen gas. During the process, the surface of the mercury became covered with a blackish film, especially at its edges where it was in contact with the glass tube. The black matter, however, was confined to the surface of the mercury, and was not generated at the same part of the tube at which gas was produced, viz. in contact with the platina wire. It still occurred even on using mercury which had been carefully distilled for the purpose.

The two last facts are the most curious that have occurred in the course of my experiments. In experiment 7, the volatile alcali was certainly decomposed, for had the hydrogen gas proceeded from the decomposition of water only, oxygenous gas would also have been obtained. Into what new combination does the azote, in this case, enter? It is not improbable that at the same instant both water and ammoniac are decomposed; that the hydrogen of both is converted into gas; and that the oxygen of the water, uniting with the azote of the alcali, composes nitric acid, which, combining with the ammoniac, produces nitrate of ammonia. The destruction of the vegetable alcali is not less certain. From the 8th experiment we derive a sufficient proof that it contains hydrogen. Azote is probably another of its constituents, but is prevented from appearing in the gaseous state by its union with oxygen, evolved, at the same instant, by the decomposition of a portion of water. These suggestions admit of being verified by a careful examination of the alcalis, after exposure to the influence of galvanism; and a third component of the vegetable alcali will perhaps be found in the black precipitate above described. I have not been inattentive, even hitherto, to these points; but the minuteness of the quantities submitted to experiment prevents me from speaking decisively, and I choose rather to reserve myself for the results of experiments now going on, than to incur the risk of being forced to retract a hasty assertion. In the mean time, I send you these particulars of unfinished experiments, because those who are practically engaged in investigating the properties of galvanism may derive, from a communication with each other through the medium of your Journal, some of the advantages of personal intercourse and cooperation.

<div style="text-align:center">

I am respectfully,

SIR,

Your obedient

humble servant,

WILLIAM HENRY.

</div>

Description

VIII.

Description of a new Instrument for repeating the Examination of the Strata which have been penetrated by the common boring Instrument used in Mine Works. By A. BAILLET, Inspector of Mines, and Professor at the Mineral School at Paris *.

AFTER remarking the great utility of boring, in order to ascertain the existence of mineral substances, particularly coal at great depths, the author observes, that it would be of great advantage if this operation could be easily repeated or verified. " It often " happens," says he, " that some doubts remain concerning the nature, the thickness, or " the position, of the principal mineral strata through which the borer has passed. This " uncertainty increases when there is reason to suspect the integrity of the persons em- " ployed; in which case there is but one method of removing the uncertainty, viz. to bore " the same excavation a second time with a tool of a larger diameter, and endeavour to " confirm the results of the first operation by observing those of the second." The slow- ness, the expence, and the other inconveniences of this method, may easily be imagined. To avoid these the author proposes the use of an instrument, which he calls a verifier (verificateur), which is of use to take at any required depth in the hole or rectiform, a sample of the ground which exists at that spot. This instrument, represented at fig. 1, plate 9, is composed of two principal pieces; a superior piece *a b*, in which there is a cylindrical cavity opening below; and another inferior piece *c d*, inserted in this cavity, and terminating in a cone.

These two pieces are connected together by means of two keys or pins *e e*, which pass through the upper piece, and occupy a circular groove *e f* in the lower piece.

The upper piece contains two knives *g g*, let into its thickness, and fixed at their upper extremity by a screw *h*. A gutter or groove *j*, of an helical form, is made from the lower part of each blade, for the purpose of conveying the fragments and powder scraped off by these knives into the cup or cavity of the lower piece *k*.

When the apparatus is addressed, the backs of the two knives touch each other at *l*, and the point of the cone *m* lies in the angle formed by the slopes of the two knives.

The extremity *s* of the instrument terminates in a screw, which is not represented in the present drawing, and the other extremity *r* likewise terminates in a screw; so that by means of these the instrument may be lodged in any part of an hole already bored. That is to say, a rod of any required length may be fixed below it by means of the screw *s*, and the other rods necessary to lower it down, may be screwed on above.

* Communicated to the Philomathic Society, and inserted in their Bulletin, No. 39, in the 8th republican year.

The

The author remarks, 1, that the two pieces of the instrument being connected together, it can easily be let down into any hole, and drawn up again; 2, that if the lower piece rests on a fixed support, the upper piece is capable of descending through a quantity determined by the space left in the excavation, which receives the two keys in *e e*; and that during this descent, the knives will be forced asunder by the cone of the lower piece forcing itself between their backs; 3, that the upper piece is capable of turning upon the lower, in which case the knives will cut away the ground, and the fragments will fall into the groove, and be conducted into the cup beneath; 4, lastly, that if the instrument be drawn up again, the two knives will retire into their cells, either by the effect of their own elasticity, or by that of a spring properly placed for that purpose, or by the re-action of the ground itself, which presses them as they rise.

The author afterwards enters more at large into the uses of his instrument, which the reporter of the society concisely states as being effected by adapting to the instrument a series of rods successively lowered into the cavity, and of which the length is such that the knives of the instrument shall be found at the exact height of the stratum intended to be examined when the lowest rod touches the bottom. Other rods are of course to be added above, in order to conduct and manage the instrument; in this situation of things nothing more is required than to turn the apparatus in the same manner as a borer, and when it is presumed that the knives have detached a sufficient quantity, the instrument is drawn up.

The author terminates his memoir by several essential observations. The first relates to the necessity of not leaving the upper rods to the action of their own weight, which would produce the great inconvenience of separating the knives too much at first, and endangering the upper rods by too great resistance. The rods may easily be lowered properly, and by degrees, by employing the frame and screw *a*, fig. 3, through which the upper rod passes.

The second observation relates to the different methods of adjusting the verifier to the precise height where the action is intended to take place. For this purpose a rod of the proper length may be forged, in order that it may be fixed to one or more of the common rods; or otherwise the operator may provide himself with two rods of one decimetre, one of two decimetres, and another of five decimetres, which system will give all the lengths from decimetre to decimetre, from 1 to 9, &c.

The third and last observation relates to the depth of the circular cut which the two knives will make. Under like circumstances a cavity of twice the dimensions may be made with one knife instead of two. The single knife must have the form represented in fig. 2.

General

IX.

*General Principles and Conſtruction of a Sub-marine Veſſel, communicated by D. Bushnell, of Connecticut, the Inventor, in a Letter of October, 1787, to Thomas Jefferſon, then Miniſter Plenipotentiary of the United States at Paris.**

THE external ſhape of the ſub-marine veſſel bore ſome reſemblance to two upper tortoiſe ſhells of equal ſize, joined together; the place of entrance into the veſſel being repreſented by the opening made by the ſwell of the ſhells, at the head of the animal. The inſide was capable of containing the operator, and air, ſufficient to ſupport him thirty minutes without receiving freſh air. At the bottom oppoſite to the entrance was fixed a quantity of lead for ballaſt. At one edge which was directly before the operator, who ſat upright, was an oar for rowing forward or backward. At the other edge, was a

* Tranſactions of the American Philoſophical Society. IV. 303.

This is the only modern inſtance I am acquainted with of the purſuit of ſub-marine navigation. The ſubject was largely and pleaſantly deſcanted upon by Merſennus in his *Tractatus de Magnetis Proprietatibus,* and Biſhop Wilkins has given a chapter at ſome length on the ſame ſubject, in his *Mathematical Magick.* (ed. 1648) where he affirms, that Cornelius Dreble had proved, beyond all queſtion, that the contrivance is feaſible by the experiments he made in England. The chapter of Wilkins is entertaining for a ſort of viſionary facility with which he removes the difficulties and enumerates the benefits of theſe ſub-marine enterprizes. For letting out and taking in ſuch things as the nature of the voyage may require, he recommends bags, or flexible tubes, ſomewhat reſembling the ſcupper bags of ſhips. The progreſſive motion may, he obſerves, be produced by fins or oars, which will operate with eaſe when the veſſel is truly equipoiſed, and if ſwiftneſs ſhould not be obtained, he ſuppoſes, the obſervations and diſcoveries to be made at the bottom of the ſea would abundantly recompence for that defect. The greateſt difficulty, in his apprehenſion, would be in the neceſſity of renovating the air for reſpiration and combuſtion; for remedying which, beſides the probability that cuſtom may render men capable of living in air of inferior purity, he has ſeveral philoſophical views and projects. The conveniencies and advantages he enumerates, are, 1. Privacy; as a man may thus go to any part of the world inviſibly, without being diſcovered or prevented. 2. Safety; from the uncertainty of tides and tempeſts which vex the ſurface; from pirates and robbers; and from the ices which ſo much endanger other voyages towards the poles. 3. It may be of uſe to undermine and blow up a navy of enemies. 4. Or to relieve a blockaded place: 5. And as the proſpect enlarges in the mind of our author, he proceeds to contemplate the unſpeakable benefit of ſub-marine diſcoveries. Experiments on the aſcent and deſcent of ſubmerged bodies; the exploration of the deep caverns and paſſages of the waters of the ocean; obſervations on the nature and kinds of fiſhes, with the allurements, artifices, and treacheries, which may ſucceſsfully be practiced during ſo familiar a reſidence in their territories; the food and oil they may afford; the probability of freſh ſprings for a ſupply of water at the bottom of the ſea; the facility of recovering ſub-marine treaſures whether loſt or naturally produced beneath the ocean; and laſt of all he adds, that

" All kinds of arts and manufactures may be exerciſed in this veſſel. The obſervations made by it may " be both written, and (if need were) printed here likewiſe. Several colonies may thus inhabit, having " their children born and bred up without the knowledge of land, *who could not chuſe but be amazed* " *with ſtrange conceits upon the diſcovery of this upper world.*"

rudder

rudder for steering.   An aperture, at the bottom, with its valve, was designed to admit water, for the purpose of descending; and two brass forcing pumps served to eject the water within, when necessary for ascending.   At the top, there was likewise an oar for ascending or descending, or continuing at any particular depth—A water gauge or barometer, determined the depth of descent, a compass directed the course, and a ventilator within, supplied the vessel with fresh air, when on the surface.

The entrance into the vessel was elliptical, and so small as barely to admit a person. This entrance was surrounded with a broad elliptical iron band, the lower edge of which was let into the wood of which the body of the vessel was made, in such a manner, as to give its utmost support to the body of the vessel against the pressure of the water.   Above the upper edge of this iron band, there was a brass crown, or cover, resembling a hat with its crown and brim, which shut water tight upon the iron band: the crown was hung to the iron band with hinges so as to turn over sidewise, when opened.   To make it perfectly secure when shut, it might be screwed down upon the band by the operator, or by a person without.

There were in the brass crown, three round doors, one directly in front, and one on each side, large enough to put the hand through—when open they admitted fresh air; their shutters were ground perfectly tight into their places with emery, hung with hinges and secured in their places when shut.   There were likewise several small glass windows in the crown, for looking through, and for admitting light in the day time, with covers to secure them.   There were two air pipes in the crown.   A ventilator within drew fresh air through one of the air pipes, and discharged it into the lower part of the vessel; the fresh air introduced by the ventilator, expelled the impure light air through the other air pipe.   Both air pipes were so constructed, that they shut themselves whenever the water rose near their tops, so that no water could enter through them, and opened themselves immediately after they rose above the water.

The vessel was chiefly ballasted with lead fixed to its bottom; when this was not sufficient, a quantity was placed within, more or less, according to the weight of the operator; its ballast made it so stiff, that there was no danger of oversetting.   The vessel with all its appendages, and the operator, was of sufficient weight to settle it very low in the water. About two hundred pounds of the lead, at the bottom, for ballast, could be let down forty or fifty feet below the vessel; this enabled the operator to rise instantly to the surface of the water, in case of accident.

When the operator would descend, he placed his foot upon the top of a brass valve, depressing it, by which he opened a large aperture in the bottom of the vessel, through which the water entered at his pleasure; when he had admitted a sufficient quantity, he descended very gradually; if he admitted too much, he ejected as much as was necessary to obtain an equilibrium, by the two brass forcing pumps, which were placed at each hand.   Whenever the vessel leaked, or he would ascend to the surface, he also made use of these forcing pumps.   When the skilful operator had obtained an equilibrium, he could row upward, or

<div align="right">downward</div>

downward, or continue at any particular depth, with an oar, placed near the top of the veffel, formed upon the principle of the fcrew, the axis of the oar entering the veffel ; by turning the oar one way he raifed the veffel, by turning it the other way he depreffed it.

A glafs tube eighteen inches long, and one inch in diameter, ftanding upright, its upper end clofed, and its lower end, which was open, fcrewed into a brafs pipe, through which the external water had a paffage into the glafs tube, ferved as a water-gauge or barometer. There was a piece of cork with phofphorus on it, put into the water-gauge. When the veffel defcended the water rofe in the water-gauge, condenfing the air within, and bearing the cork, with its phofphorus, on its furface. By the light of the phofphorus, the afcent of the water in the gauge was rendered vifible, and the depth of the veffel under water afcertained by a graduated line.

An oar, formed upon the principle of the fcrew, was fixed in the forepart of the veffel ; its axis entered the veffel, and being turned one way, rowed the veffel forward, but being turned the other way rowed it backward ; it was made to be turned by the hand or foot.

A rudder, hung to the hinder part of the veffel, commanded it with the greateft eafe. The rudder was made very elaftic, and might be ufed for rowing forward. Its tiller was within the veffel, at the operator's right hand, fixed, at a right angle, on an iron rod, which paffed through the fide of the veffel ; the rod had a crank on its outfide end, which commanded the rudder, by means of a rod extending from the end of the crank to a kind of tiller, fixed upon the left hand of the rudder. Raifing and depreffing the firft mentioned tiller turned the rudder as the cafe required.

A compafs marked with phofphorus directed the courfe, both above and under the water ; and a line and lead founded the depth when neceffary.

The internal fhape of the veffel, in every poffible fection of it, verged towards an ellipfis, as near as the defign would allow, but every horizontal fection, although elliptical, yet as near to a circle, as could be admitted. The body of the veffel was made exceedingly ftrong ; and to ftrengthen it as much as poffible, a firm piece of wood was framed, parallel to the conjugate diameter, to prevent the fides from yielding to the great preffure of the incumbent water, in a deep immerfion. This piece of wood was alfo a feat for the operator.

Every opening was well fecured. The pumps had two fets of valves. The aperture at the bottom, for admitting water, was covered with a plate, perforated full of holes to receive the water, and prevent any thing from choaking the paffage, or ftopping the valve from fhutting. The brafs valve might likewife be forced into its place with a fcrew, if neceffary. The air pipes had a kind of hollow fphere, fixed round the top of each, to fecure the air-pipe valves from injury : thefe hollow fpheres were perforated full of holes for the paffage of the air through the pipes: within the air-pipes were fhutters to fecure them, fhould any accident happen to the pipes, or the valves on their tops.

Wherever

Wherever the external apparatus paffed through the body of the veffel, the joints were round, and formed by brafs pipes, which were driven into the wood of the veffel, the holes through the pipes were very exactly made, and the iron rods, which paffed through them, were turned in a lathe to fit them ; the joints were alfo kept full of oil, to prevent ruft and leaking. Particular attention was given to bring every part, neceffary for performing the operations, both within and without the veffel, before the operator, and as conveniently as could be devifed ; fo that every thing might be found in the dark, except the water-gauge and the compafs, which were vifible by the light of the phofphorus, and nothing required the operator to turn to the right hand, or to the left, to perform any thing neceffary.

No. 2..

Defcription of a magazine and its appendages, defigned to be conveyed by the fub-marine veffel to the bottom of a fhip.

In the forepart of the brim of the crown of the fub-marine veffel, was a focket, and an iron tube, paffing through the focket ; the tube ftood upright, and could flide up and down in the focket, fix inches : at the top of the tube, was a wood fcrew (A) fixed by means of a rod, which paffed through the tube, and fcrewed the wood fcrew faft upon the top of the tube : by pufhing the wood fcrew up againft the bottom of a fhip, and turning it at the fame time, it would enter the planks ; driving would alfo anfwer the fame purpofe ; when the wood fcrew was firmly fixed, it could be caft off by unfcrewing the rod, which faftened it upon the top of the tube.

Behind the fub-marine veffel, was a place, above the rudder, for carrying a large powder magazine, this was made of two pieces of oak timber, large enough when hollowed out to contain one hundred and fifty pounds of powder, with the apparatus ufed in firing it, and was fecured in its place by a fcrew, turned by the operator. A ftrong piece of rope extended from the magazine to the wood fcrew (A) above mentioned, and was faftened to both. When the wood fcrew was fixed, and to be caft off from its tube, the magazine was to be caft off likewife by unfcrewing it, leaving it hanging to the wood fcrew ; it was lighter than the water, that it might rife up againft the object, to which the wood fcrew and itfelf were faftened.

Within the magazine was an apparatus, conftructed to run any propofed length of time, under twelve hours ; when it had run out its time, it unpinioned a ftrong lock refembling a gun lock, which gave fire to the powder. This apparatus was fo pinioned, that it could not poffibly move, till, by cafting off the magazine from the veffel, it was fet in motion.

The fkilful operator could fwim fo low on the furface of the water, as to approach very near a fhip, in the night, without fear of being difcovered, and might, if he chofe, approach the ftem or ftern above water, with very little danger. He could fink very quickly, keep at any depth he pleafed, and row a great diftance in any direction he defired, without coming to the furface, and when he rofe to the furface, he could foon obtain a frefh fupply of air, when, if neceffary, he might defcend again, and purfue his courfe.

No.

2

No. 3.

Experiments made to prove the Nature and Use of a Sub-marine Vessel.

The first experiment I made, was with about two ounces of gun-powder, which I exploded 4 feet under water, to prove to some of the first personages in Connecticut, that powder would take fire under water.

The second experiment was made with two pounds of powder, inclosed in a wooden bottle, and fixed under a hogshead, with a two inch oak plank between the hogshead and the powder; the hogshead was loaded with stones as deep as it could swim; a wooden pipe descending through the lower head of the hogshead, and through the plank, into the powder contained in the bottle, was primed with powder. A match put to the priming, exploded the powder, which produced a very great effect, rending the plank into pieces; demolishing the hogshead; and casting the stones and the ruins of the hogshead, with a body of water, many feet into the air, to the astonishment of the spectators. This experiment was likewise made for the satisfaction of the gentlemen above-mentioned.

I afterwards made many experiments of a similar nature, some of them with large quantities of powder; they produced very violent explosions, much more than sufficient for any purpose I had in view.

In the first essays with the sub-marine vessel, I took care to prove its strength to sustain the great pressure of the incumbent water, when sunk deep, before I trusted any person to descend much below the surface: and I never suffered any person to go under water, without having a strong piece of rigging made fast to it, until I found him well acquainted with the operations necessary for his safety. After that, I made him descend and continue at particular depths, without rising or sinking, row by the compass, approach a vessel, go under her, and fix the *wood-screw* mentioned in No. 2, and marked A, into her bottom, &c. until I thought him sufficiently expert to put my design into execution.

I found, agreeably to my expectations, that it required many trials to make a person of common ingenuity, a skilful operator: the first I employed, was very ingenious, and made himself master of the business, but was taken sick in the campaign of 1776, at New-York, before he had an opportunity to make use of his skill, and never recovered his health sufficiently, afterwards.

Experiments made with a Sub-marine Vessel.

After various attempts to find an operator to my wish, I sent one who appeared more expert than the rest, from New-York, to a 50 gun ship lying not far from Governor's Island. He went under the ship, and attempted to fix the wooden screw into her bottom, but struck, as he supposes, a bar of iron, which passes from the rudder hinge, and is spiked under the ship's quarter. Had he moved a few inches, which he might have done, without rowing, I have no doubt but he would have found wood where he might have

fixed the ſcrew ; or if the ſhip were ſheathed with copper, he might eaſily have pierced it : but not being well ſkilled in the management of the veſſel, in attempting to move to another place, he loſt the ſhip : after ſeeking her in vain, for ſome time, he rowed ſome diſtance, and roſe to the ſurface of the water, but found day light had advanced ſo far, that he durſt not renew the attempt. He ſays that he could eaſily have faſtened the magazine under the ſtem of the ſhip, above water, as he rowed up to the ſtern, and touched it before he deſcended. Had he faſtened it there, the exploſion of one hundred and fifty pounds of powder, (the quantity contained in the magazine), muſt have been fatal to the ſhip. In his return from the ſhip to New-York, he paſſed near Governor's Iſland, and thought he was diſcovered by the enemy, on the iſland ; being in haſte to avoid the danger he feared, he caſt off the magazine, as he imagined it retarded him in the ſwell, which was very conſiderable. After the magazine had been caſt off one hour, the time the internal apparatus was ſet to run, it blew up with great violence.

Afterwards, there were two attempts made in Hudſon's river, above the city, but they effected nothing. One of them was by the afore-mentioned perſon. In going towards the ſhip, he loſt ſight of her, and went a great diſtance beyond her : when he at length found her, the tide ran ſo ſtrong, that as he deſcended under water, for the ſhip's bottom—it ſwept him away. Soon after this, the enemy went up the river, and purſued the boat which had the ſub-marine veſſel on board—and ſunk it with their ſhot. Though I afterwards recovered the veſſel, I found it impoſſible, at that time, to proſecute the deſign any farther. I had been in a bad ſtate of health, from the beginning of my undertaking, and was now very unwell ; the ſituation of public affairs was ſuch, that I deſpaired of obtaining the public attention, and the aſſiſtance neceſſary. I was unable to ſupport myſelf, and the perſons I muſt have employed, had I proceeded. Beſides, I found it abſolutely neceſſary, that the operators ſhould acquire more ſkill in the management of the veſſel, before I could expect ſucceſs ; which would have taken up ſome time, and made no ſmall additional expence. I therefore gave over the purſuit for that time, and waited for a more favorable opportunity, which never arrived.

Other Experiments made with a Deſign to fire Shipping.

In the year 1777, I made an attempt from a whale-boat, againſt the Cerberus frigate, then lying at anchor between Connecticut river and New London, by drawing a machine againſt her ſide, by means of a line. The machine was loaded with powder, to be exploded by a gun-lock, which was to be unpinioned by an apparatus, to be turned by being brought along ſide of the frigate. This machine fell in with a ſchooner at anchor, aftern of the frigate, and concealed from my ſight. By ſome means or other, it was fired, and demoliſhed the ſchooner and three men—and blew the only one left alive, overboard, who was taken up very much hurt.

After this, I fixed ſeveral kegs, under water, charged with powder, to explode upon touching any thing, as they floated along with the tide : I ſet them afloat in the Delaware,

above the Engliſh ſhipping at Philadelphia, in December, 1777. I was unacquainted with the river, and obliged to depend upon a gentleman very imperfectly acquainted with that part of it, as I afterwards found. We went as near the ſhipping as he durſt venture; I believe the darkneſs of the night greatly deceived him, as it did me. We ſet them adrift, to fall with the ebb, upon the ſhipping. Had we been within ſixty rods, I believe they muſt have fallen in with them immediately, as I deſigned ; but as I afterwards found, they were ſet adrift much too far diſtant, and did not arrive, until after being detained ſome time by froſt, they advanced in the day time, in a diſperſed ſituation, and under great diſadvantages. One of them blew up a boat, with ſeveral perſons in it, who imprudently handled it too freely, and thus gave the Britiſh that alarm, which brought on *the battle of the kegs.*

The above veſſel, magazine, &c. were projected in the year 1771, but not completed, until the year 1775.

<div align="right">D. BUSHNELL.</div>

<div align="center">X.</div>

Queſtion reſpecting the Purification of Copper for alloying Gold. By X. Y.

<div align="center">To Mr. NICHOLSON.</div>

S I R,

<div align="right">*Newcaſtle-upon-Tyne, Feb.* 18, 1800.</div>

I BEG leave, through the medium of your valuable publication, to requeſt the favour of any of your correſpondents to inform me of the beſt method of obtaining copper in its utmoſt ſtate of purity, and fit for alloying gold without occaſioning that brittleneſs which always ariſes from uſing copper, even of the fineſt kind uſually obtained in commerce. The ductility of the gold may be reſtored by ſubſequent fuſions, but this occaſions conſiderable loſs and diſappointment to the workman, which do not happen when the copper is perfectly refined. I have looked into ſeveral chemical works, but without obtaining the ſatisfaction I want; nor would I be ſo ſolicitous about this information, could I, at any time, procure copper fit for this purpoſe.

<div align="center">I am, SIR,</div>

<div align="center">Your's, &c.</div>

<div align="right">X. Y.</div>

XI.

On the Art of Hat making. Supplementary Letter. By *N. L.*

To Mr. NICHOLSON.

SIR,

Newcastle, Feb. 14, 1800.

I SHALL think myself particularly obliged to you if you will allow me, through the medium of your Journal, to correct *a very great error in the arrangement* of the last paper of mine you printed, in your Number for May last: as it is *placed* at present, it may be productive of great mistakes, and as it will take up little room in your Journal, the inconvenience will not be great. The following is the proper arrangements, see parts of pages 74, 75, 76, vol. iii. The journeymen tell me, that the dregs are to hold or fill the body, whilst a little vitriol cleanses it of the dirt, &c. that may be on the rabbit or other wool; too much vitriol would make the whole that was weighed out to the journeymen work into the hats, but by the mutual action of the vitriol and the dregs, the quantity of the first being small, about a wine glassfull, the dirt and the strong hairs get purged out (the last from the shrinking in being flow, as well as their being straight; for was the lessening of the size at plank *rapid*, they would, in defiance of their straightness, get entangled, and even as it is, they are slightly so; but care is taken to get them out by rubbing the body of the hat well with the hand in a circular manner) whilst, at the same time, the dregs keep the hats plump. Another advantage attending the use of dregs, whether of beer, porter, or wine, is that as the boiling in the dregs does not draw out much of the mucilage from each hat, when they come to be stiffened the dregs form a body within the hat sufficiently strong or retentive to keep the glew from coming through amongst the nap: vitriol *alone* would purge or weaken the hats too much, consequently, half the quantity does better with the addition of dregs, and they disallow the body to be closer from its getting more work: many journeymen, however, to hurry this part of the process, use a quantity of vitriol, and open the body again by throwing in a handfull or two of oatmeal; by this means they get a great many made, though at the same time they are left quite grainy *from the want of labour*. This, in handling the dry grey hat, when made, may be in part discovered, but in part only; in wearing the effect is shining spots, as if of grease, but is, in reality, the glew lodging upon the grainy parts. This error in arrangement I had not discovered until I read the account of hat making, extracted from your Journal, in the supplement to the Encyclopedia Britannica published lately: I would first beg leave to add, that it was not *that* Encyclopedia alone that I meant to charge with

giving

giving *a wrong account of the bufinefs in queftion*, but all the others, as *Perthenfis*, *Londi-nenfis*. It becomes neceffary to mention this, from the manner in which the conductor of the fupplement to the Encyclopedia Britannica has introduced the fubject in queftion.

<div align="center">Believe me,</div>

<div align="center">With great refpect,</div>

<div align="center">Your's, &c.</div>

<div align="right">N. L.*</div>

<div align="center">XII.</div>

On the Inventions of ROBERT HOOKE *for regulating Time Pieces. By a Correfpondent.*

<div align="center">To Mr. NICHOLSON.</div>

S I R,

<div align="right">*London, July* 15, 1800.</div>

IN that fcarce publication of the famous Robert Hooke, entitled " A Defcription of Heliofcopes and fome other Inftruments," printed in quarto, for Robert Martyn, printer to the Royal Society, 1676, I obferve (befides the heliofcopes by repeated reflexions of the folar light, and the univerfal joint at prefent fo well known and efteemed) a poftfcript refpecting the regulation of time pieces by a fpring, of which I fend you an abftract, in hopes that fome of your ingenious readers will inform the world, whether any farther difclofure was ever made of the fubject there hinted at ; and alfo that fome of your cor-refpondents, who underftand the univerfal and real character of Bifhop Wilkins, will tranflate the communication of which I fend you a faithful copy. It feems to me as if this wonderful man had been well acquainted with all that has fince appeared as the particular difcoveries of individuals during the laft century.

The author firft vindicates his title to the invention of what is now called the pendulum fpring, contrived by him in 1658, and made the fubject of a treaty between himfelf and fome men of fortune, in 1660, which broke off upon their infifting that he fhould have no fhare in fuch improvements as might be afterwards made on his principles. They affured him that his invention would be difcovered by others in fix months, to which he anfwered, he would give the world feven years; and he remarks, that more than fourteen had elapfed without that effect taking place.

In 1664, at the earneft entreaty of a friend, the doctor read feveral of his firft Cutlerean lectures on the fubject of time pieces, and fhewed the ground and reafon of the applications of fprings to the balance of a watch for regulating its motion, as the fame time that he

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* I am forry that this letter having been miflaid, renders it neceffary to apologize to fo valuable a cor-refpondent.

<div align="right">explained</div>

explained above twenty feveral methods of application, and how the vibrations might be fo regulated as to make their durations either all equal, or the greater flower or quicker than the lefs, and that in any proportion affigned. Some of thefe ways were applicable to leffer vibrations; others to greater, as of 2, 3, 4, 6, or what number of revolutions were defired; the models of which he produced in the lecture.

As his poftfcript is directed in controverfy againft Mr. Oldenburg, the writer of the Philofophical Tranfactions, and in part againft M. Huyghens, who adopted the pendulum fpring, and has defcribed Hooke's circular pendulum to clocks in his book de Horologio Ofcillatorio, without mentioning the inventor: it contains fome matters of proof and animadverfion, into which I have no prefent intereft to enter; but conclude with quoting his own words refpecting the copy I fend you, begging leave to enter my humble proteft againft the injunction of fecrecy, which a fenfe of undeferved injury may, perhaps, have extorted from this great man (page 30.)

" I fhall conclude this tract with a fhort communication of the general ground of my
" invention for *pocket watches*, the number of particular ways being very great, which (that
" the true lovers of art, and they only may have the benefit of) I have fet down in the
" univerfal and real character of the late reverend prelate my honoured friend Dr. John
" Wilkins, lord bifhop of Chefter, deceafed. In which I could wifh all things of this
" nature were communicated, it being a character and language fo truly philofophical,
" and fo perfectly and thoroughly methodical, that there feemeth to be nothing wanting to
" make it have the utmoft perfection, and higheft idea of any character imaginable, as
" well for philofophical as for common and conftant ufe. And I have this further to
" defire of my reader, who will be at the pains to decipher and underftand this defcrip-
" tion, that he would only make ufe of it for his own information, and not communicate
" the explication thereof to any, that hath not had the fame curiofity with himfelf."
See Plate X.

" This I do not fo much to hinder the fpreading of this defcription here delivered, as
" to revive, and, if poffible, bring into ufe and practife that excellent defign: it being a
" character and language perfectly free from all manner of ambiguity, and yet the moft
" copious, expreffive, and fignificative, of any thing or notion imaginable, and which
" recommends it moft to common ufe, the moft eafy to be underftood and learnt in the
" world *."

I am,

SIR,

Your obliged reader,

M. M.

* For fome account of Bifhop Wilkins's book fee our Journal, II. 346.---N.

SCIENTIFIC PUBLICATIONS.

Philosophical Transactions of the Royal Society of London for the Year 1800. *Part I. Quarto.* 238 *pages, with a Meterological Journal, occupying* 26 *pages more, and* 8 *Plates.* Sold by Elmsly, London, price 15 Shillings.

THIS part contains the following papers: 1. The Croonian Lecture. On the Structure and Uses of the Menbrana Tympani of the Ear. By Everard Home, Esq. F.R.S. 2. On the Methods of determining from the real Probabilities of Life, the Nature of contingent Reversions, in which three Lines are involved in the Survivorship. By Wm. Morgan, Esq. F.R.S. 3. Abstract of a Register of the Barometer Thermometer and Rain at Lyndon, in Rutland, for the Year 1798. By Thomas Barker, Esq. 4. On the Power of penetrating into Space by Telescopes, with a comparative Determination of the Extent of that Power in natural Vision and in Telescopes of various Sizes and Constructions: illustrated by select Observations. By Wm. Herschell, LL.D. F.R.S. 5. A second Appendix to the improved Solution of a Problem in Physical Astronomy, inserted in the Philosophical Transactions for the Year 1798, containing some further Remarks, and improved Formulæ, for computing the Coefficients A and B, by which the Arithmetical work is considerably shortened and facilitated. By the Rev. John Hellins, B.D. F.R.S. and Vicar of Potters Perry, in Northamptonshire. 6. Account of a Peculiarity in the Distribution of the Arteries sent to the Limbs of slowly moving Animals: together with some other similar facts. By Anthony Carlisle, Esq. 7. Outlines of Experiments and Inquiries respecting Sound. By Thomas Young, M.D. F.R.S. 8. Observations on the Effects which take place from the Destruction of the Membrana Tympani of the Ear, by Mr. Astley Cooper. 9. Experiments and Observations on the Light which is spontaneously emitted, with some degree of permanency, from various Bodies. By Nathaniel Hume, M.D. F.R.S. and A.S. 10. Account of a Series of Experiments undertaken with a View of decomposing the Muriatic Acid. By Mr. William Henry (see page 209 of the present Number.) 11. On a new Fulminating Mercury. By Edward Howard, Esq. F.R S. (see Philos. Journal, page 173, of the present volume.) Appendix. Meteorological Journal kept at the Apartments of the Royal Society, by order of the President and Council.

Researches

Refearches, Chemical and Philofophical, chiefly concerning Nitrous Oxide, or .Dephlogifticated Nitrous Air, and its Refpiration. By Humphry Davy, Superintendant of the Medical Pneumatic Inftitution. 580 *Pages Octavo.* London, printed for J. Johnfon, and Biggs and Cottle, Briftol.

The Doctrine of Phlogifton eftablifhed, and that of the Compofition of Water refuted. By Jofeph Prieftley, L.L.D. F.R.S. &c. &c. 90 *Pages Octavo.* Northumberland, in America, printed for the Author, by A. Kennedy, 1800.

Tranfactions of the Royal Society of Edinburgh. Vol. V. Part I. for 1799. 116 *Pages, Quarto.*

This work contains, 1. Inveftigation of certain Theorems relating to the Figure of the Earth. By Mr. Playfair. 2. Account of certain Phenomena obferved in the Air Vault of the Furnaces of the Devon Iron Works : together with fome practical Remarks on the Management of Blaft Furnaces. By Mr. Roebuck (Philof. Jour. IV. 110.) 3. Experiments on Whinftone and Lava. By Sir James Hall, Bart. (Ph. Jour. IV. 56.) 4. A Chemical Analyfis of three Species of Whinftone, and two of Lava. By Dr. Robert Kennedy. 5. A new Method of refolving Cubic Equations. By James Ivory, Efq.
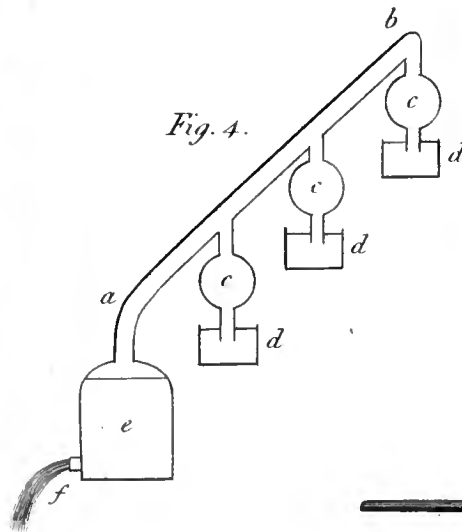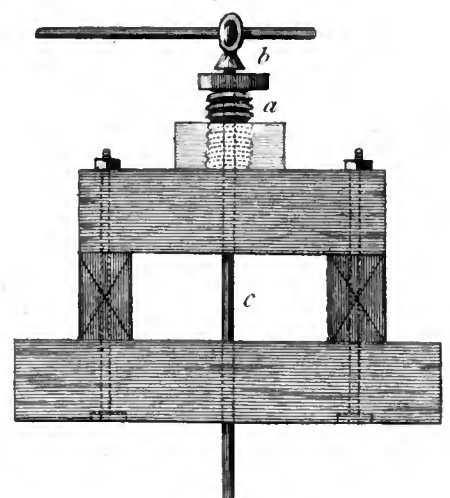
Fig. 1.
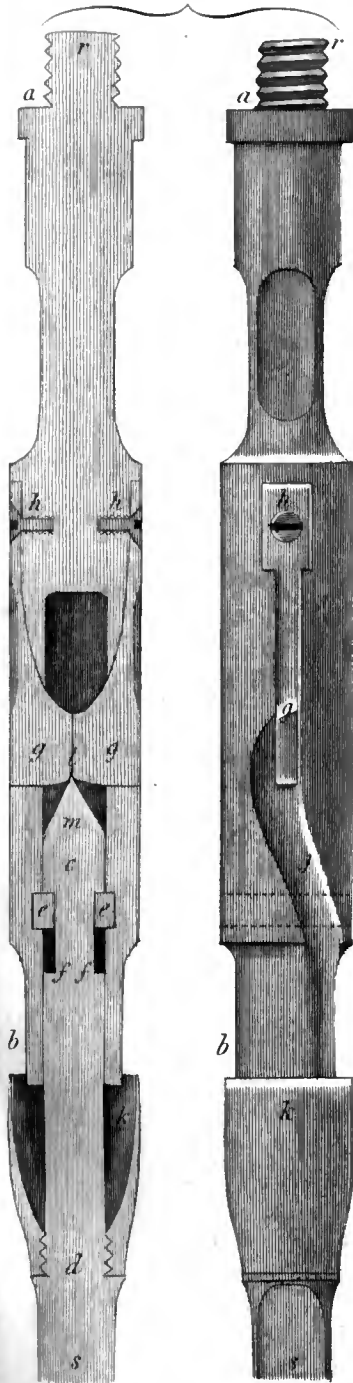
Fig. 2.

Fig. 4.

Fig. 3.

The general Principles of Dr Hooke's Invention of portable Time pieces.

Written by himself in the universal Character of Bishop Wilkins.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

ARTICLE I.

Experiments and Observations made with the newly discovered Metallic Pile of Sig. Volta. By Lieut. Col. Henry Haldane. With Remarks by W. N.

IMMEDIATELY after the publication of the procefs of Signor Volta in a morning paper*, Col. Haldane, whofe zeal in the profecution of philofophical operation is well known, conftructed an apparatus, and made a feries of experiments for the purpofe of analyfing the pile itfelf, as well as the nature of its effects, of which he had the goodnefs to favor me with an account. But as thefe inveftigations, though undoubtedly poffeffing the merit of originality, as far as relates to the fcientific acutenefs of their author, were in fome refpects the fame as others then unknown to him, but poffeffing priority in point of actual date, I poftponed the public notice of his communication, till it fhould have the advantage of his revifal, after perufing the contents of my laft paper. The fame motives at prefent lead me to give an account of the new facts, with a few remarks relative to the theory, of which we are much in want to direct our future operations.

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* Paragraph in the Morning Chronicle of May 30, 1800, giving an inaccurate account of the hiftory of the invention, and defcribing the general effects as exhibited by Dr. Garnet in the lecture at the Royal Inftitution.

Col. Haldane's apparatus confifted of forty half crowns, with an equal number of pieces of zinc, and 39 cards, which were wetted with pure water. They were placed horizontally on a table, and the experiment of decompofing water by two copper wires, was repeated and defcribed with minute accuracy.

He did not receive a fhock with wetted hands, nor fee any light when the tongue formed one of the extremes of the circuit; but his fon was ftruck with this laft appearance. Neither did his apparatus affect the electrometer of Bennet, nor my fpining inftrument. When a fewing needle was paffed beneath the fkin of a finger of one hand, and another needle inferted in like manner in the other hand, and thefe were made the extremities of the circuit, a fharp irritation was felt at the wounded parts, with a convulfive fenfation, extending to the fhoulders, and even the neck. But this did not refemble the electric fhock, being more unpleafant, and of longer duration.

The decompofition of water by filver, and by gold wires, was attended with phenomena fimilar to thofe defcribed in our former numbers. The gold wire fuffered decompofition.

In the examination of the pile, he found its power diminifhed by diminifhing the number of pieces; that it would not act if either of the three plates were omitted; that tinfoil inftead of filver acted tolerably well; and that leather was preferable to card.

When the apparatus was immerfed in water, its action was intirely fufpended; but upon taking it out and wiping the external furface, without feparating the parts, it acted as well as before. Hence I am difpofed to conclude, that the power of the Colonel's apparatus was confiderably impaired by his placing them horizontally, which muft have favored the efflux of water from the cards, and thence between the faces of the zinc and filver: whence his failure in the fhock and electric figns. For the upright pile is certainly much ftronger than a pile which has been immerfed in water and then wiped.

The apparatus was fufpended in the receiver of an air pump, which was then exhaufted of air, till the mercury in the gage ftood at half an inch. The decompofition of water in a tube with copper wires, did not proceed during this ftate, but was renewed when the air was let in.

This author has tried combinations of the various metals, and finds that zinc will act with gold, tin, lead, iron, and copper; that iron will act with the fame metals, as will alfo lead, though feebly. But no other combinations of thefe metals would anfwer, except that tin and gold afforded a very faint cloud in the water. When iron and filver were ufed, the oxidation took place at the wire connected with the iron, and the gas flowed from that connected with the filver; but the contrary happened when zinc and iron were ufed, which acted very powerfully, as in this cafe the iron connection afforded the gas. And he confiders it as worthy of remark, that the oxide of copper wire depofited in the tube was of a dufky coloured green, different from that produced by the apparatus of zinc and filver, as if fome parts of the metals compofing the apparatus had entered into the circulation, and affected the colour of the oxide. This obfervation is undoubtedly very curious, and deferves to be purfued; but I fhould be inclined to fuppofe, that the degree of oxidation

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in this procefs would vary with the intenfity of the leading agent, as well as thofe in which the fpeed of the effect is governed by heat, dilution, &c.

With an increafed pile placed in a vertical pofition, Col. H. obtained very weak figns of electricity. He connected the apparatus with the conductor of an electrical machine, and found the effect rather impeded than affifted by the common electric ftream. He placed the plate of Bennet's electrometer in the circuit without producing electric figns. He found that the galvanic apparatus placed between the outfide and infide of a jar prevented its charging, and that it is alfo capable of conducting the charge, though not rapidly: and on the whole, from the very minute exhibition of the attractive and repellent powers, while the caufticity, the fhock, and the oxidation, are fo very powerful, he cannot be perfuaded that electricity is the principal agent, though fome might be generated, or difengaged during the operation of the apparatus.

Before I attempt to give a numerical elucidation of the phenomena, I fhall take the liberty to remark, 1. that Col. Haldane's electric ftream through the apparatus might have been in the contrary direction to its own ftream; 2. that the current from an electrical machine may be incomparably lefs in quantity than that produced by the metals; 3. that the experiment with the cap of Bennet's electrometer was not arranged fo as to direct the fuppofed current through the leaves; 4. that the pile muft in any hypothefis prevent a jar from being charged higher than itfelf; 5. that Mr. Cruickfhank, of Woolwich, has charged a large jar, fo as to give a fhock, merely by placing the pile between its coatings; and, 6. laftly, whatever may be wanting in *intenfity* of electric force (upon which the figns depend) may be made up by *quantity*. Whether the quantity be, indeed, fufficient to account for what happens, muft be deduced from the facts. I am aware of the great difficulties which oppofe themfelves to our refearches into the laws by which the electric fluid is governed. All the experiments hitherto made, including a confiderable number which have employed much of my time, but are ftill too incomplete for publication, are too few and too limited to ferve as the grounds for computation, or to be extended from that intenfity which affords a fpark of more than a foot in length, to thofe minute variations of electrical power which are menfurable by fparks too fhort to become the object of our fenfes. In the following lines, therefore, when I confider the galvanic phenomena, it is fimply my wifh to fhew that they are not inconfiftent with deductions made from the prefent ftate of our knowledge, though the quantities, upon future and more ftrict examination, may turn out to be very different from what are here exhibited.

The apparatus of Volta may be compared with the common Leyden jar, if we extend by analogy the experiments of Cavendifh to all furfaces and intenfities. For fhocks nearly equal will be given when the quantities of coated furface (or electrical capacity) are directly as the fquares of the quantities of electricity, or inverfely as the fquares of the lengths of

the fpark *. But it is the character of electric fhocks which are felt to equal diftances from the extremities of the animal, that thofe which are produced by a fmall quantity of electricity at a high intenfity, produce a fudden and more tranfient fenfation than thofe which are produced by a large quantity of electricity at a low intenfity, and, therefore, probably moving more flowly.

From this laft circumftance I found it very difficult to take a fhock from one fquare foot of coated glafs which fhould much refemble the galvanic fhock. When the jar was charged to give an explofive fpark of one-twentieth of an inch, the fhock extended above the elbows not quite fo far as that from a pile of 100 half crowns; but the pain was much more fudden, fharp and tranfient. The denfe galvanic fhock feemed to fill the limbs, producing univerfal perfpiration, and leaving a difpofition to tremor and unfteadinefs in the parts through which it had paffed. When the explofion of the jar meafured only one-fortieth of an inch, the fhock ftill proved to be more unpleafant at the inftant than that of any of the piles I have tried, though it did not extend quite to the elbows, and may be confidered as equal in this refpect to the fet of which the electricity was tried by the condenfer, as defcribed at page 184 of our prefent volume. I fhall, therefore, take this as one term of comparifon. Another experiment was wanting to determine the length of the fpark anfwering to the intenfity at which the gold leaf of Bennet's electrometer ftrikes its coatings. For this purpofe I took two equal electrical jars, and charged one of them, fo as to give an explofive fpark one-thirtieth of an inch in length. In this fituation the charged jar was made to communicate with the other in the fame manner as jars in a battery at top and bottom. It was then removed; having, in fact, loft half its charge. The receiving jar was in the next place difcharged, and a connexion again made as before. This fecond procefs reduced the original charge to one-fourth. A third reiteration reduced it to one-eighth; and it may eafily be conceived that by continuing the geometrical feries, it was in my power to reduce my charge very much indeed. After feven touches the jar ftill caufed the leaves to diverge nearly to their greateft extent, and in fome repetitions of the experiment the leaves ftruck. The electricity in this cafe was reduced to $\frac{1}{128}$ part, and, confequently, would have afforded a fpark of $\frac{1}{128}$ of $\frac{1}{30}$, or about $\frac{1}{3000}$ of an inch †. We may, therefore, take it as a rule, that Bennet's electrometer, at the higheft ftate of electricity, had this intenfity.

But we are to compare this intenfity with another ftill lower. The filk face of the condenfer, ufed with the galvanic pile in the experiments referred to, is about one-fiftieth of an inch thick, and the leaves ftruck when the plate was raifed about half an inch; the diftance being thus increafed twenty-five times. The electric intenfity, or explofive fpark

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* The Hon. Hen. Cavendifh, Efq. found that four jars of the fame fize and thicknefs gave nearly the fame, or rather a greater, fhock than one of them charged with half the quantity of electricity. Philof. Tranf. LXVI. 196.

† The length of the fpark being as the quantity of electricity *ceteris paribus* in moderate intenfities.

of the condenfer, which was the fame as that of the pile itfelf, was, therefore, fo many times * lefs than that of the electrometer; that is to fay, its fpark was $\frac{1}{25} \times \frac{1}{3000} = \frac{1}{75000}$ of an inch.

Our jar of one foot furface, with its fpark of $\frac{1}{40}$ of an inch, having produced the fame fhock as the pile with this fpark of $\frac{1}{75000}$ of an inch, the furface of coated glafs of the fame thicknefs as the jar, and equivalent in capacity to the galvanic pile, muft be as the fquare of $\frac{1}{40}$ to the fquare of $\frac{1}{75000}$; that is as 1 to $3\frac{1}{2}$ millions nearly. Such, therefore, according to this method of computing, appears to be the capacity of the galvanic pile, fo prodigioufly exceeding the largeft batteries ever yet conftructed. But on the other hand, its intenfity being fo very low, the fhock may be confidered as if produced much more by the mafs or quantity of electricity, than by its velocity of motion. Now the quantities of electricity affording equal fhocks, according to the preceding conditions and doctrine, are inverfely as the intenfities, or in our cafe as 40 to 75000, or 1 to 1875, and our jar might have exploded about 10 times by each turn of a good electrical machine. The large battery of $3\frac{1}{2}$ millions of feet would, confequently, have required 187 turns to charge it to the low galvanic intenfity; and as the pile was found to charge itfelf in two feconds, which is very nearly the time required for each turn of a 24 inch (fingle) plate machine, we may affert that the production, or extrication, of electricity in a fmall apparatus of this kind is almoft two hundred times as rapid as that which can be obtained by friction by the labour of one man.

I cannot forbear adverting again to the novelty of the field of refearch in which I have thus ventured to fpeculate. We may reafonably hope that the difcoveries to which this new exhibition of the joint actions of chemiftry and electricity may lead us, will fhew other powers and energies of what is called the electrical fluid, and induce us to reject with gladnefs the imperfect theories afforded by our prefent knowledge of the fubject †.

II.

Account of a Series of Experiments, undertaken with the View of decompofing the Muriatic Acid. By Mr. WILLIAM HENRY.

(Concluded from page 214.)

A GREAT variety of fimilar experiments convinced me, that by electrifying together the carbonated hydrogenous and muriatic gafes, not the fmalleft progrefs was made towards the decompofition of the latter. All that was thus effected, confifted in the decompofition

* The charge at equal intenfities is inverfely as the thicknefs, as determined by Mr. Cavendifh in *loco citato*. In extreme cafes, however, I find that the charge follows a higher ratio.

† I have fince been favoured with another communication from Col. Haldane, which I am obliged to defer to our next.

of the water of the two gafes, by the carbon of the combuftible gas; and, when this was completely accomplifhed, no further effect enfued from continuing the electrization. The generation of carbonic acid was proved, by the following experiment.

Exper. 14. To a mixture of carbonated hydrogen and muriatic gafes, after having received above 100 fhocks, a drop of water was admitted, which abforbed the muriatic acid. The liquid was then taken up by blotting-paper; and the refiduary gas, being transferred into another tube, was brought into contact with a folution of pure barytic earth. The precipitation of this folution, evinced the prefence of carbonic acid.

It was defirable, however, that the effects fhould be afcertained, of electrifying together pure muriatic acid and pure carbonated hydrogenous gas, both perfectly free from water. Now, from the experiments related in the firft fection, it appears highly probable, that a complete purification from moifture is produced, in both gafes, by the action of the electric fluid; all the water they before contained being thus decompofed. In the following experiments, therefore, the two gafes were feparately electrified, before they were fubmitted to this procefs conjointly.

Exper. 15. To a portion of muriatic acid, diminifhed by the action of electricity from 144 to 121 meafures, 27 meafures of carbonated hydrogenous gas, expanded as far as poffible, were added, and 200 fhocks paffed through the mixture. The addition of permanent gas amounted to 14 meafures; 10 of which may be traced to the muriatic acid, and were evolved by its feparate electrization. The remaining 4 meafures, which remain to be accounted for, are too fmall a quantity to be afcribed to the decompofition of the acid.

Exper. 16. To a quantity of carbonated hydrogenous gas, which had received 400 fhocks, and occupied the fpace of 212 meafures, I added 232 of muriatic acid, through which 200 fhocks had previoufly been paffed. The electrization of the mixture was next continued, till 800 difcharges had taken place. On examining the mixture of gafes, during this operation, no change whatever took place; and, after its clofe, no more muriatic acid had difappeared, than would have been deficient after the firft electrization; nor was there any further production of permanent gas.

Exper. 17. The fame refult was obtained, by electrifying together 280 meafures of carbonated hydrogenous gas, previoufly expanded by 600 fhocks, and 114 of muriatic acid, after 400 fhocks. The additional difcharge, through this mixture, of 1000 fhocks, did not evince the fmalleft progrefs towards the decompofition of the muriatic acid.

Exper. 18. In the naturally moift ftate of thefe gafes, it follows, from the 14th experiment, that carbonic acid is produced by electrifying them in conjunction. It appeared to me of fome importance to afcertain whether, after a previous decompofition of their moifture, carbonic acid would continue to be generated. But the electrified carbonated hydrogenous gas itfelf contains carbonic acid, which, unlefs removed, would render the refult of the experiment undecifive. This was accomplifhed by paffing up, to a portion of electrified gas, a bubble or two of dry ammoniacal gas, which, uniting with the carbonic

acid,

acid, would condenfe any portion of it that might be prefent. The remainder was transferred into another tube; and, to this carbonated hydrogenous gas, perfectly deprived both of moisture and carbonic acid, muriatic acid gas, previously electrified, was added, and electrical shocks were paffed through the mixture. A drop of water was then admitted; and the refiduary gas, after having been dried, was transferred into another tube. On paffing up barytic water, not the fmalleft trace of carbonic acid could be difcovered.

From the preceding experiments, the following conclufions may be deduced.

1. The muriatic acid gas, in the drieft ftate in which it can be procured, ftill contains a portion of water. From a calculation founded on the experiments defcribed in the firft fection, the grounds of which are too obvious to require being ftated, it follows, that 100 cubical inches of muriatic gas, after expofure to muriate of lime, ftill hold in combination 1.4 grain of water.

2. When electrical shocks are paffed through this gas, the watery portion is decompofed. The hydrogen of the water, uniting with the electric matter, conftitutes hydrogenous gas, and the oxygen unites with the muriatic acid; which laft, acting on the mercury, compofes muriate of mercury.

3. The electric fluid ferves as an intermedium, in combining oxygen with muriatic acid.

4. The really acid portion of muriatic gas does not fuftain any decompofition by the action of electricity.

5. When electric shocks are paffed through a mixture of carbonated hydrogen and muriatic acid gafes, the water held in folution by thefe gafes, is decompofed by the carbon of the compound inflammable gas; and carbonic acid and hydrogenous gafes are the réfult.

6. When all the water of the two gafes has been decompofed, no effect enfues from continuing the electrization; or, if the water of each gas has been previoufly deftroyed by electrifying them feparately, no further effect enfues from electrifying them conjointly.

7. Since therefore carbon, though placed under the moft favourable circumftances for abftracting from the muriatic acid, and combining with, its oxygen, evinces no such tendency, it may be inferred, that if the muriatic acid be an oxygenated fubftance, its radical has a ftronger affinity for oxygen than charcoal poffeffes.

Though the firft impreffions excited in my mind by the total failure of the above experiments, in accomplifhing one of the greateft objects of modern chemiftry, have induced me for fome time to withhold them from the fociety, I am fatisfied by reflection, that this communication is not without expediency. The means employed in attempting the analyfis of the muriatic acid, were fuch as, after mature deliberation, appeared to me moft to promife fuccefs; and the experiments were attended with a degree of labour, which can only be eftimated by thofe who have been engaged in fimilar purfuits; not one third of thofe which were really made having been defcribed, in the foregoing account of them.

them. It may fpare therefore to others, a fruitlefs application of time and trouble, to be made acquainted with what I have done; and the collateral facts, which have prefented themfelves in the inquiry, are perhaps not without curiofity or value.

From the refult of thefe experiments, I apprehend, all hope muft be relinquifhed, of effecting the decompofition of the muriatic acid, in the way of fingle elective affinity. They furnifh alfo a ftrong probability, that the bafis of the muriatic acid is fome unknown body; for, no combuftible fubftance with which we are acquainted, can retain oxygen, when fubmitted, in contact with charcoal, to the action of electricity, or of a high temperature. The analyfis of this acid muft, in future, be attempted with the aid of complicated affinities. Thus, in the mafterly experiment of Mr. Tennant, phofphorus, which attracts oxygen lefs ftrongly than charcoal, by the intermediation of lime decompofes the carbonic acid. Yet, led by the analogy of this fact, its difcoverer found that a fimilar artifice did not fucceed in decompofing the muriatic acid. " As vital air," he obferves, " is at-
" tracted by a compound of phofphorus and calcareous earth, more powerfully than by
" charcoal, I was defirous of trying their efficacy upon thofe acids which may from
" analogy be fuppofed to contain vital air, but which are not affected by the application of
" charcoal. With this intention, I made phofphorus pafs through a compound of marine
" acid and calcareous earth, and alfo of fluor acid and calcareous earth, but without pro-
" ducing in either of them any alteration. Since the ftrong attraction which thefe acids
" have for calcareous earth tends to prevent their decompofition, it might be thought,
" that in this manner they were not more difpofed to part with vital air than by the attrac-
" tion of charcoal: but this, however, does not appear to be the fact. I have found, that
" phofphorus cannot be obtained by paffing marine acid through a compound of bones
" and charcoal when red-hot. The attraction, therefore, of phofphorus and lime for vital
" air, exceeds the attraction of charcoal, by a greater force than that arifing from the
" attraction of marine acid for lime *."

By means fimilar to thofe employed in attempting the analyfis of the muriatic acid, I tried to effect that of the fluoric acid. When electrified alone, in a glafs tube coated internally with wax, it fuftained a diminution of bulk, and there remained a portion of hydrogenous gas. But, neither in this mode, nor by fubmitting it, mixed with carbonated hydrogenous gas, to the action of electricity, was any progrefs made towards its analyfis. Thefe experiments, however, render it probable, that the fluoric acid, like the muriatic, is fufceptible of ftill farther oxygenation, in which ftate it becomes capable of acting on mercury. The carbonic acid, on the contrary, appears not to admit of two different degrees of oxygenation. When the electric fhock has been repeatedly paffed through a portion of this acid gas, its bulk is enlarged, and a permanent

* Phil. Tranf. vol. LXXI. p. 184.

gas is produced, which is evidently a mixture of oxygenous and hydrogenous gafes; for, when an electrical fpark is paffed through the gas that remains after the abforption of the carbonic acid by cauftic alkali, it immediately explodes. Thefe refults even take place on electrifying carbonic acid from marble, previoufly calcined in a low red heat, to expel its water, and then diftilled in an earthen retort *.

III.

On a New Fulminating Mercury. By Edward Howard, *Efq.* F. R. S.

(Concluded from page 209.)

SECTION XVII.

I WILL now conclude, by obferving, that the fulminating mercury feems to be characterifed by the following properties:

It takes fire at the temperature of 368 Fahrenheit; explodes by friction †, by flint and fteel, and by being thrown into concentrate fulphuric acid. It is equally inflammable under the exhaufted receiver of an air-pump, as furrounded by atmofpheric air; and it detonates loudly, both by the blow of a hammer, and by a ftrong electrical fhock.

Notwithftanding the compofition of fulminating filver, and of fulminating gold, differ effentially from that of fulminating mercury, all three have fome fimilar qualities. In tremendous effects, filver undoubtedly ftands firft, and gold perhaps the laft. The effects of the mercurial powder and of gunpowder, admit of little comparifon. The one exerts, within certain limits, an almoft inconceivable force: its agents feem to be gas and caloric, very fuddenly fet at liberty, and both mercury and water thrown into vapour. The other difplays a more extended but inferior power: gas and caloric are, comparatively fpeaking, liberated by degrees: and water, according to Count Rumford, is thrown into vapour †.

Hence it feems, that the fulminating mercury, from the limitation of its fphere of action, can feldom if ever be applied to mining; and, from the immenfity of its initial

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* Meffieurs Landriani and Van Marum (*Annales de Chimie*, tom. ii. p. 270.) obtained only hydrogenous gas, by electrifying the carbonic acid gas. But the conductor of their apparatus was an iron one; which metal would combine with the oxygen of the water, and prevent it from appearing in a gafeous ftate. In my experiments, the conductors were of platina.

† Confequently it fhould not be inclofed in a bottle with a glafs ftopper.

‡ See Philofophical Tranfactions, for the year 1797, p. 222.

The hard black fubftance mentioned by the Count, as remaining after the combuftion of gunpowder, muft, I believe, have been an alkaline fulphuret, mixed chiefly with fulphite and carbonate of pot-afh. The conjecture that it is white when firft formed, is certainly juft, as my experiment with the glafs globe evinced.

force, cannot be used in fire-arms, unless in cases where it becomes an object to destroy them; perhaps, where it is the practice to spike cannon, it may be of service, because, I apprehend, it may be used in such a manner as to burst cannon, without dispersing any splinters.

The inflammation of fulminating mercury by concussion, offers nothing more novel or remarkable, than the inflammation, by concussion, of many other substances. The theory of such inflammations has been long since exposed by the celebrated Mr. Berthollet, and confirmed by Messieurs Fourcroy and Vauquelin: yet, I must confess, I am at a loss to understand, why a small quantity of mercurial powder made to detonate by the hammer, or the electric shock, should produce a report so much louder than when it is inflamed by a match, or by flint and steel. It might at first be imagined, that the loudness of the report could be accounted for, by supposing the instant of the inflammation, and that of the powder's confinement between the hammer and anvil, to be precisely the same; but, when the electrical shock is sent through or over a few grains of the powder, merely laid on ivory, and a loud report is the consequence, I can form no idea of what causes such a report.

The operation by which the powder is prepared, is perhaps one of the most beautiful and surprising in chemistry; and it is not a little interesting to consider the affinities which are brought into play. The super-abundant nitrous acid of the mercurial solution, must first act on the alcohol, and generate ether, nitrous etherized gas, and oxalic acid. The mercury unites to the two last in their nascent state, and relinquishes fresh nitrous acid, to act upon any unaltered alcohol. The oxalic acid, although a predisposing affinity seems exerted in favour of its quantity, is evidently not formed fast enough to retain all the mercury: otherwise, no white fumes, during a considerable period of the operation, but fulminating mercury alone, would be produced.

Should any doubt still be entertained of the existence of the affinities which have been called predisposing or conspiring, a proof that such affinities really exist, will I think be afforded, by comparing the quantity of oxalic acid which can be generated from given measures of nitrous acid and alcohol, with the intervention of mercury, and the intervention of other metals. For instance, when two measured ounces of alcohol are treated with a solution of 100 grains of nickel in a measured ounce and a half of nitrous acid, little or no precipitate is produced; yet, by the addition of oxalic acid to the residuary liquor, a quantity of oxalate of nickel, after some repose, is deposited. Copper affords another illustration: 100 grains of copper, dissolved in a measured ounce and an half of nitrous acid, and treated with alcohol, yielded me about 18 grains only of oxalate; although cupreous oxalate was plentifully generated, by dropping oxalic acid into the residuary liquor. About 21 grains of pure oxalic acid seem to be produced, from the same materials, when 100 grains of mercury are interposed. (See Section 14.) Besides, according to the Dutch paper, more than once referred to, acetous acid is the principal residue after the preparation of nitrous ether. How can we explain the formation of a

greater

greater quantity of oxalic acid, from the fame materials, with the intervention of 100 grains of mercury, than with the intervention of 100 grains of copper, otherwise than by the notion of confpiring affinities, fo analogous to what we fee in other phænomena of nature?

I have attempted, without fuccefs, to communicate fulminating properties, by means of alcohol, to gold, platina, antimony, tin, copper, iron, lead, zinc, nickel, bifmuth, cobalt, arfenic, and manganefe; but I have not yet fufficiently varied my experiments, to enable me to fpeak with abfolute certainty. Silver, when 20 grains of it were treated with nearly the fame proportions of nitrous acid and alcohol as 100 grains of mercury, yielded, at the end of the operation, about 3 grains of a grey precipitate, which fulminated with extreme violence. Mr. Cruickfhank had the goodnefs to repeat the experiment: he diffolved 40 grains of filver in 2 ounces of the ftrongeft nitrous acid diluted with an equal quantity of water, and obtained (by means of 2 ounces of alcohol) 60 grains of a very white powder, which fulminated like the grey precipitate above defcribed. It probably combines with the fame principles as the mercury, and of courfe differs from Mr. Berthollet's fulminating filver, alluded to in page 250. I obferve, that a white precipitate is always produced in the firft inftance, and that it may be preferved, by adding water, as foon as it is formed; otherwife, when the mother liquor is abundant, it often becomes grey, and is re-diffolved.

P. S. Since the preceding pages were written, I have been permitted, by the Right Honourable Lord Howe, Lieutenant General of the Ordnance, to make the following trials of the mercurial powder, at Woolwich, in conjunction with Colonel Blomefield, and Mr. Cruickfhank *.

Experiment 1. From the manner in which the fcrew of the gun-breech, mentioned in Section v. had acted on the barrel, it was imagined, that by burfting an iron cafe, exactly fitted to the bore of a cannon, its fudden enlargement might make many flaws, and fplit the piece, without difperfing any fplinters. In conformity to this opinion, a caft iron cafe was conftructed, with a cylindrical chamber, of equal length and diameter, calculated to hold $3\frac{1}{4}$ ounces troy of the mercurial powder. The cafe, being firmly fcrewed together, was charged through its vent-hole, and introduced into a twelve-pounder carronade, the bore of which it exactly fitted. The powder was then enflamed, with proper precautions. The gun remained entire, but the cafe divided: the portion forming the upper furface of the chamber, was expelled in one mafs; that adjoining the breech, which conftituted the reft of the chamber, was cracked in every direction, and in part crumbled; yet it was fo wedged into fome indentations which the explofion had made in the fides of the piece, that the fragments were not removed without great labour.

Experiment 2. Another caft iron cafe was prepared, of the fame fize as the former, with a chamber alfo cylindrical, but wrought in a tranfverfe direction, and of a greater length

* It is with pleafure I take this opportunity of acknowledging the civil attention I received from the different officers.

than

than diameter; the thickness of metal at each extremity not being more than a quarter of an inch. This case was filled with nearly 5 ounces troy of the mercurial powder, and placed in the same carronade. Three twelve-pound shot were next introduced, and brought into close contact with the upper surface of the case, as well as with each other. The gun a second time withstood the explosion: the case was divided across the middle of the chamber, into two equal parts; that adjoining the breech was, as in the former experiment, much flawed, and left immoveable; that nearest to the muzzle was also much flawed, but driven out with the shot. All the three shot were broken; the two lower being divided into several pieces, and the upper one cracked through the centre.

The report was so feeble, in both experiments, that an inattentive person, I am confident, would not have heard it at the distance of two hundred yards.

Experiment 3. It was found so difficult to extract the fragments of the case remaining in the carronade, after the last experiment, that a channel was drilled through them, to the vent-hole of the piece. It was then charged with 6 ounces troy of the mercurial powder, made up as a cartridge, which did not occupy above one half of the diameter of the bore. A wad was placed over the powder, dry sand superadded, to fill all vacuities, and the gun filled to the muzzle with two twelve-pound shot. A block of wood was set at a small distance, to receive the impression of the shot, and the powder was inflamed as usual. The carronade still resisted. One of the shot was split into two pieces; and the block of wood was driven to a considerable distance, but not penetrated by the shot above the depth of one inch. The report was somewhat louder than the former ones. In all three instances, a considerable recoil evidently took place. I presume, therefore, that in the first experiment related in the fifth Section, there must have been a recoil, though too trifling to be observed; and, in the instances where the gun and the proof were burst, it was not so much to be expected.

Experiment 4. Finding that the carronade, from the great comparative size of its bore to that of its length, required a larger quantity of mercurial powder to burst it than we were provided with, we charged a half-pounder swivel with an ounce and a half avoirdupois of the mercurial powder, (the service charge of gunpowder being 3 ounces) and a half-pound shot between two wads. The piece was destroyed from the trunnions to the breech, and its fragments thrown thirty or forty yards. The ball penetrated five inches into a block of wood, standing at about a yard from the muzzle of the gun; the part of the swivel not broken, was scarce, if at all, moved from its original position.

Experiment 5. One ounce avoirdupois of the mercurial powder, enclosed in paper, was placed in the centre of a shell 4,4 inches in diameter, and the vacant space filled with dry sand.

The shell burst by the explosion of the powder, and the fragments were thrown to a considerable distance. The charge of gunpowder employed to burst shells of this diameter, is 5 ounces avoirdupois.

Experiment

Experiment 6. A fea grenade, 3.5 inches diameter, charged like the fhell in the laft experiment, was burft into numerous fragments, by ¼ of an ounce avoirdupois of the mercurial powder. The fragments were projected with but little force, and only to the diftance of eight or ten yards. The charge of gunpowder required for grenades of this fize, is 3 ounces.

Experiment 7. A fea grenade, of the fame diameter as the laft mentioned, and charged in the like manner, with ⅛ of an ounce avoirdupois, or 57½ grains, of the mercurial powder, was fplit into two equal pieces, which were not thrown ten inches afunder.

The report in the four laft experiments was very fharp, but not loud in proportion.

It feems, from the manner in which the fwivel was burft, in the fourth experiment, that a fmaller charge would have been fufficient for the purpofe. We may therefore infer, both from this inftance and from the fecond experiment made with the gun, in Section v, that any piece of ordnance might be deftroyed, by employing a quantity of the mercurial powder equal in weight to one half of the fervice charge of gunpowder; and, from the feventh and laft experiment, we may alfo conclude, that it would be poffible fo to proportion the charge of mercurial powder to the fize of different cannons, as ro burft them without difperfing any fplinters. But the great danger attending the ufe of fulminating mercury, on account of the facility with which it explodes, will probably prevent its being employed for that purpofe.

In addition to the other fingular properties of the fulminating mercury, it may be obferved, that two ounces inflamed in the open air, feem to produce a report much louder than when the fame quantity is exploded in a gun capable of refifting its action. Mr. Cruickfhank, who made fome of the powder, by my procefs, remarked that it would not inflame gunpowder. In confequence of which, we fpread a mixture of coarfe and fine grained gunpowder upon a parcel of the mercurial powder; and, after the inflammation of the latter, we collected moft, if not all, of the grains of gunpowder. Can this extraordinary fact be explained by the rapidity of the combuftion of fulminating mercury? or is it to be fuppofed, (as gunpowder will not explode at the temperature at which mercury is thrown into vapour,) that fufficient caloric is not extricated during this combuftion.

From the late opportunity I have had of converfing with Mr. Cruickfhank, I find that he has made many accurate experiments on gunpowder; and he has permitted me to ftate, " that the matter which remains after the explofion of gunpowder, confifts of pot-afh " united with a fmall proportion of carbonic acid, fulphate of pot-afh, a very fmall quan- " tity of fulphuret of pot-afh, and unconfumed charcoal. That 100 grains of good gun- " powder yield about 53 grains of this refiduum, of which three are charcoal. That it is " extremely deliquefcent, and, when expofed to the air, foon abforbs moifture fufficient " to diffolve a part of the alkali; in confequence of which, the charcoal becomes expofed, " and the whole affumes a black or very dark colour." Mr. Cruickfhank likewife informs me, that after the combuftion of good gunpowder under mercury, no water is ever perceptible.

3.

A, (Pl. VII. of the prefent volume) a ball or globe of glafs, nearly half an inch thick, and feven inches in diameter. It has two necks, on which are cemented the brafs caps, B, C, each being perforated with a female fcrew, to receive the male ones D, E: through the former a fmall hole is drilled; the latter is furnifhed with a perforated ftud or fhank G. By means of a leather collar H, the neck C can be air-tightly clofed. When a portion of the powder is to be exploded, it muft be placed on a piece of paper, and a fmall wire laid acrofs the paper, through the midft of the powder: the paper being then clofed, is to be tied at each end to the wire, with a filken thread, as fhewn at I. One end of this wire is to be faftened to the end of the fhank G, and the fcrew D inferted to half its length into the brafs cap B; the other end of the wire, *a*, by means of the needle K, is to be drawn through the hole F. The fcrew E being now fixed in its place, and the wire drawn tight, it is to be fecured, by pufhing the irregular wooden plug L into the aperture of the fcrew D, taking care to leave a paffage for air. The ftop-cock M, the fection of which is fhewn at N, is now to be fcrewed on to the part D, which is made air-tight by the leather collar *b*. The glafs tube O is bent, that it may more conveniently be introduced under the receiver of a pneumatic apparatus. P, fhews the manner of connecting the glafs tube with the ftop-cock.

IV.

Additional Remarks on Galvanic Electricity.   By Mr. W. CRUICKSHANK, Woolwich.
Communicated by the Author.

BEING defirous to afcertain with fome degree of precifion, the nature and relative proportions of the gafes obtained from water and other fluids by this influence, I procured fome wires of gold, and alfo fome of filver gilt. On trial, I found that thefe wires were not by any means fo much acted upon as filver, at leaft where water alone was decompofed; I likewife obferved, that the quantity of oxigen in the mixed gas was much greater, and amounted to nearly one third of the whole; the gas from the zinc wire was alfo more copious, than when filver or copper wires were employed.

I took a wide mouthed phial capable of containing 3 ounces, and filled it with very pure lime water, a cork was then loofely introduced into its mouth, and two gold wires paffed through it; the phial being inverted in a fmall bafon containing pure water, the other extremities of the wires were connected with thofe of the pile in the ufual way; a very copious production of gas immediately took place, more efpecially from the filver wire, and in about four hours the phial was completely filled. This gas was fubmitted to the following experiments:

One meafure of it being mixed with two of nitrous gas, a diminution of one meafure took place, the refiduum contained nitrous gas, mixed with hydrogen.

Four

Four meafures of this gas were next exploded by the electric fpark in a ftrong glafs jar over mercury, and the whole difappeared, except about $\frac{1}{15}$ of a meafure; this was not inflammable, and appeared to be azote.

From thefe experiments it would feem, that this compound gas obtained from water by means of gold wires, confifts of nearly two parts hydrogen and one oxygen, mixed with a little azote, being nearly the proportions eftimating by bulk, which are faid to enter into the compofition of water.

It has been fuppofed, although not proved by Mr. Nicholfon, that the gas which efcapes from the wire connected with the filver extremity of the pile is hydrogen, whilft that difengaged by the one connected with the zinc is oxygen gas.

In order to fatisfy myfelf about the nature and proportions of thefe gafes, I took a glafs tube 10 inches in length, and by means of the blow pipe bent it in the middle, until the legs formed an acute angle refembling the letter V; while the glafs was red hot, I contrived to blow an opening at the angle about $\frac{1}{10}$ of an inch, or a little better in diameter. Two gold wires paffed through corks fecured by cement, were introduced into the legs, and brought within an inch of each other at the bend. The tube was then filled with diftilled water, and a finger being placed on the opening at the angle, to prevent the fluid from efcaping, it was placed in a tea-cup containing water, with the angle downwards, the legs having an inclination of about 45 degrees. The extremities of the wires being then brought into contact with thofe of the pile, a quantity of gas was difengaged from both, but by far the greateft from that connected with the filver; by this contrivance the gafes from the two wires were obtained perfectly diftinct, each gas afcending in the leg of the tube which contained its generating wire. When a fufficient quantity of the two aeriform fluids had been thus obtained, they were examined as follows:

One meafure of the gas from the filver fide, was mixed with one of nitrous gas, fome red fumes were obferved, and a diminution which amounted to $\frac{1}{3}$ of a meafure took place; the refiduum confifted of a mixture of nitrous and hydrogen gas.

Two meafures of this gas mixed with one of pure oxygen, being next introduced into a ftrong glafs jar over mercury, and fired by the electric fpark, the whole very nearly difappeared, not more than $\frac{1}{5}$ of a meafure remaining; this refiduary gas appeared from the nitrous teft to be chiefly oxygen. A denfe white vapour was perceived over the mercury for fome time after the explofion.

One meafure of the gas from the zinc fide being mixed with two of very pure nitrous gas, the whole very nearly difappeared, and another meafure of nitrous gas being added, the total diminution amounted to nearly three meafures. From thefe experiments it would appear, that the gas obtained from the filver wire was chiefly hydrogen gas, and that from the zinc wire, nearly pure oxygen.

Having been favoured by Mr. Nicholfon with fmall pieces of platina wires, I contrived to faften them to filver ones, and thefe laft being covered with a compofition of refin and bees wax, the platina alone was expofed to the action of the water, &c. By means of

thefe

thefe platina points, I obtained gafes very nearly fimilar to thofe juft defcribed: but the oxygen was not quite fo pure, as it often contains $\frac{1}{3}$ or $\frac{1}{4}$ of azote; and when the mixed gafes were exploded, as in the firft experiment with the gold wires, there was a refiduum amounting to $\frac{1}{7}$ of the whole, which was found to be a mixture of hydrogenous and azotic gafes. It may be proper to obferve, that in all thofe cafes where the gafes were obtained feparately, the volume of gas from the filver fide was nearly three times that from the zinc fide; and that the former always contained a little oxygen, amounting to about $\frac{1}{8}$ or $\frac{1}{10}$ of the whole *.

The platina point, connected with the zinc as well as the gold wire on the fame fide, were after fome time evidently tarnifhed; and this effect was foon obferved when the machine was in full action.

In the following experiment I obtained a perfect folution of gold:

After having precipitated magnefia and argill from their folutions in acids, by the influence of the wire from the filver fide, I was defirous to fee what effect would be produced on folutions of lime. For this purpofe the glafs tube was filled with a folution of the muriate of lime which had been cryftallifed, and the gold wires applied in the ufual manner. When the tube was placed in the circle of communication, little or no gas efcaped from the filver wire, but a confiderable quantity began to afcend immediately from the zinc one, and the fluid furrounding it affumed a fine yellow colour, which was found to proceed from a folution of the gold, the wire at the end of the procefs being much corroded. After fome time gas was likewife difengaged from the filver wire, but there was not the leaft precipitation of lime; when the wires were removed the fluid fmelled of aqua regia, or the ox. muriatic acid.

When platina points were employed inftead of the gold wires, the fmell of the nitro-muriatic acid was alfo foon perceived, but no fenfible folution of the platina was obfervable.

This formation of the nitro-muriatic, or oxygenated muriatic acids, was not obferved but where the perfect metals gold and platina were employed; the reafon of which muft be obvious. When the tube was filled with a folution of common falt inftead of the muriate of lime, a nitro-muriatic acid was likewife produced. The effects produced by the gold or platina wires on the tinctures of Litmus and Brazil wood, were ftill more remarkable than thofe defcribed in the former paper, more particularly on the litmus, which was very quickly reddened. When the tube was filled with diftilled water only, and gold wires employed; after the influence had paffed through for fome time (without confining the gas) that portion of the fluid in contact with the filver wire being decanted, ftrongly reddened the tincture of Brazil wood. This mode of making the experiment was preferred, as it might be fuppofed that the hydrogen in its nafcent ftate would unite with the colouring matter of the Brazil wood, and produce the effect of an alkali.

* Was this oxygen originally held in folution by the water? C.

From

From the experiments related in this and the former paper, we may draw the following conclusions :

1. That hydrogen gas mixed with a very small proportion of oxygen and ammonia, is somehow disengaged at the wire connected with the silver extremity of the machine; and that this effect is equally produced, whatever the nature of the metallic wire may be, provided the fluid operated upon be pure water.

2. That where metallic solutions are employed instead of water, the same wire which separates the hydrogen revives the metallic calx, and deposits it at the extremity of the wire in its pure metallic state ; in this case no hydrogen gas is disengaged. The wire employed for this purpose may be of any metal.

3. That of the earthy solutions, those of magnesia and argill only are decomposed by the silver wire, a circumstance which strongly favours the production of ammonia.

4. That when the wire connected with the zinc extremity of the pile consists either of gold or platina, a quantity of oxygen gas, mixed with a little azote and nitrous acid is disengaged, and the quantity of gas thus obtained is a little better than $\frac{1}{3}$ of the hydrogen gas separated by the silver wire at the same time.

5. That when the wire connected with the zinc is silver, or any of the imperfect metals, a small portion of oxygenous gas is likewise given out, but the wire itself is either oxydated or dissolved, or partly oxydated and partly dissolved ; indeed, the effect in this case produced upon the metal, is very similar to that of the concentrated nitrous acid, where a great deal of the metal is oxydated, and but a small quantity held in solution *.

6. That when the gases obtained by gold or platina wires, are collected together and exploded over mercury, the whole nearly disappears and forms water, with probably a little nitrous acid, for there was always a thick white vapour perceived for some time after the explosion. The residuary gas in this case appeared to be azote.

In reflecting on these experiments it would appear, that in some of them, the water must be decomposed; but how this can be effected, is by no means so easily explained. For example, it seems extremely mysterious how the oxygen should pass silently from the extremity of the silver wire to that of the zinc wire, and there make its appearance in the form of gas. It is to be observed likewise, that this effect takes place which ever way the wires are placed, and whatever bends may be interposed between their extremities, provided the distance be not too great. On considering these facts more minutely, it appeared to me, that the easiest and simplest mode of explanation, would be, to suppose that the galvanic influence (whatever it may be) is capable of existing in two states, that is, in an oxygenated and deoxygenated state. That when it passes from metals to fluids containing

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* The great difference in the effect produced by this influence on gold and silver, which have always been considered as equally difficult to oxidate, can only be explained on the supposition, that nitrous acid is generated ; for this acid, it is well known, acts powerfully on silver, but has no action whatever on gold. The same observation applies to platina,

oxygen, it feizes their oxygen, and becomes oxygenated; but when it paffes from the fluid to the metal again, it affumes its former ftate, and becomes deoxygenated. Now when water is the fluid interpofed, and the influence enters it from the filver fide deoxygenated, (and we fuppofe that it always paffes from the deoxygenated to the oxygenated fide) it feizes the oxygen of the water, and difengages the hydrogen, which accordingly appears in the form of gas; but when the influence enters the zinc wire, it parts with the oxygen, with which it had formerly united, and this either efcapes in the form of gas, unites with the metal to form an oxyde, or, combined with a certain portion of water, &c. may, according to the German chemifts, form nitrous acid. When a metallic folution is the interpofed fluid, the effect produced may be explained in two ways*, but the fimpleft is to fuppofe, that the influence in paffing from the filver wire, feizes the oxygen of the metallic calx, and afterwards depofits it on entering the zinc one; in this cafe no gas fhould appear at the filver wire, but when a perfect metal is employed, oxygen fhould be difengaged from the zinc wire; and this, as has been already mentioned, is exactly what takes place. What I confider, however, as the ftrongeft argument in favour of this hypothefis, is, that all fluids which do not contain oxygen, are incapable of tranfmitting the galvanic fluid, fuch as alcohol, æther, the fat, and effential oils, as I have proved by direct experiment; but on the contrary, that all thofe which do contain oxygen conduct it more or lefs readily, as all aqueous fluids, metallic folutions, and acids, more efpecially the concentrated fulphuric acid; which it decompofes. In this laft inftance, the oxygen produced can hardly be afcribed to the decompofition of water; for this acid, when properly concentrated, does not contain any fenfible quantity. By this theory alfo, we can readily explain the oxydation of the zinc plates in the machine; where the fluid in paffing from the different pairs of plates, appears to be alternately oxygenated and deoxygenated. Although I am not by any means intirely fatisfied with this hypothefis, yet, as it is the only one by which I can explain the different phenomena, it was thought advifeable to throw it out, merely with a view to induce others to reafon upon the fubject, and to incite them to make experiments, by which alone truth can be afcertained.

A convenient and powerful machine is at prefent a great defideratum in galvanifm: the common pile, although at firft fufficiently ftrong, very foon lofes its power; it is befides very troublefome to be conftantly repiling it, and clearing the different pieces from oxidation, &c. which muft neceffarily be done, if it is intended that the apparatus fhould produce the full effect. The contrivance which has fucceeded beft with me is as follows: I conftructed a kind of trough of baked wood, 26 inches in length, 1,7 inches deep, and 1,5 inches wide; in the fides of this trough grooves were made oppofite to each other, about the tenth of an inch in depth, and fufficiently wide to admit one of the plates of zinc and filver when foldered together; three of thefe grooves were made in the fpace of one inch and three tenths, fo that the whole machine contained 60 pairs of plates. A plate of zinc

* See the former paper on this fubject.

<div align="right">and</div>

and filver, each 1,6 inches fquare, well cemented together, were introduced into each of thefe grooves or notches, and afterwards cemented into the trough by a compofition of rofin and wax, fo perfectly, that no water could pafs from one cell to the other, nor between the plates of zinc and filver. This circumftance muft be ftrictly attended to, elfe the machine will be extremely imperfect. When all the plates were thus fecured in the trough, the interftices or cells formed by the different pairs of plates were filled with a folution of the muriate of ammonia, which here fupplied the place of the moiftened papers in the pile, but anfwered the purpofe much better. It is hardly neceffary to obferve, that in fixing the zinc and filver plates, they muft be placed regularly, as in the pile, *viz.* alternately zinc and filver, the filver plate being always on the fame fide. When a communication was made between the firft and laft cell, a ftrong fhock was felt in the arms, but fomewhat different from that given by the pile, being quicker, lefs tremulous, and bearing a greater refemblance to the common electrical fhock. I conftructed two of thefe machines, which contained in all 100 pairs of plates; thefe when joined together, gave a very ftrong fhock, and the fpark could be taken in the day time at pleafure ; but what furprized me not a little, was the very flender power which they poffeffed in decompofing water; in this refpect they were certainly inferior to a pile of 30 pairs, although fuch a pile would not give a fhock of one third the ftrength.

This apparatus retained its power for many days, and would in all probability have retained it much longer, had not the fluid got between the dry furfaces of the metals. To remedy this defect, I have now foldered the zinc and filver plates together, and find that this method anfwers very well.

The zinc plates may be cleaned at any time, by filling the different cells for a few minutes with the dilute muriatic acid. Although this apparatus may not intirely fuperfede the pile, efpecially if it fhould be found to decompofe water, &c. but flowly, yet in other refpects it will no doubt be found very convenient and portable.

Having been lately favoured by Mr. Howard with narrow flips of platina, of a confiderable length, I repeated the experiments refpecting the decompofition of water, and with very nearly the fame refults. Seven meafures of the compound gas obtained in the manner formerly defcribed, being exploded over mercury by the electric fpark, there remained no more than $\frac{1}{5}$ of a meafure, or $\frac{1}{35}$ of the whole ; this appeared to be azote, for it fuffered no diminution with nitrous gas, nor was it inflammable. I likewife mixed one meafure of this compound gas with two of nitrous gas, and the diminution amounted to one meafure; nearly one third of the whole was therefore oxygenous gas, the remaining two thirds being hydrogen gas, with a little azote.

Having found that the folutions of metals in acids were decompofed by this influence, I next wifhed to afcertain if their folutions in alkalies, more particularly ammonia, could be decompofed in the fame way. For this purpofe, I added to a dilute folution of the nitrate of filver fome pure ammonia, until the mixture fmelled ftrongly of the latter fubftance. This mixture was introduced into a glafs tube fecured by corks in the ufual way, and filver wires applied. When the tube was placed in the circle

of

of communication, a very rapid production of gas took place from the wire connected with the filver extremity of the machine, although but little or none efcaped from the zinc wire during the whole procefs.

After fome time a quantity of greyifh flacks, evidently metallic filver, were feparated by the wire which gave out the gas, and a dark grey powder was depofited on the zinc wire; at this period of the procefs much lefs gas was difengaged, although a confiderable quantity ftill appeared at the filver wire. After fome hours the apparatus was removed; at this time a very confiderable quantity of metallic filver was depofited, and the zinc wire was encrufted with a blueifh black fubftance. On endeavouring to remove this cruft with the finger part of it exploded, although ftill moift: the wire was found to be very much corroded, and full of holes. Next morning the powder which adhered to the wire being dry, was touched with a knife, when it exploded again, with a very confiderable noife. There can be little doubt, that thefe explofions proceeded from the fulminating filver of Bertholet, which muft have been formed at the zinc wire; indeed fome effect of this kind was expected. This experiment was repeated, and inftead of a folution of filver nothing but pure ammonia was introduced into the tube, which anfwered equally well; for the filver of the zinc wire after being corroded, &c. was immediately taken up by the Vol. Alkali, and afterwards depofited in its metallic form by the filver wire. A fmall quantity of a fulminating fubftance likewife adhered to the zinc wire; and the fluid furrounding it, being poured into a wine glafs, depofited after ftanding for fome time; a black fhining film, which when dried, detonated like Bertholet's powder, and refembled it in all refpects.

Another tube containing pure ammonia had copper wires introduced into it; after this had been fubmitted to the action of the machine for fome time, the fluid furrounding the zinc wire affumed a fine blue colour from diffolved copper, and the filver wire began to depofit metallic copper; this procefs continued for feveral hours, when a confiderable quantity of very pure metal was precipitated; the blue colour of the fluid, however, never extended fo high as the end of the filver wire, for as faft as it afcended, the metal was immediately thrown down in its metallic form, by this wire. I next filled the tube with an ammoniacal folution of copper, and employed copper wires. In a very fhort time after this had been expofed to the action of the machine, the upper part of the fluid in contact with the filver wire, became confiderably paler, and copper was precipitated; in about an hour the whole of the fluid in this part of the tube became as colourlefs as diftilled water, fo completely had the metal been precipitated. The precipitated metal was alfo the pureft I had ever feen. From thefe experiments it would appear, that the galvanic influence might be employed with fuccefs in the analyfis of minerals, more particularly in feparating lead, copper, and filver, from their different folutions; a very fmall quantity of a metal may likewife be detected in this way, as I have found from direct trial. Before I finifh thefe obfervations on ammonia and ammoniacal folutions, I fhall give the refult of one experiment, where the vol. alkali itfelf was decompofed.

A quantity

A quantity of pure ammonia being introduced into a bottle, and inverted in fome of the fame fluid in the manner already explained, the connecting wire on the zinc fide was changed from filver to platina; in this cafe a very rapid production of gas took place from both wires, but the greateft from the filver wire. After two ounce meafures of this gas had been collected, it was examined, and found to confift of,

Hydrogen gas	-	-	15 parts
Azotic gas	-	-	13
And oxygen gas	-	-	2 nearly.

30

Indeed the quantity of oxygen was fo fmall, that it could only be detected by nitrous gas, and it may have been originally contained in the fluid. In this cafe the ammonia muft have been decompofed at the zinc wire, the difengaged oxygene uniting with the hydrogen, while the azote of the ammonia efcaped in the form of gas, and mixed with the hydrogen difengaged at the fame time from the filver wire.

In a former paper I mentioned that the fulphuric acid was decompofed by galvanifm, without defcribing however the particular appearances. A quantity of the concentrated fulphuric acid was poured into a tube bent in the form of the letter V, two flips of platina were introduced, one into each leg, and brought within an inch of each other at the bend; the tube was then placed in an inclining pofition with the bend downwards, and the platina flips were attached to the extremities of the machine.

In a few feconds a very confiderable quantity of gas made its appearance at the zinc wire, and continued to rife during the whole procefs; from the filver wire a fmall quantity of gas was likewife difengaged, but in a fhort time the fluid in this leg of the tube loft its tranfparency, and became milky and opake. After the procefs had been continued for feveral hours, a quantity of a yellowifh white powder was depofited on the filver wire, and the fluid ftill continued muddy; the acid, however, in the other leg, not only preferved its tranfparency, but became even more fo; it likewife had a peculiar fmell, fomewhat like that of the ox. mur. acid when very dilute. The tube being removed, water was added to the muddy acid, in confequence of which a whitifh powder was depofited, which was found to be fulphur. The yellowifh powder depofited on the platina flip proved likewife to be fulphur. The gas obtained during this procefs was not particularly examined, it was only found not to be inflammable, or if it contained any inflammable gas, it muft have been in very fmall quantity.

The effects of this influence upon the nitrous acid were not exactly what might have been expected; fome of this fluid in its moft concentrated and fuming ftate, being introduced into the bent tube juft defcribed, and the platina flips applied, very little or no gas was difengaged from the flips or wires of either fide, nor even after fome time, was the appearance of the acid fenfibly changed. The influence, however, was perfectly tranf-mitted, for the machine, although in full action, did not give the leaft fhock whilft the

tube

was applied; befides the wire on the zinc fide being removed from the plate, and plunged in a cup containing water, whilft a copper wire reaching from the cup to the zinc plate, preferved the communication, gas iffued copioufly from the platina wire, and the copper was corroded as ufual.

The acid was then diluted with an equal bulk of water, or rather more, and being again introduced into the tube, ftill very little gas was produced on either fide. From thefe facts it would appear that the nitrous acid is fo perfect a conductor of this influence as to tranfmit it like metals, without being at all acted upon by it. This may poffibly be owing to the great proportion of oxygen which enters into its compofition, for we have already remarked, that all fluids, containing very little or no oxygen, are perfect non-conductors, or nearly fo; and it is probable that the conducting fluids are more or lefs fo in proportion to their quantity of oxygen.

It was remarked in a former paper, that it was highly probable, the nitrous, or fome other acid, was produced at the zinc wire, I fhall now bring forward fome additional facts and arguments in fupport of this hypothefis.

1ft. If the tincture of litmus be introduced into the bent tube already mentioned, and platina wires employed; after fome time the whole of the litmus in the leg of the tube connected with the zinc wire will be rendered perfectly red, and in fome places the colour even partially deftroyed, a well known effect of the nitrous acid on blue vegetable infufions. But if the litmus be mixed with a little of any pure alkali, no fuch change of colour can be perceived.

2dly. All metals which are diffolved or acted upon by the nitrous acid, are likewife very quickly corroded by the galvanic influence; but fuch as are not acted upon by this acid are not at all affected by this influence. Of this we have ftriking examples in filver, mercury, gold, and platina; the firft two are readily acted upon both by the nitrous acid and galvanifm, but the two laft by neither in any fenfible degree, and poffibly not at all when perfectly pure. Now filver, which has always been confidered as a perfect metal, cannot be oxydated by any of the ufual methods, any more than gold, yet this influence has fully as great an effect upon it, as upon copper; which we conceive may be explained in the following manner:—Let us fuppofe that a little nitrous acid is produced on the furface of the filver, the metal muft in this cafe be in fome degree acted upon by it, and this action will neceffarily difpofe it to unite with oxygen, which, being thus in its nafcent ftate, muft readily combine with the filver, and form infoluble nitrate with excefs of oxyde, which will, confequently, be but little foluble in water. Now that this is really the cafe, we think is rendered probable by the following experiments:

A fmall wide mouthed-bottle was filled with a dilute folution of pure pot-afh, and inverted in a cup, containing fome of the fame folution. Two filver wires, previoufly bent, were introduced a little way beyond its mouth, and then connected with the machine in the way formerly defcribed. A very rapid production of gas took place from both wires, and a few black flocks were perceived floating in the fluid: when a fufficient

<div align="right">quantity</div>

quantity of gas was thus obtained, it was examined, and found to confist of oxygen eleven parts, and hydrogen twenty-five parts; and four meafures of it being exploded by the electric fpark, lefs than half a meafure remained, or about one-ninth of the whole. From hence it is evident that the filver was but very little acted upon in this experiment, for we obtained nearly the fame quantity of oxygen as if gold or platina wires had been employed. The bottle was next filled with diftilled water, and the fame procefs repeated. In this cafe the filver was much corroded, and very little gas efcaped from the zinc fide; a fufficient quantity of gas being, however, obtained, it was examined, and found to confift of oxygen one part, and hydrogen fix parts. This mixture did not explode without the addition of more oxygen. The caufe of thefe different refults is readily explained on the fuppofition of an acid. For in the firft experiment the generated acid would immediately unite with the alkali in preference to the filver, and by this means the principal agent in the oxydation would be removed. Similar experiments were made with copper wires, and with exactly the fame refults: in this cafe, when the bottle was filled with the alkali, no green fubftance was produced at the zinc wire; indeed, the copper was fcarcely, if at all, acted upon.

3dly, The corroded fubftances generated by this influence at the extremities of the zinc wires are not pure oxydes, which, upon the fuppofition of fimple oxydation they ought to be, when nothing but diftilled water is employed. We have remarkable examples of this in the blueifh green fubftance obtained from copper, and the dirty greenifh yellow one from mercury, the laft of which exactly refembles what has been called nitrous turbeth when too much wafhed with water, and if pure ammonia be added to it, it inftantly becomes black, a proof that it contains fome acid, for ammonia has no effect upon the pure oxyde of mercury, but a confiderable one upon all the impure, rendering them black, as turpeth mineral, &c. With regard to the green fubftance from copper, it is very certain that we have no pure oxyde of this colour, for all the green oxydes, as they have been improperly called, contain acids of fome kind, as the carbonic, arfenical, acetous, &c. and it is well known that the precipitates by the carbonated alkalis, &c. are all of this kind. Indeed, there is but one pure oxyde of copper, viz. the dark red, or deep brown. Now, as thefe impure oxydes cannot be generated without an acid, and as there can be none in diftilled or lime water, it muft follow, that an acid is fomehow generated in the procefs; and as this acid acts both upon mercury and filver, it is moft probably the nitrous. The quantity, however, is certainly fmall, owing, in a great meafure, to the weaknefs and imperfection of our machines; and it is this circumftance which renders it fo difficult to detect it with certainty.

It may be afked, how can this acid be formed? If azote were a compound fubftance, as has been lately affirmed, this queftion could be very readily anfwered; but if not, we muft then fuppofe that a fmall quantity of azote is always prefent in water, however long boiled, or repeatedly diftilled, and that it is with this the oxygen in its nafcent ftate

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unites

unites and forms nitrous acid. The production of ammonia at the silver wire can be explained on either hypothesis; but we shall take another opportunity of examining this part of the subject.

The new apparatus which I described in a former paper, seems to answer better than was at first expected; it has now been in use occasionally for a month, and whenever a fresh solution of salt, or muriate of ammonia, is introduced, it appears to be fully as strong as at first. The addition of a very small proportion of nitrous or muriatic acid to the solution of common salt, adds considerably to its efficacy.

V.

Description of a Mercurial Air Pump; and of a Double-barreled Air Pump. By RICHARD AUGUSTUS CLARE, *Surgeon, Jamaica. Communicated by the Author.*

FIG. 1, Pl. XI. represents a section of the different parts of the pump; A, a large iron tube (being the barrel of the pump) B a smaller ditto. These tubes are flanched to the iron step C. This step has a perforation in it, making a communication between the bottoms of the tubes. The tube B is surmounted by a plate of iron, on which is flanched the iron hemisphere D. The hemisphere has a hole at top, one-tenth of an inch in diameter, with a cap valve and bason at E. A pipe, *b b*, of iron or glass, is inserted through the upper part of the hemisphere, and reaches within a little of the iron plate on which it is flanched. The pipe *b b* rises about thirty-five inches above the level of the bason E; it is reverted at top, and descends to be inserted into the upper part of the piece G, which has a stop cock at *c*, leading to the receiver. To the lower part of the piece G, the long barometer gauge *d* is attached, the lower end of which descends into the bason of mercury *e*. F is a cylindrical piece of light wood (which I shall call the piston) made to slide easy in the hollow cylinder A. There is a small screw at *a*, to draw off the mercury when required. The valve at E is made of a thin iron hoop about three-fourths of an inch long, and half an inch in diameter; the lower end of this hoop is covered with a piece of bladder in the manner of a drum. The valve fits loosely into the cap, which has three grooves on the inside, made in a perpendicular direction from bottom to top. The use of these grooves is to allow the mercury to rise freely by the sides of the valve hoop. The use of the bason is only to contain the superflous mercury. To try the power of this pump, let the piston be pushed down to the bottom of the cylinder, and retained there, as it will have a tendency to rise when the mercury is poured in. Take the valve out of the cap at E, and pour mercury into the bason till it rises as high as the top of the cap, then thrust down the valve, till the mercury runs over the top of its hoop and fills it. The specific gravity of the hoop so filled will be little less than that of mercury itself. Let the piston be now

drawn

drawn up, the mercury will fubfide from the bottom of the valve, and make a vacuum in the hemifphere. No air can enter by the valve, as it will be always furrounded by and covered with mercury.

When the mercury finks below the bottom of the tube *b b*, the air will enter the vacant hemifphere from the gauge or receiver. When the pifton is depreffed, the mercury will again rife in the hemifphere, and clofing the orifice of the tube *b b*, will expel the air through the valve at E. The mercury will (as the exhauftion goes on) rife in the tubes *b b* and *d*, and thereby indicate the degree of rarefaction, but it can never rife fo high as to run over the arch of the tube *b b*, becaufe the column of mercury in that tube will be long enough to balance the weight of the atmofphere. As the ofcillation of the mercury in the tube *b b* is apt to be very violent if not reftrained, a fmall cork ball, with a fhort piece of iron wire ftruck into one fide of it, may be placed under the orifice of that tube, the iron wire rifing within the tube as reprefented at fig. 2. This ball will float on the mercury, and being thereby preffed tight againft the orifice of the tubes, will prevent the mercury from rifing too faft, or producing any fudden ofcillation therein. The drawings and defcription of an air pump on this principle, reduced to a very portable fize, were fent to Mr. Adams, mathematical inftrument maker, of Charing-crofs, London, in the beginning of the year 1796, who deemed the execution of it impracticable. Being informed of this, I defired the drawings, &c. might be laid before Mr. Nairne; who, though he offered to make the inftrument, gave it as his candid opinion that it would not anfwer my purpofe to the full extent of my wifhes. I, therefore, dropt the idea of getting an inftrument of this kind made in England, on account of the great diftance and length of time neceffary to carry on a correfpondence on fuch a fubject.

Mr. Nairne having given no reafon, however, for his opinion, except that it would coft a great deal of money, I was refolved to attempt the fabrication of one myfelf. As I had no means of getting iron pipes made to the fize I wifhed, I was obliged to make fhift with what I could get. The tube A A was of caft iron (and had formerly belonged to a water pump) cut to the proper length. The tube B B was a gun barrel, and the reft of the inftrument was made up in the fame way, of any thing I could pick up. Being but a bungling workman, however, having but few tools, and no advice, or affiftance, I never could make all the joints perfectly air tight, though I laboured hard to effect it.

I am fully convinced, however, from the experience I have gained with this clumfy and imperfect inftrument, that it would anfwer the purpofe if it were well conftructed.

The greateft inconvenience attending the ufe of it was occafioned by the ofcillation of the mercury, in confequence of the great difference in the diameters of the tubes through which it moves.

Deſcription of a Double-barreled Air Pump.

Fig. 2. reprefents a fection of the pump, &c. where A and B are the two barrels, open at top, and furnifhed with piftons, which are worked by rack and wheel in the ufual manner. The piftons are alike in each barrel, and are perforated through the centre, as is the folid part of the rack alfo, to admit the hollow wire *a a* to flide therein. This wire is made to move air tight, by means of collars of leather in the centre of the pifton. The wire is hollow from end to end, and is fixed to the centre of the bottom of the barrel. Near the upper end of the wire a hole is drilled through it, at right angles with that in its axis; this hole is at fuch a diftance from the bottom of the barrel, that, when the pifton is drawn up, the hole opens into the barrel, as is feen in the barrel B. Each pifton is furnifhed with a common filk valve *g*, opening upwards. The hole in the pifton rack, through which the wire flides, is fecured at top by the ftop fcrew *h*. The barrels are flanched on the bottom piece *c c*, into which the fliding wires are fixed as above defcribed. The hollow wire in the barrel B communicates at bottom with the duct *d b*, leading through the cock *c*, and that in the barrel A communicates with the duct *e f*; *f* is the hole leading back to the receiver.

The cock *c* is perforated, as reprefented in the figure, and has a motion of one quarter of a revolution to the left, from its prefent pofition. The piftons fhould be well leathered, and always have about a quarter of an inch of oil on their upper furfaces. The operation of this pump is as follows:—Let the cock ftand as reprefented in the figure, in which fituation it opens a communication between the receiver and the barrel B, through the duct *c b* and fliding wire, when the pifton is drawn up. When the pifton B is depreffed, the lateral hole in the fliding wire is ftopped by paffing through the collar of leather, and the air below it not being able to return into the receiver, is expelled through the valve *g* in that pifton. By this time the pifton A has arrived at the top of that barrel, and the air will have a free paffage into it from the receiver, through the hollow wire, &c. On the depreffion of this pifton the air below it will be expelled through its valve *g*, and thus by the alternate elevations and depreffions of the piftons a pretty good exhauftion will be made: at length, however, it will ceafe to take more air from the receiver, in confequence of the fmall quantity of air in the valve holes and under the piftons that cannot be expelled. When this is the cafe, let the cock be turned one quarter round to the left, in which fituation it will make a paffage through the duct *d b*, at the fame time that it cuts off the communication between the barrel B and the receiver, and the little air under the valve, &c. in the barrel A will immediately run into the exhaufted barrel B through the duct *d b* and hollow wire *a a*. The pump A will now make a better vacuum, and take air again from the receiver, which will, when the pifton A defcends, be transferred to the barrel B; and, finally, when the pifton B defcends, the air will be expelled through the valve in its

<div align="right">pifton.</div>

piſton. Thus, by only turning the cock one way or the other, the cock is made to exhauſt with one barrel or both. When it exhauſts with one only, the other is rendered ſubſidiary to it by clearing it of air after each ſtroke.

To render the pump more eaſy in working, there ſhould be a hole in the bottom of each barrel, furniſhed with a ſilk valve, through which the air would enter during the firſt ſtrokes of the pump. The ſituation of theſe holes are ſhewn by dotted lines in the figure. The air may be reſtored to the receiver by unſcrewing the ſtop ſcrew *h*.

Being foiled in all my attempts to procure a mercurial air pump, I turned my thoughts to the improvement of the common double barreled pump, which, I believe, was invented by Dr. Robert Hooke; and which appeared to me the moſt ſimple and convenient form, provided it could be made more accurate in its operation.

The reſult of ſome thought, and various experiments made to this purpoſe, was, that in theory I brought that pump to the perfection of rarefying permanent dry air 248000000 times, ſuppoſing the air to have a uniform power of expanſion to ſuch a degree, and the barrels of the pump to be twelve inches long by two in diameter. The drawings and deſcription of this pump are now in the hands of Mr. Nairne, who is making one on that conſtruction, particularly adapted to the performance of chemical experiments on air. I have been purpoſely ſhort in my deſcription of the ſcheme herewith ſent (which differs in many reſpects from that making by Mr. Nairne) as I purpoſe giving a full deſcription of the other and its performance (if I find it to equal thoſe hitherto contrived) when the inſtrument comes to my hands.

Query, does the lower valve of Mr. Cuthberton's air pump increaſe the power of that inſtrument, and would it not work equally well without it?

My reaſons for ſuſpecting the utility of this valve are the following:

Firſt, there muſt always be a quantity of air above the piſton, when drawn up, that cannot be expelled.

Secondly, this quantity of air will be uniformly the ſame, whether at the beginning or end of the exhauſtion, as it will take the ſame force to open the expulſion valve at one time as at another, and this air muſt be as denſe as that of the atmoſphere at leaſt.

Thirdly, when the piſton begins to deſcend, the large conical valve muſt thruſt out of its ſhell, before that at the bottom of the barrel can poſſibly ſhut, for the ſhutting of the one depends on the motion which cauſes the opening of the other.

Fourthly, when the piſton begins to aſcend, the valve at the bottom of the barrel muſt open before that in the piſton can ſhut, for the reaſon above ſtated.

Now let us ſuppoſe the piſton at the top of the barrel, when the winch is turned the large conical valve will be opened, before that in the bottom of the barrel can ſhut, at leaſt there will be an inſtant when both valves may be ſaid to be half open, the air above the piſton being much denſer than that below it, will take advantage of this opportunity to diffuſe itſelf equally through the barrel and receiver. If, however, the air does not all

M m 2 eſcape

escape at the commencement of the down stroke, it will have another opportunity at the commencement of the up stroke, for then the bottom valve opens before that in the piston can shut, and the air in the barrel will again be at liberty to exert its superior spring to get back into the receiver.

It is with the greatest diffidence I put this question, having the highest respect for Mr. Cuthberton's abilities.

If I have advanced what is erroneous, or if I have mistaken the principle of the instrument, I beg to be corrected and informed.

VI.

A Memoir, in which the Question is examined, whether Azote be a simple or compound Body? By CHRISTOPHER GIRTANNER, *Doctor of Physic at Gottingen.*

FROM this experiment we see that pure hydrogen gas combines with the oxygen gas which had remained in the lungs, and forms azote gas.

16. Another experiment, which appears to me to confirm my opinion, was made by Mr. Henry of Manchester. He repeated it several times, and always with the same success. His explanation of this phenomenon may be seen in Scherer's Journal, which is different from mine, because he considered azote as a simple body. His experiment is as follows:—In a recurved tube over mercury a mixture was made of 94,5 measures of carbonated hydrogen gas from acetite of pot-ash, and 107,5 measures of very pure oxygen gas obtained from the oxygenated muriate of pot-ash. The mixture amounting to 202 measures, was reduced, by an electric explosion passed through it, to 128,5 measures, and afterwards by lime water to 54,0 measures. So that in this experiment 23 measures of azote gas were produced by the electric explosion of the oxygen united with the hydrogen.

17. Of all known bodies zinc is, if I am not deceived, that which most easily unites with oxygen; it takes it from almost every other body, and this property renders it highly useful for the exhibiting the minutest quantities of oxygen. It was more particularly by means of zinc that I succeeded in separating the oxygen of the muriatic acid from its base. I have likewise used it to make the last analysis of ammoniac. Filings of zinc are to be mixed in a retort, with concentrated fluid ammoniac. A communication being thus made between the retort and the pneumatic apparatus, the whole being kept in digestion for several days, taking care not to raise the heat too much, the ammoniac is decomposed. The oxyde of zinc remains in the retort, and in the pneumatic apparatus a considerable quantity of hydrogen gas is obtained, together with a small quantity of ammoniacal gas and of undecomposed azote. It is easy to prove that the hydrogen gas was

not

not obtained from the water in which the ammoniac was diffolved; becaufe the quantity of the gas is too large, and that of the azote too fmall, to admit of any doubt refpecting the decompofition of the latter.

Such are the experiments, from which it appears to me, that we are juftified in concluding, that azote is a compound of oxygen and hydrogen. I muft refer to a fecond memoir for feveral other experiments no lefs decifive, which require to be repeated before I fubmit them to the examination of thofe accurate and enlightened chemifts, to whom this memoir is addreffed.

Azote being, therefore, if I am not deceived, a body compofed of hydrogen and oxygen, it follows:

That the atmofphere is not, as has been hitherto fuppofed, a mixture of oxygen and azote gafes, but rather a mixture of oxygen and hydrogen gafes; water in form of gas, if I may be permitted to ufe this expreffion.

When, by thofe chemical experiments which have been improperly called eudiometrical, the oxygen is feparated from the hydrogen, this feparation can never be totally or completely made. Part of the oxygen remains united to the hydrogen, and forms that chemical combination which we call azote, and obtain in thefe experiments. The oxygen, fo indifpenfible for fupporting the life of organized beings, is changed by its combination into hydrogen, into azote, which is not only incapable of fupporting life, but is a true poifon, in confequence of the affinity it has with oxygen, and the avidity with which it takes it from organized bodies.

Oxygen has fo ftrong an affinity with hydrogen when both are mixed in the atmofphere, that it is very difficult to feparate them entirely, and hence the great difficulty of anayfing azote. When charcoal, fulphur, a lighted candle, or the metals, have ceafed to burn in atmofpheric air, when animals have perifhed in a given quantity of this fluid, the remainder ftill contains a notable proportion of oxygen. Phofphorus burns in this refidue very well, and for a confiderable time: and even when the phofphorus has ceafed to burn, there always remains a fmall quantity of oxygen united to hydrogen, that is to fay, there is a remainder of azote. We may, neverthelefs, as I have feveral times obferved, deprive the atmofpheric air of almoft the whole of its oxygen, and render the analyfis nearly complete by heating phofphorus in it for a certain time. Phofphorated hydrogen gas is then obtained by the change of part of the azote into hydrogen.

From the experiments here recited, it follows, that the eudiometer, fuch as it exifts at prefent, is founded on erroneous principles, the azote obtained in thefe experiments being always a product of the operation, which did not exift before in the form of azote previous to the examination. Von Humboldt, who is much difpofed to draw general conclufions from infulated facts, appears to have been deceived when he advances that the earths may be ufed to determ.[e]c the quantity of azote contained in atmofpheric air. The earths do not point out the azotic part; but change a portion into azote.

2

Cultivated

Cultivated earth, brought into contact with the atmosphere, abforbs the oxygen, and forms azote. In countries fituated near the poles, and on the top of the Alps, where the earth is always covered with fnow, the atmofpheric air contains a greater quantity of oxygen than in fouthern or low countries, becaufe the fnow prevents the air from coming into contact with the earth.

The conftituent principles of azote being now known, we are enabled to form a new theory of the art of making nitre. In the Annals of Chemiftry, Vol. XX. I obferve fome valuable obfervations on this fubject. For inftance, p. 313, " Earth extracted from deep fubterraneous " places, inacceffible to the light, fuch as caverns, requires only to be expofed to the air " a few days to produce falt-petre in abundance. It is proper to obferve, that thefe earths " do not furnifh a fingle atom of nitre when they are firft taken from the moift and dark " place where they were formed ; and that it is only by combination or combuftion of the " azote of the earth with the oxygen of the atmofphere that this falt is produced."

The explanation which is given, page 314, of that interefting phenomenon, is a proof how diftant we were from the truth before the nature of azote was underftood.

As lime abforbes oxygen with avidity, we fee the caufe why rooms that have been newly white-wafhed are unwholefome.

The following bodies are combinations of hydrogen with oxygen in different proportions:

Azote.	Oxygenated muriatic acid.
Gafeous oxyde of azote.	Nitro-muriatic acid.
Nitrous gas.	Water.
Nitrous acid.	Atmofpheric air.
Nitric acid.	Ammoniac.
Muriatic acid.	

I hope foon to prove that pot-afh, foda, and fulphur, ought to be added to that lift. Phofphorus appears to me to be hydrogen, in the pureft ftate in which we are acquainted with it; but this I am very far from having it in my power to prove. The analyfis of fixed alkalis is completed. As, however, I have no intention of faying any thing upon that fubject in the prefent article, I fhall confine myfelf to pointing out one of the decifive experiments. When an alkali is fufed with filiceous earth to make glafs, the alkali is analyfed. The hydrogen efcapes under the form of gas, and the oxygen becomes combined with the filiceous earth; glafs being merely an oxyde of filex. Too great a quantity of oxygen fhould not be admitted, as it renders it lefs tranfparent; for this reafon it is ufual to add fubftances which ftrongly attract oxygen, fuch as manganefe.

Mr. Mayer, to whom I had communicated the refult of my experiments, by which I fo evidently confirmed an idea which he had ventured to throw out, and of which he has the merit of being the author, fent me a paper, from which I fhall give an extract :

" I am inclined," fays he, " to think with Mr. De Luc, that the evaporation of water, " as it continually takes place on a large fcale in nature, is a real converfion of the water

" into

" into air. It is true, that we have not been able in our laboratories * to accomplish the
" changing of water into evaporation. This arifes from our being ignorant of the fhare
" which electricity and light have in this natural procefs. Neverthelefs, it appears pro-
" bable to me, that the ponderable parts of atmofpheric air, that is to fay, oxygen and
" azote has no other fource than the water with which the furface of the globe is covered.
" The very fmall quantity of oxygenous gas which is exhaled by vegetables by means of
" the fun, is far from being able to repair the enormous confumption of that principle
" which daily takes place in our atmofphere in fo many different ways. Now, fuppofing,
" according to this theory, that 100 grains of water are changed, by the unknown procefs
" of nature, into 100 grains of atmofpheric air, that is to fay, into a chemical mixture of
" oxygen and hydrogen, we can find, by a very fimple calculation, in what proportion
" oxygen and hydrogen are combined to form the azote which we obferve in the atmof-
" phere. Let us not, however, forget that the nature of what we call azote is not always
" the fame, and that it varies confiderably in the proportions of its conftituent prin-
" ciples †.

" I fhall call water E, oxygen O, hydrogen H, the ponderable part of the atmofpheric
" air L, the ponderable part of azote gas S.

$$\text{" Then } 100 \, E = 85 \, O + 15 \, H.$$

" But a cubic inch of atmofpherical air is compofed of $\frac{1}{4}$ of a cubic inch of gas,
" and $\frac{3}{4}$ of a cubic inch of azote gas; therefore

$$1 \, L = \tfrac{1}{4} \, O + \tfrac{3}{4} \, S.$$

" One inch of atmofpherical air weighs 0:46 grains.

" One inch of oxygenous gas weighs 0.51 grains.

" One cubic inch of azote gas weighs 0.44 grains; confequently

$$0.46 \, L = \tfrac{0.51}{4} \, O + \tfrac{3}{4} 0.44 \, S.$$
$$1.84 \, L = 0.51 \, O + 1.32 \, S.$$
$$184 \, L = 51 \, O + 132 \, S.$$
$$100 \, L = 27.8 \, O + 72.2 \, S.$$

" Azote gas being a compound of oxygen and hydrogen.

$$S = x \, O + y \, H; \text{ fo that}$$
$$100 \, L = (27.8 + 72.2 \, x) \, O + 2 \, y \, H.$$

" Now nature changes 100 grains of water into an hundred grains of atmofpherical
" air; confequently

$$100 \, E = 100 \, L; \text{ whence}$$
$$27.8 + 72.2 \, x = 85, \text{ and}$$
$$72.2 \, y = 15;$$

* See, however, Prieftley, in Philof. Journal, IV. 193, and elfewhere.---N.

† This is a very important point upon which Mr. Mayer fixes our attention. It will be neceffary to
make a great number of very exact experiments to determine the different degrees of the oxydation of hydro-
gen, and to diftinguifh the different gafes comprehended, till now, under the general name of azote gas.

<div align="right">Then.</div>

Then $x = 0.79$, and $y = 0.21$ nearly.

$S = 0.79\,O + 0.21\,H$, or $100\,S = 79\,O + 21\,H$.

" That is to fay, 100 grains of azote gas are compofed of 79 grains of oxygen and 21 " grains of hydrogen."

Having applied this ingenious calculation of Mr. Mayer to other oxydes of hydrogen, I found as follows:

Let N denote the nitric acid: we know by experiments that

$100\,N = 79.5\,O + 20.5\,S.$

Inftead of S, let us put its value, as found by the preceding calculation, and we fhall have

$100\,N = 79.5\,O + 20.5\,(0.79\,O + 0.21\,H) = 95.7\,O + 4.3\,H$ nearly.

Thus 100 grains of nitric acid contain nearly 96 grains of oxygen, and 4 grains of hydrogen.

The following are the refults of the fame calculation:

100 parts of ammoniac $= 63.72$ oxyg. $+ 36.28$ hydrog.

100 parts of azote $= 79.00$ oxyg. $+ 21.00$ hydrog.

100 parts of atmofph. air 84.67, oxyg. $+ 15.33$ hydrog.

100 parts of water $= 85.66$ oxyg. $+ 14.34$ hydrog.

100 parts gafeous oxyde of azote $= 86.77$ oxyg. $+ 13.23$ hydrog.

100 parts of nitrous gas $= 93.28$ oxyg. $+ 6.72$ hydrog.

100 parts of nitrous acid $= 94.33$ oxyg. $+ 5.67$ hydrog.

100 parts of nitric acid $= 95.70$ oxyg. $+ 4.30$ hydrog.

Are then oxygen and hydrogen the two elements of which all the bodies in nature are formed. It appears to me very probable. It appears alfo probable that the heavier a body is the more oxygen concentrated and deprived of caloric it contains, and that, on the contrary, the more hydrogen a body contains, the lighter it muft be. But thefe are only hypothefes which future experiments will either confirm or replete.

It feldoms happens that the two conftituent principles of azote enter, as fimple bodies, into other combinations; they much oftener form one fingle ingredient by their union in different proportions. Coal, or the oxyde of diamond, is found in many bodies, and the diamond itfelf in none with which we are acquainted. We obtain coal in our chemical decompofitions, and never the diamond. We do not even know fuch a thing as diamantic or adamantic acid, although carbonic acid is familiar to us. No chemift would think of affirming that we exhale diamond in the act of refpiration, but that we exhale a quantity of coal. The diamond itfelf is not, perhaps, a fimple body; it probably contains oxygen; fince all tranfparent bodies, if I miftake not, contain more or lefs of it. Azote, like coal, is fufceptible of various combinations merely as azote; as a compounded principle, which gives to bodies properties very different from what is given to them by the combination of the two fimple principles of which azote is compofed. Azote, as a conftituent principle of bodies, is not hydrogen and oxygen, but azote: exactly as foap is not oil and aklali, but foap. We muft never lofe fight of this.

I have

I have obferved, in feveral experiments, that fulphur frequently affumes a yellow, yellowifh, or orange colour. We are feldom deceived in fuppofing that there is fulphur in bodies which exhibit that colour. Thofe bodies which contain coal are of a black, brown, or a violet colour. Azote difcovers itfelf by a green colour. Plants (etiolées) which have vegetated, defended from the air and light, are white, and contain very little azote. Expofe them to thefe two agents for a few days, they affume a green colour, and from that time the proportion of azote in their compofition becomes much more confiderable. When nitrate of pot-afh is fufed in a glafs retort, and the oxygen gas is feparated, it is obfervable at the end of the operation, whilft the azote is forming, that is to fay, when the oxygen has a greater affinity to hydrogen than to caloric, that the green colour appears, and the feparation of the oxygen gas entirely ceafes.

But what demands the attention of every philofopher is, that when water is expofed to the fun, the light decompofes it, and feparates the oxygen in large quantities. The hydrogen retains the laft particles of the oxygen; azote is formed, and announces itfelf by a green colour. More and more of the water is decompofed; the oxygen (which I have already fhewn to be the principle of life and irritability in organized nature) becomes more fixed; and this azote, produced from the effect which the fun has upon water, becomes an organized body, the conferva fontinalis, a plant which lives, grows, and perpetuates its fpecies. Here are the limits at which philofophy muft ftop—to admire and refpect the fecrets of nature, without knowing if we fhall ever be permitted to penetrate them. It is certain, and I have fatisfied myfelf refpecting it by a great number of experiments in which I have been engaged every fummer for the fpace of more than twelve years, that the influence of the folar light is abfolutely neceffary for changing the water into a plant, or organized azote. No degree of heat can fupply the place of it, and this experiment alone ought to be fufficient to convince every unprejudiced reafoner that heat and light are two fubftances totally different. I am furprized that this green matter of Prieftley, this fontinalis, to which I have given the name of organized azote, has not engaged more of the attention of chemifts. It is the moft wonderful fubftance which exifts, the moft fingular body in nature. Nothing can be more abfurd than what Prieftley has faid concerning it. To reafon as he has done, is, indeed, not to reafon at all. This great man, whofe name will live as long as the fciences fhall be cultivated, has made the moft eminent difcoveries. I admire his talents, but I am forry to obferve, that in all his productions he appears to be more of an experimentalift than a philofopher. Whilft with one hand he unfolds to us the aftonifhing fecrets of nature; with the other he is always ready to fhut our eyes in cafe we have a defire to penetrate further than he wifhes us to fee. In the difpute which he maintained with Dr. Ingenhoufz, concerning the fubject of the green matter about which we are fpeaking, he has given a ftriking inftance of his difinclination to permit the wonders of nature to be beheld, except through his ecclefiaftical perfpective. Ingenhoufz, an enlightened chemift, having defcribed feveral experiments, worthy the attention of every thinking being, relating to this fingular fubftance, adds,

" Water itfelf, or fome fubftance in water, is, in my opinion, changed into this fpecies of
" vegetation. It is a real tranfmutation, which may appear incomprehenfible to a phi-
" lofopher, but which, in reality, is not more extraordinary than the transformation of
" grafs and other vegetables into fat in thofe animals which feed upon plants, or than that
" of the aqueous juice of the olive into oil."

Water is changed into a plant. This is the fact. Ingenhoufz ftops there, and fays,
I do not comprehend it; this is the language of a philofopher. Prieftley, on the con-
trary, is fcandalized at fuch language. He afks Ingenhoufz if he is not afhamed to wifh
to re-eftablifh the long refuted doctrine of fpontaneous generation *.

He fpeaks to him nearly in the language of the inquifitor to Galileo, when he proved
to that immortal man that the fun turns round the earth. Prieftley afterwards gives us
his own theory of the production of the organized azote. He fays, that the feeds of this
plant float every where in the air, over the earth, the fea, the Alps, the low ground, at
the poles, and at the equator, in fummer, in winter, and in every feafon, and that they
are received into water where they germinate. But organized azote is produced in bottles
well ftopped. Dr. Ingenhoufz has even proved that if a bottle be filled with well water,
and reverfed over a bafin filled with water, a great quantity of organized azote will be
formed. Prieftley, who could not, without attributing a fuperior intelligence to the
pretended feeds of this plant, maintain that they came through the water purpofely to fix

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* Dr. Prieftley fays, that the theory of fpontaneous generation is a doctrine long fince refuted. Thefe
are his words :---" Confidering how long the doctrine of equivocal, or fpontaneous generation has been
" exploded." A philofopher fhould never make ufe of fuch an expreffion. There is not an exploded
opinion which we may not recur to and re-examine. The philofopher acknowledges no authority which
prohibits him from admitting any opinion, or forbids him to examine it. There are many other opinions
long fince exploded to which we muft now have recourfe. That, for inftance, of the tranfmutation of
metals. What chemift, of the prefent day, would prefume to deny the poffibility of it: The change of
one metal into another fhould appear lefs difficult than the changing of the fweeteft body (fugar) into the
foureft body (the oxalic-acid) ; than the change of the hardeft body (the diamond) into the fofteft body, the
carbonic acid gas ; or than the changing the moft tranfparent body, the diamond, into coal, the moft opaque
of all bodies. In the nineteenth century, the tranfmutation of metals will be generally known and prac-
tifed. Every chemift, every artift, will make gold. The utenfils for our kitchens will be made of filver,
or even of gold, and this will contribute, more than any thing elfe, to prolong life, which has hitherto
been poifoned by the oxyde of copper, lead, and iron, which we daily fwallow at our meals. There will
then be no other wealth than natural wealth, the production of the foil. Artificial wealth, fuch as gold,
filver, and paper, will be extinguifhed in the hands of thofe who fhall have accumulated them. What a
revolution in fociety! But every enlightened chemift will agree with me, that this revolution is not only
probable, but near.---Note of the author.

Though I have no remarks to offer on this laft affertion of our author, I cannot forbear remarking that
gold and filver, though much ufed as figns of wealth, or tickets of commercial transfer, have always, and
more efpecially in modern times, conftituted a very fmall portion of national wealth. If gold could be
rendered as cheap as iron, the effect upon the wealth of fociety would be incomparably lefs than has refulted
from either of the inventions of the ftocking loom, the fpining gear, or the fteam engine.---N.

themfelves.

themfelves into this bottle which was prepared for them, extricates himfelf by making them pafs through by imperceptible cracks *. Such are the means which are always ufed when we will not fee that which is too obvious. Thus it was that phlogifton was made to pafs through crucibles, and azote through tubes and retorts.

I muft obferve, that in order to fucceed in thefe experiments, it is abfolutely neceffary that the water which is ufed fhould contain gas in a ftate of diffolution, whether it be oxygenous gas, or carbonic acid gas. The more gas it contains, the greater quantity of organized azote will be formed.

It is to be wifhed that the change, which earths undergo from the oxygen which they abforb in decompofing the vapours of water, were attentively inveftigated. I do not doubt but that fuch an inveftigation would lead to fome very important difcoveries.

I fubmit thefe ideas to the enlightened criticifms of thofe illuftrious French chemifts, the editors of the Annales de Chimie, the fathers and creators of this fcience. Before their time, chemiftry was only a crude mafs of facts, ill arranged, and worfe explained.

VII.

An Account of fome Experiments made with the Galvanic Apparatus of Signor Volta. By Mr. Davy, Superintendant of the Pneumatic Inftitution. Communicated by the Author.

IN purfuing a courfe of experiments on the galvanic influence I have made fome new obfervations. They are connected with the curious facts already detailed in Mr. Nicholfon's Philofophical Journal, and they may poffibly lead to elucidations of the phænomena.

The apparatus that I employed was conftructed for Dr. Beddoes, and never confifted of lefs than 110 pairs of metallic plates. I found the fenfible galvanic fhock very much increafed when the parts communicating with the conductors were moiftened with folution of green fulphate of iron. A pile erected with pieces of cloth, wetted in that fubftance, acted rather more intenfely than a fimilar pile erected in the ufual mode. It, however, loft its powers in a fhorter time: perhaps a folution of fulphate of zinc may be employed with advantage.

a. Struck with the curious phænomena noticed by Meffrs. Nicholfon and Carlifle †, namely, the apparent feparate production of oxygen and hydrogen from different wires, or from different parts of the water compleating the galvanic circle, my firft refearches were directed towards afcertaining if oxygen and hydrogen could be feparately produced from quantities of water not immediately in contact with each other:

* " Through fome unperceived fracture," Vol. I. p. 294. " The feeds of this plant infinuate them-
" felves into veffels of water, through the fmalleft apertures," p. 308.

† Philof. Journal, page 183, Vol. IV.

Two

Two filver wires, one from the zinc end of the apparatus, and the other from the filver end, were made to communicate with two glaffes, diftant from each other about five inches, and filled with water that had been long boiled, and was yet warm. Into one of thefe glaffes I dipped the fingers of my right hand, and into the other the fingers of my left, fo that the communication between them was made through my body. Immediately after the fhock, the zinc wire* began to calcine very faft, white clouds diffufing them-felves from it through the water. At the fame time gas was formed round, and extri-cated from the filver wire in the other glafs. The communication was kept up for half an hour; during this time no gas was produced from the zinc wire, which continued to cal-cine throughout the procefs. The gas from the filver wire was caught in a fmall inverted cylinder; examined by the teft of nitrous gas, it appeared to contain no oxygen, and in-flamed with twice its bulk of common air, gave fuch a diminution, as denoted it to be hydrogen nearly pure.

This apparatus being adjufted as before, the communication between the glaffes was made through three perfons; the procefs went on, though lefs rapidly; the oxygen was fixed as before by the filver in one veffel, whilft the hydrogen was given out in the other. When mufcular fibre, living vegetable fibre, or a moiftened thread not exceeding three feet in length, was employed as the medium of connection between the glaffes, fimilar effects were produced, though the gas was evolved more flowly than when the living animal was the connector. Mufcular fibre appeared to be a better conductor than vege-table fibre, and vegetable fibre a better conductor than the moiftened thread.

b. Several glafs tubes about $\frac{1}{3}$ of an inch in diameter, and four inches long, having each a piece of gold wire inferted into one end hermetically fealed, and the other end open, were provided. Two of thefe tubes were filled with diftilled water, and inferted into fe-parate glaffes filled with that fluid. The glaffes were made to communicate by means of frefh mufcular fibre; the gold wires were connected with the machine by means of filver wires; one with the filver end, the other with the zinc end. Gas was immediately given out from both the gold wires; but moft rapidly from that connected with the filver. In four hours and a quarter the procefs finifhed; the water in the tube communicating with the filver being below the gold wire. The gafes were examined; the quantity from the water communicating with the zinc was equal to 33 grain meafures, that from the water connected with the filver to nearly 65 grain meafures. The zinc gas was mingled with 80 meafures of nitrous gas, containing $\frac{1}{22}$ nitrogen; a rapid diminution took place, and when the refiduum was expofed to folution of green muriate of iron †, not quite five mea-

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* To prevent unneceffary repetitions, after Mr. Cruickfhank, I have called the wire connected with the zinc end of the apparatus, the zinc wire, and that connected with the filver end, the filver wire.

† Solution of green muriate of iron rapidly abforbs nitrous gas, without effecting any change in it at common temperatures, and it is poffeffed of no action on gafes not abforbable by water, and incapable of fupporting flame. Solution of green fulphate of iron likewife abforbs nitrous gas without decompofing it. See *Refearch. Chem. & Phil. concerning Nitrous Oxide, page* 179. *Johnfon.*

fures remained. Hence the 33 meafures of gas evidently contained more than 31 meafures of oxygen. The 61 meafures in the other tube, gave with nitrous gas a diminution but barely perceptible, and fired after the abforption of the nitrous gas by the electric fpark, with 60 meafures of oxygen, left a refiduum nearly equal to 36 meafures; hence the gas was hydrogen almoft pure.

c. There was every reafon to fuppofe, that the flight diminution produced by the mixture of the hydrogen with nitrous gas in the laft experiment, as well as the refidual gas of the oxygen, were owing to common air held in folution by the diftilled water, and given out from it during the procefs. To afcertain if the gafes could be obtained perfectly pure, when water deprived of its loofely combined air by boiling was employed. The two tubes were filled with water that had been boiled for more than eight hours, and that was yet fo hot as to be painful to the fingers; the glaffes were filled with water of the fame kind, and the procefs conducted as before; the tubes being fuffered to cool before the communication was made, gas was given out very rapidly from the water connected with the filver; but very flowly from that connected with the zinc. During the whole of the procefs, no globules of air formed on the fides of the tubes, as in the laft experiment. In five hours the tube connected with the filver contained 56 grain meafures of gas. That connected with the zinc contained only 14 meafures. The 56 meafures gave no diminution with nitrous gas, and appeared by the teft of detonation, to be pure hydrogen. The 14 meafures tried by the tefts mentioned in the laft experiment, appeared to be oxygen, mingled with no perceptible quantity of other gas. In this experiment, as in the laft, the gold wires were not apparently acted upon, nor was their color in the flighteft degree altered; the deficient proportion of oxygen, there was every reafon to believe, was owing to the abforption of that gas in the nafcent ftate by the boiled water. Boiled water was now expofed to, and agitated in oxygen over mercury, till it was judged to be faturated with that gas. The tube connected with the zinc was filled with this water; the other tube was filled with common boiled water. The galvanic procefs was continued feven hours. In this time the water connected with the zinc had given out 27 grain meafures of oxygen, apparently pure; from the water connected with the filver, 57 meafures of hydrogen had been extricated.

d. Having thus afcertained that oxygen and hydrogen, nearly in the proportions required to form water, could be feparately produced from quantities of water, having no communication with each other, except by means of the dry metallic conductors and mufcular fibre. I next endeavoured to afcertain, if the contact of the metallic wires, with the metallic plates of the apparatus, were effential to the effect. The conducting ends, *i. e.* the filver and zinc, were made to communicate with two glaffes of water, by means of two unconnected pieces of mufcular fibre. A piece of filver wire was made the medium of connection between the glaffes. Immediately after the connection, I was furprized by feeing that end of the wire in the water communicating with the filver calcining; whilft gas was given out from that part of it in the veffel communicating with the zinc; as was

the

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the case in the broken circuit described by Mr. Nicholson. When the tubes with gold wires, connected by silver wires, were employed, oxygen was given out in the water connected with the silver, and hydrogen in that communicating with the zinc. In none of these experiments could any production of gas from the muscular fibre be perceived; but the parts exposed to the water became whiter than before.

When (the glasses being connected with the apparatus by muscular fibre) the communication between them was made through my body by means of the gold wires in the tubes; one being in contact externally with my right hand, the other with my left, oxygen was produced as before in the glass connected with the silver, and hydrogen in that connected with the zinc. When I made the communication, holding a silver wire partly plunged into the water connected with the silver, in my right hand, the fingers of my left hand being in the other glass, the silver wire became slowly oxydated, and no gas was perceptibly given out in either of the glasses. When on the contrary, I introduced my hand into the silver glass, and the wire into the zinc glass, gas was given out round the wire, no oxydation took place, and no gas was extricated in the silver glass.

When the glasses were made to communicate both with the machine, and with each other, by means of muscular or vegetable fibre; and metallic wires introduced into either or both of the glasses, and wholly or partially covered with water, no gas was given out from them, and no apparent chemical change took place.

Reasoning on this separate production of oxygen and hydrogen, from different quantities of water, and on the experiments of Mr. Henry, junr. on the action of galvanic electricity on different compound bodies [*], I was led to suppose, that the constituent parts of such bodies (supposing them immediately decomposable by the galvanic influence) might be separately extricated from the wires, and in consequence obtained distinct from each other.

a. I filled two of the small tubes mentioned in *b.* 1. with strong solution of caustic pot-ash, and inverted each of them in a glass filled with the same substance; the glasses were made to communicate with each other by means of muscular fibre, and the gold wires in the tubes connected with the ends of the pile. Gas was produced much more rapidly in this process from both wires, than in the experiment with simple water. In three hours no deposition had taken place in either of the glasses, nor were the gold wires sensibly acted upon. The gas given out in the tube connected with the zinc, measured exactly 37 grain measures, and proved to be oxygen absolutely pure, for with 80 measures of nitrous gas, containing about $\frac{1}{36}$ nitrogen, it diminished to less than 3 measures. The gas given out in the tube connected with the silver, was equal to rather more than 72 grain measures. It gave no diminution with nitrous gas, and two 20 grain measures of it, fired with rather more than one 20 grain measure of oxygen, containing about ,06 nitrogen, left a globule of air hardly perceptible.

[*] Phil. Journ. Vol. IV. p. 224.

Surprized

Surprized at these results, which proved that no decompofition of pot-afh had taken place, and that that fubftance in this mode of operating, only enabled the galvanic influence to extricate oxygen and hydrogen more rapidly from water, I was induced to operate upon this fubftance in the way of direct communication. Two gold wires were paffed through holes in the fide of a fmall glafs tube clofed at one end, and cemented fo as to be diftant about the eighth of an inch. This tube was filled with folution of pot-afh, and inverted in glafs filled with the fame fubftance; the gold wires were made to communicate with the ends of the pile. Gas was produced rapidly from both wires, but moft from the filver wire; the gold was not acted upon, and no depofition took place. When near a quarter of a cubic inch of gas had been collected, it was transferred to a detonating tube, and fired by the electric fhock over mercury, it gave a vivid inflammation, and left a globule of air not equal to $\frac{1}{80}$ of the whole quantity.

b. Solutions of cauftic ammoniac were expofed to the galvanic influence in the two tubes with gold wire, and connected by mufcular fibre. Gas was given out very flowly in the tube connected with the zinc, and the gold wire was evidently acted upon, being in fome parts corroded, and in other parts covered by a yellow depofit. In the tube connected with the filver, gas was given out more rapidly, and the gold was not altered in appearance. In five hours the gas in the zinc tube was equal to 5 meafures, and proved to be a mixture of nearly 3 oxygen, and 2 of nitrogen. The gas in the filver tube meafured 31 grains, and appeared to be hydrogen, mingled with a minute quantity of nitrogen. I repeated this experiment feveral times, to afcertain if after the folution of ammonia had been long galvanized, the proportion between the gafes would be different. The gas given out in the zinc tube was always to that in the filver tube nearly as 1 to 6, but the quantity of oxygen appeared to increafe towards the end of the procefs. A quantity of folution of ammoniac, the fame as that ufed in the experiment expofed to heat in a mode defcribed in *Refearch. Chem. & Phil.* page 241, readily produced $\frac{1}{20}$ of its bulk of unabforbable gas, which gave no diminution with nitrous gas, and appeared to be nitrogen. In every experiment yellow depofit was formed upon the gold in the zinc tube; muriatic acid poured upon fome of this yellow depofit flowly diffolved it, a little nitrogen being given out during the folution.

When a folution of cauftic ammoniac was expofed in the filver tube, and water in the zinc tube, the gold wires were not perceptibly altered, and oxygen was given out in the zinc tube and hydrogen in the filver tube, nearly in the proportions required to produce water. When on the contrary water was connected with the filver, and folution of ammoniac with the zinc, hydrogen was produced from the water; the zinc gold wire was corroded, and the mixture of oxygen and nitrogen to the hydrogen, as fix to one, (one to fix?) was produced as before.

c. Concentrated fulphuric acid was galvanifed in the double tubes connected by mufcular fibre. The gold wire in the zinc tube was not diffolved or corroded, and in a great length of time 41 meafures of gas, which proved to be pure oxygen, were given out from

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it.

it. But little gas was given out in the filver tube, the acid in it was clouded with a white fubftance, which was evidently fulphur; it was perpetually produced round the point of the wire. 15 meafures of gas were evolved, which gave rather greater diminution, fired with oxygen, than pure hydrogen; hence they might poffibly have been partially fulphurated hydrogen.

When folution of cauftic pot-afh was put into the filver tube, and fulphuric acid into the zinc tube, pure hydrogen was difengaged in the filver tube, and pure oxygen in the zinc tube; the fame phenomena took place when water was employed inftead of folution of pot-afh.

When water was connected with the zinc, and fulphuric acid with the filver, the products were the fame as when pure fulphuric acid was ufed in both tubes. When very diluted fulphuric acid was employed in both tubes, oxygen and hydrogen, nearly in the proportions required to form water, were feparately evolved.

d. The tubes were filled with pure folution of muriatic acid, and the communication made as before. No gas was given out in the zinc tube, and the gold in it was very much corroded; 30 meafures of pure hydrogen were collected in the filver tube, the gold in which was not perceptibly acted upon. When water was made to communicate with the zinc, and muriatic acid with the filver, neither of the gold wires were acted upon, and 22 meafures of oxygen were collected from the water, and 41 of hydrogen from the muriatic acid. When water was connected with the filver, and muriatic acid with the zinc, the fame phænomena took place, as when pure muriatic acid was ufed in both tubes.

e. Concentrated folutions of nitric acid were galvanifed in the tubes, 19 meafures of pure oxygen were produced in the zinc tube. A globule of gas only, not equal to half a meafure, was produced in the filver tube, and the acid became green on the top. The globule of gas did not diminifh with oxygen, it was too fmall to be tried by other tefts. The gold wires were not acted upon.

When nitric acid was connected with the zinc, and water with the filver, oxygen and hydrogen were feparately produced. When water was connected with the zinc, and nitric acid with the filver, oxygen was given out from the water; no gas was given out from the acid, and it became green.

In the experiments on the fulphuric and nitric acids, it is moft probable, that the acids were decompofed by the nafcent hydrogen in the filver tube. In the experiments on the muriatic acid and ammoniac, the deficiency of the oxygen in the gold tube moft probably, partly arofe from the oxydation of the gold, in confequence of what may be called predifpofing affinity. In thefe proceffes, none of the compound bodies appear to have been immediately decompofed by the galvanic influence.

The difference between my refults and thofe of Mr. Henry, may be accounted for from the difference between our modes of operating; I fufpect, however, that on repeating his experiment on the folution of pot-afh under new circumftances, that ingenious chemift will find reafons for altering his conclufions with regard to the decompofition of the alkali.

Judging

Judging from the rapidity with which the gafes were extricated, folution of pot-afh would feem to be a better conductor than water, water a better conductor than folution of ammoniac, and folution of ammoniac, better than either of the three mineral acids. Poffibly phofphoric acid, and other acids, may be decompofed when expofed to nafcent hydrogen, produced under the galvanic influence. If the ratio between the quantities of the oxygen and hydrogen produced from the different wires, be always the fame, whatever fubftances are held in folution by the water connected with them, this nafcent hydrogen will become a powerful and accurate inftrument of analyfis.

VIII.

Defcription of the Machine for kneading Dough, which is made Ufe of in the Public Bake-Houfes of Genoa; from a Model prefented to the Patriotic Society of Milan *.

THE drawings in Plate XII. reprefent the machine for kneading; the parts being regularly numbered. 1. A wooden frame upon which the gudgeon of the arbor of the wheel is fupported. 2. A ftrong wall of three and a half palms † thick, through which the arbor paffes. 3. Another wall of the fame kind at the diftance of 21 palms. 4. The arbor, 30 palms long, and $1\frac{1}{3}$ palms thick. 5. A large wheel turning on the arbor. Its diameter is 28 palms, and its width for admitting two men is 5 pounds. 6. Steps within the wheel to afford foot hole for the men. They are $\frac{1}{3}$ of a palm high, and 2 palms afunder. 7. A concrete cog wheel near the end of the arbor, and of the diameter $12\frac{1}{2}$ palms. 8. A bar or beam of wood parallel to the arbor, proceeding from one wall to the other, 21 palms in length, and $1\frac{1}{3}$ in thicknefs. 9. Another beam of the fame dimenfions not vifible, becaufe on the oppofite fide of the arbor; but its relative fituation may be feen in the fhadow upon the wall. 10. A crofs piece near the wall 3, which connects the two pieces 8 together, and fupports the extremity of the arbor. Its length is 14 palms, and its thicknefs $1\frac{1}{4}$. 11. Another crofs piece of the fame defcription near the firft wall, but not vifible on account of the interpofition of that wall. 12. A ftrong curved piece attached to the pieces 8, 9. It receives the pivot of the upright arbor, which carries the lantern pinion and kneaders. Its clear length is 14 palms, and its thicknefs $1\frac{1}{2}$ palms. 13. The lantern pinion, in diameter $5\frac{1}{2}$ palms, and height $1\frac{1}{4}$. 14. The arbor to which is affixed a ftrong crofs of wood. 15. The crofs formed of two bars of wood unequally divided, in fuch a manner as that the four branches are of unequal lengths. They are 1 palm wide, and $\frac{7}{12}$ of a palm thick. 16. Four wooden pieces or kneaders of a triangular form, fixed

* From the Italian of the *Atti della Societa Patriotica di Milano, Vol. II.*

† The Genoefe palm, which is ftated by Dodfon, in his Calculator, at 9,780, Paris fo. inches of our meafure.

in each branch of the crofs in its lower face, fo that they revolve in the trough parallel to the arbor, with one of their edges foremoft.   They are $1\frac{3}{4}$ palms long, and $\frac{1}{2}$ a palm thick. 17. A circular trough, or barrel, made very ftrong, and well hooped with iron.   Its diameter is 6 palms, its clear height $1\frac{1}{2}$ palms, and the thicknefs of the wood $\frac{1}{4}$ of a palm.

The leaven or mixture required to be kneaded, is made in a wooden trough 4 palms long, and 3 palms by the fide in which it is carried to the kneading mill.

The action of this mill is eafily underftood.   When the men walk in the large wheel, they turn the kneaders which divide and agitate the pafte in the trough, and perform the fame effect as is commonly produced by human hands.   The trough contains 18 rubbi (   ) of flour, which being properly mixed and raifed, is carried to the mill to be kneaded, where in a quarter of an hour it is made into excellent pafte.   But the ftate of the dough determines the proper time for taking it out, according to the judgment of the attendant baker; fo that it may take a little more or lefs time, according to circumftances.

IX.

On the Ufe of Caoutchouc in Manufactures, and on the Amelioration of Spirits by Age.   By J. A.

To Mr. NICHOLSON.

S I R,

Clyde, Auguft 19, 1800.

To obferve your attention to the ufeful parts, in preference to the mere curiofities of philofophy, has given me, in common with many others, great fatisfaction: and I fincerely hope your labours are proving ufeful to yourfelf.

In No. 37, of your inftructive Journal, I obferve that Mr. Howitfon, an old acquaintance of mine, has made feveral promifing effays towards obtaining a valuable manufacture by covering cloth with the Eaft Indian caoutchouc.   In confirmation of the practicability of this, I have to mention, that I have had an opportunity of knowing that hats have been covered with it in South America.   Capotas (a kind of cloaks common to the Portuguefe and Spaniards) have alfo been covered with this fubftance; and I have known capotas made in Portugal, and fent to Brazil for this exprefs purpofe.   It is evident that any article of drefs covered with caoutchouc muft be perfectly impervious to wet; but the weight of the capotas (which it was faid the manufacturers could not remedy) was fo enormous, that they could only be ferviceable as boat cloaks.

I have often remarked that fpirits, particularly Weft India rum, is univerfally admitted to improve as its age encreafes; but I do not find that this has been completely and philofophically accounted for: therefore, through the medium of your monthly publication, I fhould be glad to know the refult of any refearch that may have been made into the

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nature of this change. Your obfervations on this fubject, and perhaps thofe of fome of your correfpondents who have directed their attention this way, cannot fail to be very interefting and ufeful to the Weft India merchants, and will be highly gratifying to

SIR,

Your humble fervant,

J. A.

P. S. As expofure to the atmofphere has been found to produce a premature old age in rum, it has been prefumed that it was caufed by evaporation ; but if this was the cafe, reducing the ftrength of the fpirit with water ought to have an effect nearly fimilar. Whatever may be loft by expofure, I have formed the idea, and am clear in it (though I have only the vague evidence of the fenfes, without any chemical experiment on my fide) that a great part of the improvement fo obfervable arifes from fome abforption. At firft I thought oxygen was abforbed ; but the increafe of a certain oily appearance does away this idea. It has alfo occured that (agitation promoting the improvement) the particles of liquor may undergo fome more intimate combination in confequence of fomething fimilar to friction. The particles of old liquor having been kept long in clofe contact, may have been by that means more affimilated. Thefe ideas are, however, quite crude, and deferve little attention.

The changes which take place in ardent fpirits and the other products of fermentation by keeping, have not formed the fubject of direct inveftigation. How far the flow re-action of principles, the extrication of effential oil by fubfidence or floating, the abforption or combination oxygen from without, or of tannin or gallic acid from the cafk, &c. may be concerned, can only be collected from enquiry among intelligent dealers in thefe articles. I fhall take the firft opportunities of doing this, and ftating the refults.—W. N.

X.

Abftract of a Memoir on a Method of ufing the Syphon for raifing Water in the Machine of C. Trouville. By Citizen PRONY [*].

To a tube A B (Plate IX. fig. 4.) of any form, are adapted a number of vertical branches ; one of them terminates in a refervoir E, filled with water, and diftinguifhed by the name of the great afpirator, and the others lead to refervoirs C, full of air, named fmall afpirators. Each fmall afpirator communicates by its tube, fuppofed to be filled with a

[*] Bulletin de la Societé Philomatique, H. 92.

refervoir

refervoir beneath, which is open, and filled with water; and there is a valve opening upwards at the lower end of each of thefe tubes. The air within is of the fame denfity and temperature as that without; but the external communication of this included air is prevented by the fides of the engine and the water which occupies part of its interior.

The great afpirator, the fmall afpirators, and the refervoirs, placed beneath thefe laft, have all the form of vertical prifms; the fmall afpirators are equal and fimilar, and the portions of tube comprized between the afpirators and their refervoirs are of equal length.

Things being in this fituation, we are to fuppofe that the water of the great afpirator flows out by the orifice *f*, and it is required, 1. what will be the depreffion of the water when the fpring of the included air, and the charge of the water on the orifice of emiffion, fhall be equal to the weight of the atmofphere; 2. the quantity of water which, at this period, fhall have rifen in the fmall afpirators; 3. the requifite dimenfions of the apparatus, in order that the water emitted from the great afpirator, plus that raifed in one of the other afpirators may be a determinate part of the water which a fpring of known product affords in a day; and, 4. the proportions of the fame apparatus which fhall give the greateft ratio between the fum of the volumes raifed in all the fmall afpirators, multiplied by their elevation, and the volume of water emitted from the great afpirator, multiplied by its height.

Formulæ for the Solution of the foregoing Problem.

Let $Q =$ the volume of water afforded by the fpring in one day.

$\gamma =$ the number of times which this volume of water contains that which flows from the great afpirator from the inftant of its being filled to the ceffation of its efflux.

$c =$ the height of the great afpirator.

$b =$ the common length of the portion of pipe comprized between a fmall afpirator and the refervoir, whence its water is drawn.

$n =$ the number of fmall afpirators.

$s =$ the horizontal fection of each fmall afpirator.

$\iota =$ the horizontal fection of the lower refervoir, from which the fmall afpirator draws the water.

$S =$ the horizontal fection of the great refervoir.

$V =$ the capacity of the interior parts of the machine, which are at no time occupied by water, but conftantly hold air.

$\lambda =$ the height of the column of water which meafures the total preffure of the atmofphere.

$x =$ height of the water above the orifice of emiffion of the great afpirator, at the moment when the fpring of dilated air within, and the charge of water on the orifice of emiffion, are equal to the weight of the atmofphere.

$y =$ the correfpondent depreffion of the upper furface of the water in the great afpirator.

$z =$

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s = the correfpondent height of the prifm of water, which penetrates into any one of the fmall afpirators.

Making $\frac{t}{s} = K$; $K + 1 = m$; $V m + S m (\lambda + c) + n s \lambda = A$, we have

$$\dot{x} = \frac{A \pm \sqrt{A^2 - 4 S m \lambda (n h s + S m c)}}{2 S m};$$

and affuming the equation $\psi = \frac{\lambda}{\lambda - x}$, we obtain the following:

$$y = c - x = \frac{\lambda - (\lambda - c) \psi}{\psi},$$

$$z = \frac{x - h}{m} = \frac{\lambda (\psi - 1) - h \psi}{m \psi}$$

$$\psi = \frac{\lambda}{\lambda - h - m z} = \frac{\lambda}{\lambda - c + y}$$

$$h = \frac{(\lambda - m z) \psi - \lambda}{\psi}$$

which relates to the vertical dimenfions. With regard to the horizontal, making $\alpha = \frac{V}{n s z}$, we have,

$$s = \frac{Q - \gamma S y}{\gamma z} = \frac{Q}{\gamma z (1 + n [\alpha (\psi - 1) + \psi])}$$

$$S = \frac{Q - \gamma s z}{\gamma y} = \frac{z}{y} \, n s (\alpha (\psi - 1) + \psi).$$

Laftly, if we fuppofe the prifms which we have called great afpirators, to have their height = the value of z here found; the ratio between the product of the height of the great afpirator by its expenditure, and the product of the mafs of the water raifed by the height to which it is raifed, namely, $\frac{c}{h} \cdot \frac{S y + s z}{n s z}$, being denoted by Φ, we have the formula.

$$\Phi = \frac{c}{h} \left\{ a (\psi - 1) + \psi + \frac{1}{n} \right\}.$$

If from Φ the term $\frac{c}{n h}$ be retrenched, the furplus will be the ratio between the moving caufe and the effect produced; and the lefs this ratio differs from unity, the more will the effect approach the maximum of value of which it is capable. It is requifite, in order that this maximum of value fhould take place, 1. that the excefs of m beyond unity, and the height fhould be diminifhed, as much as the poffibility of execution will allow; and, 2. that ψ fhould be determined by the equation.

$$\psi = \frac{\lambda}{\lambda - m z} \left\{ 1 \pm \sqrt{\frac{\lambda + a m z}{(a + 1) \lambda}} \right\}.$$

The

The theory of the machine of Cit. Trouville is deduced from the preceding formula, suppofing the fpring to have a fall equal to the height of the great afpirator, to which it fupplies water; and that the firft fmall afpirator draws from the fpring, and empties itfelf into the refervoir of the fecond afpirator; that this difgorges into the refervoir of the third, and fo forth; all thefe effects producing each other by appropriate machinery.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Klaproth's Analytical Effays.

IT will undoubtedly afford much fatisfaction to the cultivators of philofophical chemiftry, to be informed, that a tranflation of the above work, by the learned editor of *Gren's Principles of Modern Chemiftry*, is in the prefs, and will fhortly appear.

Multiplication of Engravings in Relief.

Mr. George Gray, of Newcaftle upon Tyne, has prefented me with fome fpecimens of a very ufeful application, of the art of taking impreffions in foil, or leaf metal. They confift of three blocks; the firft about three inches fquare, containing fixteen lines of pica italic, formed by impreffing a leaf of tin foil upon the face of the printers types, fet up in the ufual manner. The tin foil is ftrengthened by plaifter of Paris poured into the concavity, to the thicknefs of a little more than half an inch. This block is not as fharp as the type itfelf; but affords impreffions on paper perfectly legible, and about as neat as common ballads are ufually printed. The fecond is an impreffion taken in brafs foil, from a wood cut of *Bewicke*, $2\frac{1}{4}$ inches long, and near $1\frac{1}{2}$ inch broad. A folution of fal ammoniac was applied to the concave fide, and lead then poured in, which confequently adhered, and gave the requifite ftrength. It is very fharp, but the face has been bended in taking off; fo that it is not eafy to print from it. The third is a fmall figure of a fhip, within a bounding line of three quarters of an inch fquare. It is made in the fame manner with brafs foil, and though the engraving abounds with fine ftrokes, it affords an impreffion which could not, I think, be diftinguifhed from another taken from the original.

It may perhaps be true, that this art is rather inferior to the method of moulding and cafting, defcribed at page 63 of the fecond Volume of this Journal. But it requires lefs of preparation, as well as of fkill, and with fome improvements in the foil, the manipulations and the ftrengthening, may probably exceed the other method in precifion and effect.

Rational

Rational Toys.

I give the following from a paper tranfmitted to me :

It requires but flight obfervation to be fatisfied of the utter inutility of the articles with which the Toy-fhop is ufually replenifhed. And on clofe reflection, it may poffibly appear neither ftrained nor fevere to condemn moft play-things as worfe than ufelefs. Their gay appearance, and the movements which they are fometimes contrived to perform, doubtlefs raife ftrong and fudden defires in the mind of children. But fatiety as quickly follows. The flufh of delight, arifing from the firft impreffion, cannot but be tranfitory ; and no fooner does the little poffeffor examine into the ftructure of his new acquifition, than he flings it afide, or breaks it to pieces and tramples it under foot, as if to revenge himfelf upon it for belying the promife of its exterior.

This fucceffion of longing and loathing is a more ferious evil than may at firft be apprehended. If it be true, that youthful curiofity cannot be frequently baulked with impunity, every fuch difappointment may be confidered as fome advance towards dullnefs.

We often meet with a fpecies of toy calculated to excite furprize. This, if not liable to the fame objection as the unmeaning toy, may be fufpected of foftering a difpofition for petty ftratagems, by which a connection between pleafure in the individual who plays them off, and pain in others is almoft inevitably eftablifhed.

Ten years ago, the idea of fubftituting models of machines in the place of ordinary toys, fuggefted itfelf to one of the perfons whofe names are fubjoined to the prefent paper.

Every quality, he conceived, which diftinguifhes models, would fecure them againft neglect and deftruction, the merited fate of toys. The knowledge conveyed by the mutual dependency of the parts, and by the purpofe of the whole, would be laid up with advantage and might be revived with pleafure. Whatever improvement the underftanding derives from mathematics, would more agreeably flow from well-conftructed models. And mathematics would be ftudied with more fuccefs by children accuftomed to fuch models. They would roufe the faculty of invention, and confer the habit of purfuing trains of thought to a great extent. To girls, by conveying information without awakening their fenfibility, they would be particularly ferviceable. From this ftatement, the utility of a fet of models in fchools and private families is obvious. It is equally obvious that their utility would not be confined to young people.

This fcheme has been generally approved by thofe to whom it has been mentioned. Different perfons, long fince, offered to advance money towards its execution. Indeed, in 1796, it was partially carried into execution, when the whole defign was announced under the title of Rational Toys, in a letter prefixed to Mr. Donne's explanation of his elementary mathematical models. Towards its compleat execution, however, there was wanting a perfon well informed concerning machines, and of ready mechanical invention. This difficulty is now removed by the offer of Mr. Robert Weldon, to conduct a manufactory of Rational Toys.

It is however, requifite that a fum of money fufficient to fet it on foot be advanced ; and to fatisfy fubfcribers (as far as fuch a circumftance may afford fatisfaction) that they are

likely

likely to receive an adequate return for any fum they may advance, and alfo out of regard to Mr. Weldon, the perfons, named below, have agreed to act as a committee of fuperin-tendance.

Soon after a fufficient fum fhall have been fubfcribed, Mr. Weldon will furnifh the fub-fcribers with a lift of models and prices, from which each may choofe to the amount of his fubfcription. A defcription and drawing will accompany the models; and they will be contrived fo as to take to pieces, and its name will be fixed to each part, where this is practicable.

Hereafter, if the undertaking profpers, the public may be accommodated with fingle models. But at prefent to fpare the committee's time, it is propofed:

I. That no fubfcription of lefs than 10l. be received.

II. That every fubfcription not exceeding 10l. be immediately advanced. Larger fub-fcriptions may be paid by inftalments of 10l.

Thefe terms may ftartle many, even opulent, parents. And it is true that a fet of inftructive models will require a greater facrifice than has been cuftomary in education. But as public opinion on this fubject is daily becoming more liberal, it may begin to be felt that of all poffible methods of confulting happinefs, to fubtract information for the fake of adding to fortune, is the moft prepofterous. A few additional hundred pounds, well laid out upon the boy, may more improve the condition of exiftence than the mines of Potofi, bequeathed to the man. The ill-advifed niggardlinefs of parents is one principal caufe why genius has fo feldom been exerted upon the means of early inftruction, and why the moft difficult of all occupations—that of forming the fyftem of thought and feeling—has been fo often confided to perfons, whofe endowments are barely adequate to the rudeft mechanical labour.

COMMITTEE.

THOMAS BEDDOES, M.D. Clifton.

JOHN BILLINGSLEY, Efq. Afhwick-Grove.

WM. CLAYFIELD, Efq. Briftol.

BENJ. HOBHOUSE, Efq. M.P.

JOHN H. MOGGRIDGE, Efq. Boyce-Houfe.

JAMES STEPHENS, Efq. Camerton-Houfe.

JOHN WEDGWOOD, Efq. Cote-Houfe.

WM. WYNCH, Efq. London.

Subfcriptions are received by

SAVERY and Co. Bankers, Briftol.

HOBHOUSE and Co. Bankers, Bath.

WM. REYNOLDS and Co. Bankers, Salop.

SIR JOHN CALL PYBUS, Bart. and Co. Bankers, London.

SIR JAMES ESDAILE and Co. Bankers, London.

*** Letters (poft-paid) addreffed to Mr. WELDON, Pneumatic Inftitution, Briftol, will be duly attended to.

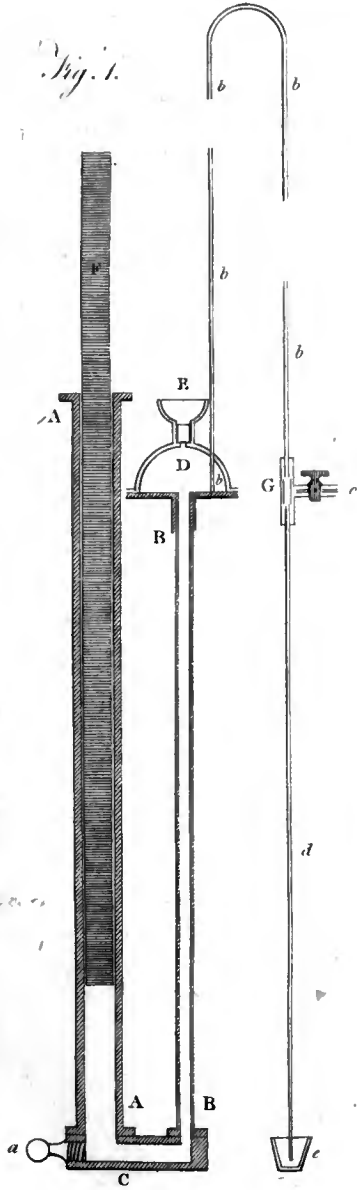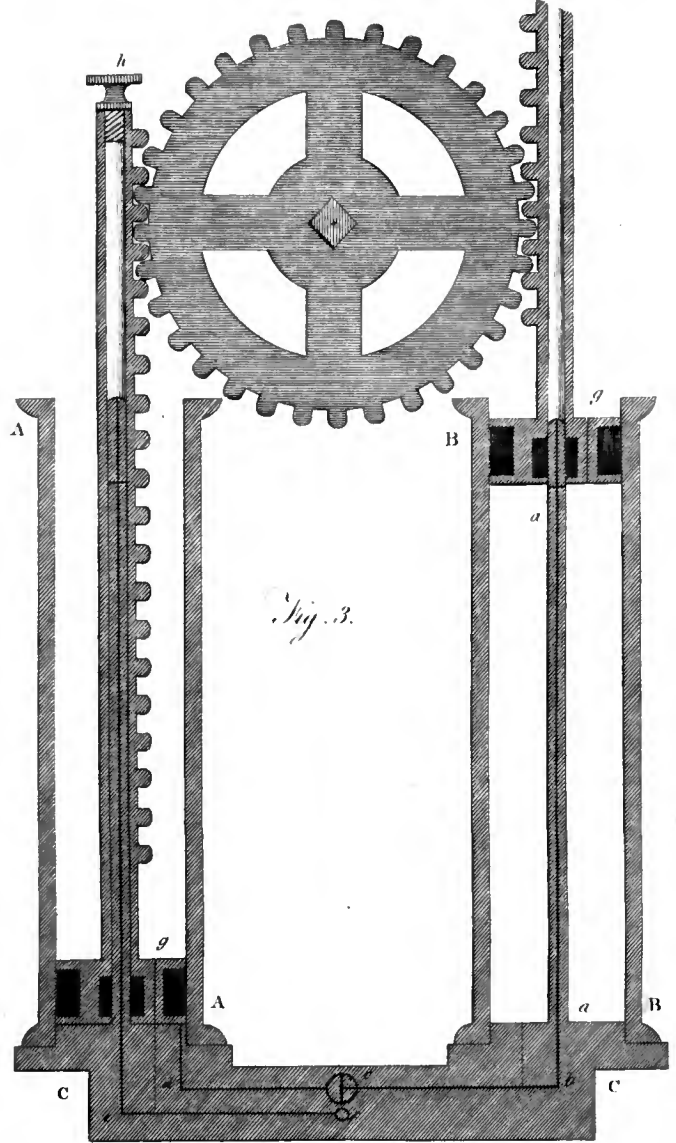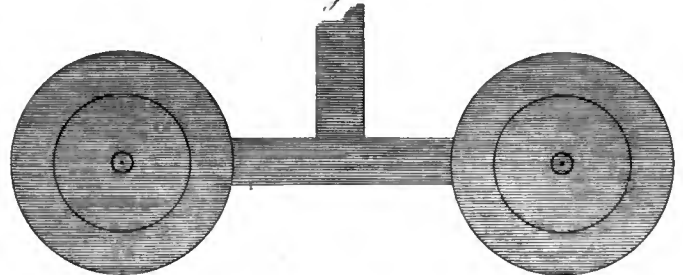Mercurial air pump.

Double barrelled air pump.

Fig. 1.

Fig. 2.
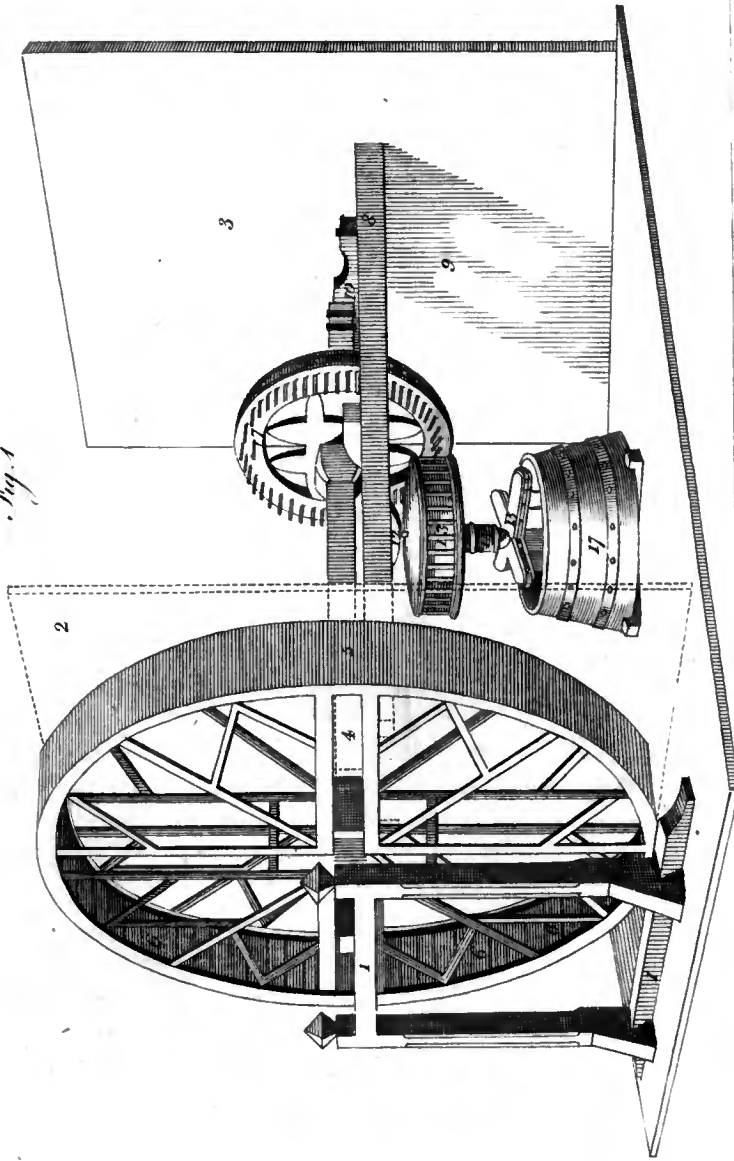
Fig. 3.

Fig. 4.

Kneading Machine.

Fig. 1

Fig. 2

A

J O U R N A L

O F

NATURAL PHILOSOPHY, CHEMISTRY,

A N D
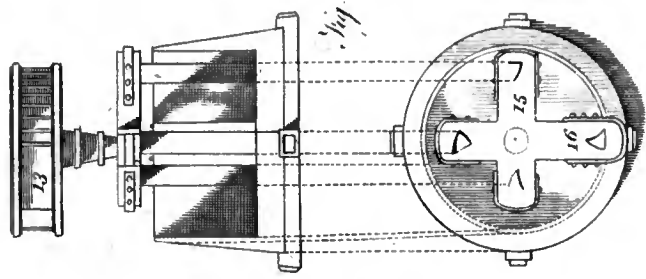
THE ARTS.

ARTICLE I.

A Project for extending the Breed of Fine-wooled Spanish Sheep, now in the Possession of his
MAJESTY, into all Parts of Great Britain, where the Growth of fine Clothing Wools is
found to be profitable*.

AFTER experiments had been tried for several years, by the King's command, with
Spanish sheep of the true *Merino* breed, imported from various parts of Spain, all of which
concurred in proving, that the valuable wool of those animals did not degenerate in any
degree in this climate, and that the cross of a *Merino* ram, uniformly increased the quan-
tity and meliorated the quality of the wool of every kind of short-wooled sheep, on which
it was tried, and more particularly so, in the case of the South Down, Hereford, and
Devonshire breeds; his Majesty was pleased to command, that some *Merino* sheep should
be procured from a flock, the character of which for a fine pile of wool, was well
established.

Application was accordingly made to Lord Auckland, who had lately returned from an
embassy to Spain; and in consequence of his lordship's letters, the Marchioness del

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* This paper was drawn up by the Right Hon. Sir Joseph Banks, Bart. P. R. S. &c. at whose expence
it has been lately circulated.

Campo di Alange was induced to present to his Majesty five rams and thirty-five ewes, from her own flock, known by the name of *Negretti*, the reputation of which, for purity of blood and fineness of wool, is as high as any in Spain : for this present, his Majesty was pleased to send to the Marchioness in return eight fine English coach horses.

These sheep, which were imported in the year 1792, have formed the basis of a flock, now kept in the park of his Royal Highness the Duke of York at Oatlands, the breed of which has been preserved with the utmost care and attention.

The wool of this flock, as well as that of the sheep procured before from Spain, was acknowledged by the manufacturers who saw it, to be to all appearance of the very first quality, yet none of them chose to offer a price for it, at all equal to what they themselves gave for good Spanish wool, left, as they said, it should not prove in manufacture so valuable as its appearance promised; it became necessary, therefore, that it should be manufactured at the King's expence, in order that absolute proof might be given of its actual fitness for the fabric of superfine broad cloth ; and this was done year after year, in various manners, the cloth always proving excellent; yet the persons to whom the wool was offered for sale, still continued to undervalue it, being prepossessed with an opinion, that though it might not first degenerate, it certainly sooner or later would alter its quality, much for the worse.

In 1796 it was resolved to sell the wool at the price that should be offered for it, in order that the manufacturers themselves might make trial of its quality, although a price equal to its real value should not be obtained; accordingly, the clip of that year was sold for 2*s.* a pound, and the clip of the year 1797, for 2*s.* 6*d.*

The value of the wool being now in some degree known, the clip of 1798 was washed in the Spanish manner, and it sold as follows :

The number of fleeces of ewes and wethers was 89 ;

Which produced in wool washed on the sheeps' backs -	295 lb.
Loss in scowering - - - -	92
Amount of scowered wool - - -	203

Which produced, Raffinos, 167 lb. at 5*s. per* lb. ⎱
            Finos,   23,   at 3*s.* 6*d.*   ⎬ 47*l.* 8*s.*
            Terceros, 13,   at 2*s.* 6*d.*   ⎰

The clip of 1799 was managed in the same manner, and produced as follows :
The number of fleeces of ewes and wethers was 101 ;

Which produced in wool, washed on the sheeps' backs -	346 lb.
Loss in scowering - - - -	92
Amount of scowered wool - - -	254

Which produced, Raffinos, 207 lb. at 5*s.* 6*d. per* lb. ⎱
            Finos,   28,   at 3*s.* 6*d.*   ⎬ 63*l.* 14*s.* 6*d.*
            Terceros, 19,   at 2*s.*   ⎰

The

The ram's wool of the two years forted, together produced as follows :

Quantity of wool, waſhed on the ſheeps' backs   -   314 lb.

Loſs in ſcowering   -   -   -   -   99

Amount of ſcowered wool   -   -   -   215

Which produced, Raſſinos, 181 lb. at 4*s*. 6*d*. *per* lb. ⎫
            Finos,   22,   at 3*s*. 6*d*.   ⎬ 45*l*. 15*s*. 6*d*.
            Terceros, 12,   at 2*s*.   ⎭

It is neceſſary to account for theſe extraordinary prices by ſtating, that in the year 1799, when both ſales were effected, Spaniſh wool was dearer than it ever before was known to be ; but it is alſo proper to add, that 5*s*. 6*d*. was then the price of the beſt Spaniſh piles, and that none were ſold higher, except as is ſaid, a very ſmall quantity for 5*s*. 9*d*.

The King has been pleaſed to give away to different perſons, who undertook to try experiments, by croſſing other breeds of ſheep with the Spaniſh, more than one hundred rams and ſome ewes : in order, however, to make the benefit of this valuable improvement, in the ſtaple commodity of Great Britain, acceſſible to all perſons who may chooſe to take the advantage of it, his Majeſty is this year pleaſed to permit ſome rams and ewes to be ſold, and alſo to command that reaſonable prices ſhall be put upon them, according to the comparative value of each individual ; in obedience to which it has been ſuggeſted, that five guineas may be conſidered as the medium price of a ram, and two guineas that of a ewe ; a ſum which it is believed the purchaſer will in all caſes be able to receive back with large profit, by the improvement his flock will derive from the valuable addition it will obtain.

Though the mutton of the Spaniſh ſheep was always excellent, their carcaſes were extremely different in ſhape, from that mould which the faſhion of the preſent day teaches us to prefer ; great improvement has however been already made in this article, by a careful and attentive ſelection of ſuch rams and ewes as appeared moſt likely to produce a comely progeny : and no doubt can be entertained, that in due time, with judicious management, carcaſes covered with ſuperfine Spaniſh wool, may be brought into any ſhape, whatever it may be, to which the intereſt of the butcher, or the caprice of the breeder, may chuſe to affix a particular value.

Sir Joſeph Banks, who has the honor of being intruſted with the management of this buſineſs, will anſwer all letters on the ſubject of it, addreſſed to him in Soho-ſquare. The rams will be delivered at Windſor, the ewes at Weybridge in Surrey, near Oatlands.

As thoſe who have the care of his Majeſty's Spaniſh flock, may naturally be ſuppoſed partial to the project of introducing ſuperfine wool into theſe kingdoms, it has been thought proper to annex the following notice, in order to ſhew the opinion held of a ſimilar undertaking, in a neighbouring country, where individuals, however they have miſtaken their political intereſt, are rather remarkable for purſuing and thoroughl weigh-

ing

ing their own perfonal advantage, in all their private undertakings, and for fagacity in feizing all opportunities of improving, by public eftablifhment, the refources of their nation.

FRENCH ADVERTISEMENT.

On the 24th of May laft, an advertifement appeared in the *Moniteur*, giving notice of a fale of two hundred and twenty ewes and rams of the fineft wooled Spanifh breed, part of the flock kept on the national farm of Rambouillet; alfo two thoufand pounds of fuperfine wool, being the prefent year's clip of this national flock, and one thoufand three hundred pounds of wool, the produce of the mixed breeds of fheep kept at the Menagerie at Verfailles.

This advertifement, which is official, is accompanied by a notice from Lucien Buonaparte, Minifter of the Interior, as follows:

"The Spanifh breed of fheep that produce the fineft wool, introduced into France "thirty years ago, has not manifefted the fmalleft fymptom of degeneration: famples of "the wool of this valuable flock, which was brought from Spain in the year 1786, "are ftill preferved and bear teftimony, that it has not in the leaft declined from its "original excellence, although the diftrict where thefe fheep have been kept, is not of "the beft quality for fheep farming; the draughts from this flock, that have been "annually fold by auction, have always exceeded in value the expectation of the "the purchafers, in every country to which they have been carried, that is not too damp "for fheep.

"The weight of their fleeces is from fix * to twelve pounds each, and thofe of the "rams are fometimes heavier.

"Sheep of the ordinary coarfe wooled breeds, when croffed by a Spanifh ram, produce "fleeces double in weight, and far more valuable, than thofe of their dams; and if this "crofs is carefully continued, by fupplying rams of the pure Spanifh blood, the wool of "the third or the fourth generation, is fcarce diftinguifhable from the original Spanifh "wool.

"Thefe mixed breeds are more eafily maintained, and can be fattened at as fmall an "expence, as the ordinary breeds of the country.

"No fpeculation whatever offers advantages fo certain, and fo confiderable to thofe "who embark in it, as that of the improvement of wool, by the introduction of rams "and ewes of the true Spanifh race, among the flocks of France, whether the fheep "are purchafed at Rambouillet, or elfewhere; in this bufinefs, however, it is of the "greateft importance to fecure the Spanifh breed unmixed, and the utmoft precaution "on that head fhould be ufed, as the avarice of proprietors may tempt them to fub-"ftitute the croffed breeds inftead of the pure one, to the great difappointment of the "purchafer.

* This muft mean fleeces unwafhed, or in the yoke, as it is technically termed.

"The

" The amelioration of wool at Rambouillet has made fo great a progrefs, that in a
" circle from twenty-four to thirty-fix miles in diameter, the manufacturers purchafe
" thirty-five thoufand pounds of wool, improved by two, three, or four croffes. Thofe
" who wifh to accelerate the amelioration of their flocks by introducing into them ewes
" of this improved fort, may find abundance to be purchafed in that neighbourhood at
" reafonable rates."

II.

*Defcription of an Engine for raifing Water by the lateral Motion of a Stream of Water
through a conical Tube In two Letters from Mr. WILLIAM CLOSE.*

LETTER I.

TO MR. NICHOLSON.

S I R,

Dalton, Aug. 25, 1800.

PART of this town being fituated upon a dry eminence of lime-ftone, the inhabitants
experience confiderable fatigue and inconvenience in providing the quantity of water
which is indifpenfibly neceffary for culinary and domeftic purpofes. There are excellent
fprings in the neighbourhood, and at a fmall diftance, if taken in a direct line, but the
neareft road to return is fteep and incommodious. This fituation has frequently turned
my attention to the contemplation of hydraulic machinery to remove the inconvenience,
and I doubt not it might eafily be done by various methods, as a ftream of confiderable
force runs very near the fprings, and the height to which water might be raifed with
confiderable advantage is not great: but this is not the object of my communication.

The experiments of Citizen Venturi on the lateral motion of fluids, induce me to think
that an hydraulic engine, operating by the preffure of the atmofphere, and in this refpect
fimilar to that excellent one invented by H. Goodwin, Efq. * may be brought into action
by this principle. With this view I have made fome experiments, and endeavoured to
reduce my ideas into the form of an engine, which I expect to fee much improved, and
of which I fend you a drawing and defcription.

A A, in Plate XIII. Fig. 1, reprefents a refervoir of water, kept conftantly full, at the
fame time that the jet B is running full under a confiderable preffure.

C a tube fixed into the bottom of the fpherical veffel D, and paffing up to fome height
above its middle.

E another tube fixed into the bottom of D, and ending near its top. The lower part of
this tube is bended, and the extremity of it is introduced into the fmaller aperture of the
conical tube B.

F a pipe to empty the veffel D, when filled with water.

* Philof. Journal, IV. 163.

3

G

G a fmall tube paffing through the tube F, and rifing to near the top of D, for ad-miffion of air to quicken the defcent of water out of that veffel. Both thefe tubes are clofed at their lower ends by the lever L, which is fixed upon a cock in the tube E, and has a weight upon one end, in order that the other end may bear againft the openings of the tubes, at the fame time that it fupports the veffels I, which is fufpended upon it by a cord or wire. The upper furface of that end of the lever which comes in contact with the tubes, muft be covered with leather, and muft prefs againft them with confiderable force, that the veffel D may be perfectly tight when the ends of the other two tubes are placed in water.

H a fmall ciftern, provided with a fyphon, to be filled with water from the refervoir A, at the fame time with D; this muft be done by regulating the cock *k*, upon the pipe which fupplies H with water.

I a fmall veffel and fyphon fufpended from the lever L.

The operation of the engine will be as follows:

The refervoir A being conftantly full of water, and the conical tube B completely filled at its wider end by the water which runs out of A; the force of the lateral motion being encreafed by the form of the tube B, and acting upon the end of E, will rarify the air in D, and the preffure of the atmofphere upon the furface of the water in the refervoir, will caufe part of that water to rife in the tube C to run over its top and fill D, then to defcend through E, and enter the ftream which flows out at B.

When D is full of water, if the tubes F and G are opened, the water will run out; therefore, foon after D is full, H is full alfo, and the fyphon of this laft veffel beginning to empty the water, fills I, which overbalances the weight upon the lever L, and opens the pipes F and G, and clofes the cock in E: the column of water in C immediately defcends into the refervoir, and if the fmall tube G be full of water, it will be emptied by the defcent of that column, admit air into D, and the water will flow out at F. The veffels D and H will be empty of water about the fame time, and the veffel I, by its fyphon foon after: the weight upon the lever L will then clofe the tubes F and G, open the paffage through E, and the water will rife into D as before.

If the water fhould defcend through E, before F and G are opened, it will render the cock in E more tight.

Experiment 1. The veffel D was conftructed of a cylindrical form, one inch and three quarters in diameter, and two inches in height, without the tubes F and G, but with two holes, one to let out water, and the other to admit air, both clofed with wooden ftoppers. The tube C was three-eighths of an inch infide diameter, and the tube E, one-fifth of an inch in its internal diameter: this laft was placed within the tube C for readinefs of con-ftruction, its upper end was flanted off, and touched the top of D, and it was confined in its place by a wedge of wood thruft into the bottom of C. A conical tube, nine inches long, one inch diameter at its fmaller end, and one inch and three quarters at its wider end, was fixed to the fide of a refervoir eight inches deep. The depth of the water in the

4   refervoir

refervoir above the fmaller aperture of the conical tube being fix inches and a half, and the water which ran through that tube completely filling its wider extremity; the crooked end of the tube E was introduced into the fmaller aperture of the conical tube, while the bottom of C was two inches below the furface of the water in the refervoir; part of the water immediately rofe fourteen inches in C and filled D; the tubes C and E were then lifted out of the water, and D was emptied by taking out the ftoppers. The experiment was frequently repeated.

Experiment 2. The veffel D was made fpherical, two inches diameter, the fame tubes C and E adapted as before. The two tubes F and G were added to this, and clofed by the finger of the operator when water was raifed. The tube E being lengthened a little, was placed within the fmall aperture of B, when the charge of the refervoir was feven inches. The bottom of the tube C was juft below the furface of the water. In a very fhort time part of the water rofe near fixteen inches through C, and filled D; the tubes *f* and *g* were then opened, and this water ran out.

From feveral experiments, I think water will not rife more than fixteen inches under fimilar cirtumftances.

Upon the admiffion of air into D, the ftream of the jet diminifhed fo much, that it did not half fill the wider aperture, and it was long after the tubes F and G were clofed before it could recover its full force. This fhews the neceffity of a cock or valve to clofe E, to prevent the diminution of the jet, when air is admitted into D, if we wifh to make the engine raife much water, and empty itfelf at ftated times. For no water will rife fo long as the conical tube is not full. If a cock be perfectly air tight, it will anfwer the purpofe very well.

Fig. 2 points out a method of conftructing a valve for this purpofe: the veffel D, in this figure, is cylindrical, the fmall tube *g* opens very near the top, under that letter, and the dotted line *n* paffes through the middle of F. The tube C and E are explained before. *m* is a valve which, by its weight, clofes the upper aperture of E, when the lever L opens the tubes F and *g*, and which immediately before it clofes thefe pipes it raifes again: the communication may be made by a piece of ftrong wire fixed into *m*, and which, paffing through *f*, will reft upon the lower valve or lever L. This wire is reprefented by the dotted line *n*.

It perhaps might be advantageous to have a valve in C to fupport the column of water in that tube alfo.

The power of the engine feems to be very confiderable when conftructed upon the fimple plan mentioned in the experiments; it feems very defirable to determine the beft form of the conical tube, and the method to throw moft force upon the rarifying tube, fo as to produce the greateft effect under a given preffure of water in the refervoir. Citizen Venturi has determined the form of a tube to produce the greateft expenditure*, and it feems likely that it may alfo be the beft form for this purpofe.

* Philof. Journal, II. 274. This figure is adapted to a certain definite preffure only, and muft be varied for other depths.---N.

The lateral motion of a current of air will, to a certain extent, produce the same effect as a current of water, but to what height water may be raised in this way, I have hitherto made no experiment to determine. The power of an instrument in the form of Mr. Boswell's ventilator * is very considerable when made upon a small scale, if the small tube be forcibly blown through while the end of the vertical tube is placed in water, one expiration will cause the water to rise in it several inches.

<div style="text-align:center">

I am, SIR,

Your's, &c.

WILLIAM CLOSE.

</div>

<div style="text-align:center">

LETTER II.

</div>

SIR,

HAVING constructed the model of an engine upon the preceding principles, on the 29th of August last, I set it to work, and kept it in action for several hours.

The rarefying tube E, fig. 1. was placed within the ascending water tube C, until they had both entered the spherical vessel; they were then divided, and the end of the water tube was used as the support of a metallic valve, which, by its weight, closed the opening of the tube E. To one side of this last tube was fixed another small one, which opened into the external air by the side of the pipe to let out water, and both these were closed by the end of the lever L, or lower valve. This small tube, at the same time that it served for admission of air, to hasten the descent of water, served also to contain the wire which raised the upper valve, *n*, fig. 2.

The lower valve was covered with leather, and when it was well moistened, and the engine had been in action for some time, I could perceive no fault.

The method of making the engine work itself, which I adopted in imitation of the plan proposed by Mr. Boswell †, answered very well: the lower valve was opened between six and seven times in a minute, and water always flowed out; the higher valve completely prevented the diminution of the stream through the conical tube, and when it was lifted by the closing of the lower valve, no difference could be perceived, although a little air was admitted at each rise.

The descending branch of the syphon in the higher vessel H, was made of considerable length, to prevent a constant dribble, and to make the reciprocation end at once. The syphon of the lower or descending vessel was made full as wide as the other, in order that the weight might preponderate quickly, and close the lower valve immediately after raising the higher. The upper vessel was hung upon the side of the reservoir, and supplied with water over the edge by a syphon, one aperture of which was made of a proper width. The capacity of the lower vessel was about five ounces; the higher about seven; but

<div style="display:flex; justify-content:space-between">

* Philos. Journal, IV. 4.

† Philos. Journal, IV. 117.

</div>

<div style="text-align:right">was</div>

was sometimes diminished, by introducing a solid body into its upper part, to make it fill sooner. Weight, rather above six ounces. Length of lever, one foot; it had holes in its under edge to hang the vessel and weight at different distances from its centre. The rise of the water above the surface of water in the reservoir was one foot. I adopted this height for convenience. There was no valve to prevent the column from descending every time the lower valve was opened.

This particular description may appear tedious, but it will save some time and consideration in the repetition of such an experiment.

I entertain no doubt respecting the operation of an engine of this kind, and that a column of water may be raised to any height, not much exceeding thirty feet, by proportionally encreasing the pressure of water in the reservoir, and the dimensions of the conical tube: in many situations, however, the requisite quantity for this purpose cannot be had, others may not admit of sufficient descent, and in some a reservoir so deep may be dangerous and inconvenient.

Where the stream has sufficient descent, the water may be raised by a number of *lifts* instead of one.

Suppose 1, 3, 5, fig. 3. to be three reservoirs with conical tubes, and 2, 4, 6, three cisterns, through which the raised water is carried in succession by a stream passing through the three reservoirs, and acting upon three engines in the following manner:

The cisterns 2 and 4 being filled with water, the current through the reservoir 1 will raise a quantity of water into its engine, which will be emptied into the cistern 2, at the same time the reservoir 3 raises an equal quantity out of 2 into 4, and 5 another out of 4 into 6.

The same method may be used when there is plenty of water, but not convenience for a deep reservoir; in this case the conical tubes may be fixed to different parts of the reservoir, and all upon the same level. Each engine must be provided with a lever and weight, to work its own valves; but they may be all opened at the same time by the descent of one vessel connected to all the levers, or each by a vessel connected to that only.

To quicken the reciprocations of the engine, and encrease the quantity of raised water, a valve may be made to support the column of water in the ascending tube; if the column be not heavy, this valve may be contained in a chest at the bottom of the tube: it will be easy to repair in this place; a piece containing it may be fixed on with screws: it will rise by the pressure of the water below.

This kind of machinery, by altering the position of the rarefying tubes, will raise water from a depth below the stream equally as well as to an height above it; and in situations where there is plenty of water and convenience for a reservoir, a lower body of water may be conveyed into a stream above by the help of a single tube only; one end of it being placed in the water to be raised, the other must be introduced into the smaller

aperture of the conical tube adapted to the reservoir. A constant stream will then rise so long as water below can supply the tube*.

I am,

SIR,

Your humble servant,

Dalton, Sept. 1, 1800.   W. CLOSE.

My acknowledgements are due to Mr. Close for his obliging private postscript. In reply to his doubt, left from the number of hydraulic inventions which have appeared in this Journal, his contrivance might not be inserted; it seems proper to observe, that I consider hydraulic works as of the highest importance to society, whether we look to a supply of the first necessity for consumption, or to the advantageous uses of neglected, though valuable, first movers; that these works must in most cases be modified by localities and other circumstances; and, consequently, that the most useful practical knowledge will not consist in an acquaintance with one or more of the best engines, but with that great variety of happy contrivances which reading and enquiry must point out. From reasons of this sort, when exercising my duty as editor of a work rendered respectable by the correspondence and favor of learned and ingenious men, I have been little solicitous of the abstract comparative merits of the works I receive. If they contain useful facts, clear deductions, or new prospects tending to lead others to improvements in science or the arts, they are certainly well calculated to form part of this miscellany.—N.

III.

On double Images caused by Atmospherical Refraction. By WILLIAM HYDE WOL- LASTON, *M. D. F. R. S*†.

IN some of the last volumes of the Philosophical Transactions, there have been related many instances of strong atmospherical refraction, by which objects seen near the horizon have appeared inverted, and the horizon itself either elevated or depressed.

Mr. Huddart first took notice of a distant image, inverted beneath the object itself; and, in the Philosophical Transactions for 1797 ‡, has described several such appearances, accompanied with an optical explanation, wherein he shews, that the lowest strata of the air were at the time endued with a weaker refractive power, than others at a small elevation.

* See the experiments of Venturi on the lateral motion of fluids, before quoted.—C.
† Phil. Transf. 1800, p. 239.
‡ Or this Journal, I. 146. See also 225, and elsewhere, as by the index.

In the volume for 1799, Mr. Vince has given an inftance* (tab. 1. fig. 1.) where erect as well as inverted images were vifible above, inftead of beneath, the objects themfelves; and by tracing the progrefs of the rays of light, in a manner fimilar to Mr. Huddart's, concludes that thefe phenomena arofe from " unufual variations" of increafing denfity in the lower ftrata of the atmofphere.

In the volume for 1795, Mr. Dalby mentions having feen " the top of a hill appear " detached, for the fky was feen under it." In this cafe, as well as in the preceding, it is probable that reverfion took place, and that the lower half of the portion detached was an inverted image of the upper, as the fky could not be feen beneath it, but by an inverted courfe of the rays.

Since the caufes of thefe peculiarities of terreftrial refraction have not received fo full an explanation as might be wifhed, I have endeavoured,

1ft. To inveftigate theoretically the fucceffive variations of increafing or decreafing denfity to which fluids in general are liable, and the laws of the refractions occafioned by them.

2dly. To illuftrate and confirm the truth of this theory, by experiments with fluids of known denfity.

And laftly, to afcertain, by trial upon the air itfelf, the caufes and extent of thofe variations of its refractive denfity, on which the inverfions of objects and other phenomena obferved, appear to depend.

The general laws may be comprized in three propofitions.

Prop. 1. If the denfity of any medium varies by parallel indefinitely thin ftrata, any rays of light moving through it *in the direction of* the ftrata, will be made to deviate during their paffage, and their deviations will be in proportion to the increments of denfity where they pafs.

For each ray will be bent towards the denfer ftrata, by a refracting force proportioned to the difference of the denfities above and below the line of its paffage; and as their velocities are the fame, and therefore the times of action of the forces equal, the deviations will be at the refracting forces, *i. e.* as the increments of denfity.

Prop. 2. When two fluids of unequal denfity are brought into contact, and unite by mutual penetration; if the denfities at different heights be expreffed by ordinates, the curve which terminates thefe ordinates will have a point of contrary flexure.

For the ftraight lines *d a*, *r n* (Plate XIII.) Fig. 1. which terminate the ordinates *r x*, *d y*, of uniform denfity, will be parallel, and, if not united by contrary curvature, fome ftraight line of union, as *a o* muft be fuppofed. But, from whatever caufe the firft line *a o* is inferred, by the fame caufe other intermediate lines *m p*, *t q*, &c. will be produced, and curves *d e f m*, *m t s r*, will be ultimately formed, having a point of contrary flexure at *m*.

The form of the curve does not appear to admit of accurate inveftigation, nor is it of importance to the fubfequent reafoning, if wholly unknown. We may, however, form

* Ibid. II. 140.

fome

fome judgment of its nature; for, whether the denfities depend on different fpecific gravities of different fluids, or on unequal temperatures of different portions of the fame fluid, the curves will be nearly alike.

In each of thefe cafes, to whatever fmall diftance *p c*, Fig. 2, the mutual attraction is fufficient to occafion intimate union of the fluids, the denfity *m n* of the mixture will be an arithmetic mean; and, for the fame reafon, at any intermediate fmaller diftances, there will be a feries of arithmetic means *e f*, *g h*, &c. interpofed, and the line *a o*, uniting the ordinates, will be ftraight.

By progreffive effect of this attraction, and fucceffive interpolations, in Fig. 1, curves *d e f m*, *r s t m*, will be formed, of which the ftraight lines *m p*, *t q*, &c. are tangent.

The attracting denfities *n p*, *o q*, &c. are fubtangents; and if it be admitted that thefe are every where equal, the curves fo produced are logarithmic, and the increment of the ordinate greateft at *m*, where they meet.

Prop. 3. If parallel rays pafs through a medium varying according to the preceding propofition, thofe above the point of contrary flexure will be made to diverge, and thofe below the fame point will converge, after their paffage through it.

For fince the deviation of each ray depends on the increment of denfity where it paffes, and fince the increment of denfity is greateft at the point of contrary flexure, any rays, as *a b*, Fig. 3, paffing near to that point, will be refracted more towards the denfer medium than thofe at *c d*, which move in a higher ftratum, and will diverge from them, but will be refracted towards and meet thofe at *e f*, which pafs nearer to the denfer medium, where the increments of denfity are alfo lefs.

Cor. Hence adjacent portions of the converging rays will form a focus, beyond which they will diverge again; and the varied medium will produce effects fimilar to thofe caufed by a medium of uniform denfity * having a furface fimilar to the curve of denfities, fince convergence or divergence will be produced, according as the curve of denfities is convex or concave; confequently, by tracing backwards, to the extremities of an object, the progrefs of the vifual rays (or axes of the pencils received by the eye) it will be manifeft that,

Any object feen through the inclined concave part *r m*, Fig. 5, would appear elevated, erect, and fomewhat diminifhed.

An object feen through *m d*, where it is convex and inclined, would be elevated; and, if fituated beyond the focus of vifual rays from the eye, it would appear inverted. The magnitude would depend on the relative diftances of the eye and object.

* In the varied medium, *b c* and *b m*, Fig. 4, the correfponding increments of the abfcifla and ordinate, are to each other as radius to the tangent of the angle *c*. Therefore, the tangent of deviation, which is as the increment of the ordinate, varies as the tangent of the angle *c*.

So alfo, in the uniform medium, fince the fines of refraction and incidence are in a given ratio, then differences will bear a given ratio to either of them; and, when the angles are fmall, the tangent of deviation will vary as the tangent of incidence, or as the tangent of the angle *c*, which is equal to it.

Below

Below the point *d*, where the curve terminates, vifion would be direct, fo than an object might be fituated fo as to be feen in all three ways at the fame time, direct at O, inverted at I, and erect again at A.

I confider the foregoing propofition as applicable to all cafes of varying denfity, whether occafioned by mutual folution of different fluids, or partial rarefaction of the fame fluid; and, by trial of various fluids, however different in denfity, or even in vifcidity, I find that the refractions obferve a law agreeable to the theory, as will appear by the following experiments:

Experiment 1. Into a fquare phial containing a fmall quantity of clear fyrup, I put about an equal quantity of water, in fuch a way that it floated on the furface of the fyrup without mixing. For a fhort time, the ftratum of union was fo thin that nothing could be diftinctly feen through it. But foon, by mutual penetration of the water and the fyrup, the effects reprefented at A, Fig. 6, were produced.

Through the fyrup, a word written on a card, placed behind, was feen erect, and in its place; through the adjacent variable medium, an inverted image was vifible above the true place; and alfo, above that, a fecond image of the fame object appeared erect.

When thefe appearances are firft difcernible, the variations of denfity are fo great, that the object to be looked at may be in contact with the phial; but when the variations of denfity become more gradual, and thereby the focus more diftant, any object fo near is only elongated, and require to be removed an inch or two, to be feen inverted.

Exper. 2. Over the furface of the water, in the fame phial, I next put about the fame meafure of rectified fpirit of wine.

At the ftratum where the water and fpirit united, the appearances were the fame; but fince the refractive power of fpirit exceeds that of water, the true place of the object was feen uppermoft; the inverted and erect images are below. Fig. 6, B.

When an oblique line *d e r* is viewed through any variable medium fo made, it appears bent into different forms, according to its fituation with refpect to focal diftance.

If it be at the diftance of the principal focus, one point of it is dilated into a vertical line, as *l m*. Fig. 7, A.

If beyond that focus the portion *l m* is inclined backwards, being an inverted image of *d l*; and *m n* is another image of the fame portion feen erect. Fig. 7, B.

On this account, it becomes a convenient object for afcertaining the ftate of any medium under examination.

In thefe experiments, the appearances continue many hours, even with fpirits of wine; with fyrup, two or three days; with acid of vitriol, four or five; with a folution of gum arabic, much longer; but although their difpofition to unite is fo different, the appearances produced are neverthelefs the fame in all.

The refraction is greateft nearly in the plain of original contact of the fluids, diminifhing from thence both upwards and downwards. The exact rate of diminution above or

below

below this point I had no means of meafuring with the acccuracy that would be requifite for determining the nature of the curves of denfity formed according to the firft propofition.

But the truth of the fecond propofition appeared capable of confirmation by experiment; for the deviation of a ray is there faid to depend on the increment of denfity, and time of the ray's paffage, jointly; accordingly, the deviation caufed by a given increment fhould be in proportion to the extent of the medium.

In order to try what effect a greater extent of medium would produce,

Exper. 3. I made a rectangular glafs veffel, of which the fides were in the ratio of 10 to 30,6; and having put into it fome clear fyrup, with water on its furface, I meafured the greateft refractions through it in both directions, and found them in the ratio of 10 to about 29.

In another veffel, of which the fides were as 10 to 40,4, the refractions were, on an average, as 1 to 4.

Being now fully fatisfied of the effect of different fluids, I made the following experiment, whereby it appears, that the variations of denfity occafioned by difference of temperature between adjacent ftrata of the fame fluid, follow a fimilar law.

Exper. 4. Having put fome cold water into a fquare veffel, I covered its furface with a piece of writing paper perforated with a few fmall holes, and then filled the veffel cautioufly with boiling water. The paper nearly prevented any mixture of the hot and cold water; but, by floating gradually up, left them to communicate their heat by contact alone.

While they were in this ftate, I examined the appearance of remote objects through the varied medium, and found, that when my eye was removed four or five feet from the veffel, the effects were the fame as in the preceding experiments with different fluids; above any object viewed through the cold water, I could diftinguifh two images of it, the one inverted, the other erect, as ufual; but thefe appearances did not continue more than five or fix minutes.

Having thus eftablifhed, by experiments fufficiently varied, that the contiguity of two fluids of unequal denfity is capable of occafioning all the appearances that have been obferved, I fhall proceed to fhew by what means the air may be made to exhibit fimilar phenomena.

Exper. 5. I heated a common poker red hot, and held it fo as to look along the fide of it at a paper 10 or 12 feet diftant. The rarefaction occafioned by it caufed a perceptible refraction to the diftance of about $\frac{3}{8}$ of an inch from the fide of the poker. A letter feen more diftant from it appeared as ufual; within that diftance there was a faint image of it reverfed; and ftill nearer to the poker was a fecond image direct, and as diftinct as the object itfelf, but fomewhat fmaller, as in Fig 8, in which a fection of the atmofphere furrounding the poker is reprefented. At the bottom and fides it is nearly circular; but upwards the circular form is loft in undulations, occafioned by the rapid afcent of the rarefied air.

The

The greatest deviation produced in this case measured about half a degree.

Exper. 6. By a red hot bar of iron, 30 inches long, the refractions were much greater, the extreme deviations amounting to full $1\frac{1}{4}$ degree.

The refractions observed in these experiments may at first view be thought greater than could be caused by difference of temperature alone, being in one instance more than double the greatest horizontal refraction of the heavenly bodies; in which case, as the rays enter from a vacuum, the greatest possible effect of the atmosphere might be expected. But it must be remembered, that when a star appears in the horizon, its rays intersect the superior strata of the atmosphere at an inclination of several degrees, and that they pass but once through the variations from rarity to density; but, on the contrary, that in the experiments with red hot iron, the rays may pass actually in the direction of the strata, and that they are refracted not only by their entrance from the denser into the rarer medium, but the effect is doubled, since the refraction caused by their emergence is equal to that produced by their incidence.

Although a stratum of air, heated by these means to so great a degree, affords an erect, as well as an inverted, image of objects seen through it, the more moderate warmth communicated to it from bodies heated by the action of the sun upon them, seems insufficient to produce both images; but the inverted image may generally be seen when the sun shines upon a brick wall, or other darker coloured surface.

While the eye of the observer is placed nearly in a line with the wall, if another person, at 30 or 40 yards distance, extends any object towards the wall, an image similar to it will appear to come out to meet it.

It would be difficult to ascertain with accuracy the degree of rarefaction capable of shewing this appearance, but it may be of some use to future observers to mention the different degrees of heat which I observed.

In one instance, a thermometer, in contact with the wall, stood at 96°; but, at $\frac{3}{8}$ of an inch distance, 82°.

One morning, when the sun shone bright, I examined the temperatures and refraction produced at the surface of a deal bar painted green, about eight feet long.

A small thermometer, in contact with the bar, rose to 96°; at $\frac{1}{4}$ of an inch distance, it stood at 73°.

The refraction at the same time exceeded 20 minutes.

To explain why red hot iron occasions two images, while solar heat produces but one, I imagine that the intense heat in the former case rarefies the air for some small distance uniformly, and thereby affords the same series of variations as between other fluids of uniform density; but that, in the latter, the heat is conveyed off as as fast as it is generated; so that, as there is no extent of medium uniformly rare, the densities corresponding to the concave portion *r m*, Fig. 5, of the curve before described do not take place, but the phenomena occasioned by the convex part *m d* are alone produced.

It

It muſt be remarked, that the vertical poſition of the ſurface contributed greatly to encreaſe the effect; for, ſince the heated air riſes in the direction of the ſurface, its aſcent has in this caſe no tendency to blend it with the adjacent denſer ſtrata, and hence very different degrees of denſity take place in the thickneſs of $\frac{1}{4}$ of an inch; ſo that, as the increments of denſity are great, the refractions will be proportionally ſo; but where the heated ſurface is horizontal, the aſcent of the rarefied air into the ſuperincumbent denſer ſtrata renders the variations far more gradual; conſequently, a heated ſurface of far greater extent muſt be requiſite to produce equal refraction.

However, over extenſive plains, when the ſun ſhines, ſome degree of inverſion is very frequently to be ſeen; but the inverted images are rarely well defined, unleſs over a very even ſurface. One of the beſt ſituations for this purpoſe is over a level open road, with a gentle breeze blowing acroſs it. A current of air brings a cool ſtratum more cloſely in contact with the heated ſurface; and, ſince refraction depends on the increment or difference of denſity in a given ſmall pace, a very moderate breeze will thereby render inverſion more perceptible; but a ſtrong wind will reduce the temperature of the ſurface, and may make the heated ſtratum too thin for any object to be ſeen through it from a diſtance.

In one inſtance, when I ſaw a refraction of about 9 minutes, at the diſtance of about $\frac{1}{3}$ of a mile, a thermometer in the ſand was 101°; at 4 inches above, 82°; and, at 1 foot above, 76°.

Over water, the evenneſs of the ſurface is favourable to the production of ſuch appearances; but, ſince the action of the ſun is weak on a body ſo tranſparent, a far greater extent of ſurface is requiſite to produce any perceptible inverſion.

Being at Bognor one bright morning, when the ſea was calm, I had an opportunity of obſerving the appearance of Selſea Bill, about ſix miles diſtant. The whole extent of coaſt, when viewed with a pocket teleſcope magnifying about ſixteen times, appeared inverted from one end to the other; and the lower part of a brick houſe upon the ſhore was ſeen as diſtinct as the houſe itſelf. I judged the quantity of refraction, in this caſe, to be about two minutes of a degree.

This ſtate of atmoſphere appears to be not very uncommon; for, at Shanklin Chine, in the Iſle of Wight, a few days preceding, ſimilar appearances were viſible in ſeveral directions, but I neglected to make any eſtimate of the quantity of refraction.

In the inſtance of the inverted veſſel ſeen by Mr. Huddart, (Phil. Tranſ. for 1797, Tab. 1, Fig. 3.) at the diſtance of eight miles the refraction ſeems to have been about 3′.

All the appearances deſcribed by him, I am inclined to think, aroſe from difference of temperature alone. He offers a conjecture, that evaporation might occaſion the lower ſtrata of the atmoſphere to have a weaker refractive power; but from the following experiments it ſeems to have a contrary effect:

Exper. 7. I took a plate of glaſs, and, while I looked along the ſurface, I poured upon it a ſmall quantity of ether.

A line

A line upon the opposite wall appeared instantaneously elevated many minutes, and at times above half a degree.

This fluid being the most volatile, and most soluble in the atmosphere, of any known liquid, produces the greatest effect; since the cold, during evaporation, conspires with the ether diffolved in the air to increase the refractive power.

Rectified spirit of wine also produces, from the same cause, a very considerable effect.

Exper. 8. By moistening a board, five feet in length, with alcohol, and obferving the elevation of an object viewed over its furface, I found the refraction to be 15′.

Exper. 9. I next made a fimilar experiment with water itself. Of this, the effect was barely vifible, when tried in the same way; but, by means of a furface of ten feet, and by viewing a luminous point at a greater distance, the refraction became evident, and the object elevated above three minutes.

In the course of these experiments, I tried whether confining the faturated atmosphere, by boards on each fide, would vary the effect, and found the refraction in all cafes much leffened; and when water was ufed, it became imperceptible; but as foon as the boards were removed, and a free current allowed to pass acrofs, the full effect was again produced. The reafon of this difference appears to be, that the quicker evaporation increafes the degree of cold, and the current brings greater differences of denfity contiguous.

The state of rapid evaporation will fully account for the phenomenon witneffed by Mr. Latham, who has defcribed (in the Phil. Tranf. for 1796, p. 357; or this Journal, II. 417) an extraordinary elevation of the oppofite coaft of France, fo as to be feen from the beach at Haftings, and other parts of Suffex.

There is a fact of the same kind stated by De la Lande (*Aftron.* Tom. II.) who fays that the mountains of Corfica (though at the distance of more than 100 miles) are occafionally vifible from Genoa.

It is probably owing to the fame caufe, that other objects have been fometimes feen, at fuch distances that we should expect them to be intercepted by the curvature of the earth; for it is evident, that whenfoever the evaporation over each mile of furface occafions a refraction of about one minute, the rays receive a curvature equal to that of the ocean, fo that its furface will appear flat; and the fpherical form of the earth will not obftruct horizontal vifion of objects at any diftance.

It still remained to explain the phenomena feen by Mr. Vince, as I had not hitherto made an atmosphere capable of exhibiting images inverted, as well as elevated, by increafed denfity. For, in the refractions produced in the 7th, 8th, and 9th experiments, by evaporation at an expofed furface, I obferved the effect was always greateft in contact with the evaporating furface; any lower point *a*, Fig. 9, appeared brought nearer to a higher point *c*, by the pencil of rays from *a* being more refracted at *b*, than the pencil from *c* was refracted at *d*. Therefore, any rays paffing from the eye

at *e,* as a point, through *b* and *d,* would be made to diverge to *a* and *c*; consequently, visual rays could not, under these circumstances, intersect each other, and no objects could appear inverted.

But whenever the lowest strata of the air become saturated with moisture, the variations between the saturated stratum, and the incumbent atmosphere of the common density, will follow a law similar to what is found at the confines of other fluids of unequal density; hence, inversion will become visible, as there will be a point below which the increment of density will decrease, and where the refractions will consequently be less, although through a denser medium.

Exper. 10. To produce these appearances, I procured a trough of thin deal, five feet long, one inch wide, with sides $2\frac{1}{2}$ inches high, and closed the extremities of it with glass. A section of it is given in Fig. 10.

When the bottom was wetted with ether, the greatest refraction was, at intervals, more than $\frac{3}{4}$ of an inch from the bottom of the trough; and, beneath this height, I saw a second image inverted, when my eye was removed to 14 or 15 feet distance, and the object was about 70 feet. The focus seemed at the same time to be about nine feet distant.

There was not depth enough of uniformly saturated atmosphere for the object itself to be seen at through it, but its true place, compared with that of the images, is represented at *a.*

Exper. 11. When I made use of rectified spirit in the same apparatus, I had also sufficient proof that the laws of evaporation would admit of such appearances being produced; for the same object now appeared curved downwards, as in Fig. 11, so that rays nearer to the bottom were manifestly less refracted than such as passed at some distance above. A degree of convergency must therefore have been produced, although the distance at which the rays would meet was beyond that of my eye, and circumstances would not admit of my removing beyond 35 feet.

The evaporation of water could not be expected to produce any sensible effect of this kind in so short a space; but in a view of some miles extent, there can be no doubt, from the foregoing experiments, that evaporation from the surface of the sea, in such a state of the atmosphere as would allow the lower strata to be saturated, is capable of occasioning all the phenomena which have been described, and probably was the cause of those which Mr. Vince observed.

Since heat alone tends to depress objects, and evaporation produces apparent elevation, it is probable, that in the instance of refraction related by Mr. Dalby (Phil. Transf. for 1795, p. 587) the heat of the sun was the principal agent, and that the moisture rather tended to counteract than assist its action.

Simple inversion may generally be seen when the sun shines upon a *dry* even road of $\frac{1}{3}$ or $\frac{1}{2}$ mile extent; but when the ground has been wet, I have very rarely seen it, and have even failed of discerning it, when the heat has been sufficient to raise a stream from the ground.

4

The

The following experiment shews that it is not to be expected but by very great extent of surface.

Exper. 12. I placed a dark coloured board in the fun shine, and having examined the refraction along its surface, I made a wet line along it, with a sponge dipped in boiling water. Notwithstanding this additional heat, the refraction, in the direction of the wet line, was far less than over the rest of the board, although I took care to observe the effect before the surface could be cooled again by evaporation.

I should therefore expect the depression of the horizon at sea where the refraction occasioned by heat must always be counteracted by evaporation, never to exceed a few minutes; and that any one in a situation commanding a view of the sea, by attention to the various degrees of the dip of the horizon under different circumstances, might soon form some estimate of the proper allowance to be made for brightness of the sun at the time of an astronomical observation, or for difference of temperature between the sea and air.

Having now examined the several peculiarities of refraction which I proposed for consideration, I shall, in few words, recapitulate the purport of the foregoing pages.

According to the theory here given, there appear to be two opposite states of the atmosphere, either of which may occasion objects to be seen doubled or tripled, since both increase and decrease of its density, when partial, produce the same effects.

It has been explained,

1st. Why air heated by the moderate warmth of the sun's rays occasions objects to appear doubled and inverted.

2dly. Why rarefaction, by a higher degree of heat, gives an additional image which is not inverted.

3dly. In what state of evaporation the increase of the air's density brings distant objects into view by unusual elevation.

4thly. Under what circumstances evaporation may also produce an inverted image less elevated.

And it is probable that the same reasoning will afford a ready explanation to other varieties of terrestrial refraction, that may have been, or may hereafter, be observed.

A Memoir

IV.

A Memoir on the Nature of the colouring Principle of the Lapis-lazuli. By Citizen GUYTON*.

THE blue ftone known by the name of *lapis-lazuli* has long been an object of chemical refearch, with a view more particularly of difcovering that colouring matter which is the caufe of its high price, from the luftre which it gives to works in which it is employed, and on account of the preparation of ultramarine blue, which is fo highly efteemed as a pigment.

This colour was at firft attributed to copper. The celebrated Margraf demonftrated the error of this opinion, but he found that it confifted merely of filiceous earth, fulphate of lime, lime, and a fmall quantity of iron. Others, fince his time, have fuppofed that oxide of cobalt formed a part of it, whilft others, like Rinman, imagined the fluoric acid to be one of its ingredients. A more ftrict examination foon deftroyed thefe conjectures.

The methods of analyfis having of late years been carried to a degree of perfection beyond the moft fanguine expectations, it was natural to think that thofe chemifts who were the moft fkilful in this new art, would not neglect the application of them to the folution of this important queftion. Amongft others, I fhall refer to Mr. Klaproth, whofe works have fo much enriched the chemiftry of mineral bodies, and who has paid a particular attention to every kind of foffil of a blue colour.

In 1784, he publifhed fome experiments, which demonftrated that what is called *native Pruffian blue*, which is found in peat grounds, and is often of a white colour before it is expofed to the air, is indebted for its colour to nothing elfe than a combination of iron and phofphoric acid †.

At Vorau, in Auftria, another mineral remarkable for the fame colour was difcovered, and was fucceffively taken for fmalt, or native blue oxide of cobalt, for another kind of native Pruffian blue, and for a blue oxide of copper. It is afcertained by the examination of the celebrated chemift of Berlin, that it contains only filiceous earth, alumine, and iron, and though he found that it refifted the action of the fire in a lefs degree than the lapis-lazuli, he thinks it might be claffed among its varieties if it likewife contained lime ‡.

This laft conclufion proves that M. Klaproth had, with his ufual accuracy, determined the conftituent parts of the true lapis. He, in fact, points them out in the tenth article of

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* Read before the philofophical and mathematical clafs of the National Inftitute of France 6 Pluviofe, An. 8, and inferted in the Annales de Chimie, XXXIV. 54.

† Chemifche Annal. 1784, page 396.

‡ Beytrage zur Kentnifs der Mineral Korper, etc. Tom. I. p. 197.---Annales de Chimie, XXI. p. 144. (or this Journal, I. 77.)

his

his refearches into mineral fubftances, where it is obfervable that the pureft lapis, which is called oriental, is a compofition of 46 in the 100 of filiceous earth, 28 of carbonate of lime, 14.5 of alumine, 6.5 of fulphate of lime, 3 of oxide of iron, and 2 of water.

He therefore corrected the analyfis of Margraff by adding alumine to it, which the other had not noticed, and which more than fifteen years ago I had demonftrated to be contained in it, by touching a plate of the lapis with fulphuric acid, which, at the end of feveral hours, left very regular cryftals of alum, which are preferved upon it.

But by what principle can oxide of iron be tinged with blue without being combined either with Pruffic acid, or the acid of phofphorus ? M. Klaproth replies that he cannot tell *.

The experiments, of which I fhall now give an account, appear to me to folve this important queftion; but before I explain the proceffes and their refults, I muft, for greater perfpicuity, take notice of fome operations which led the way to this conclufion.

In the year 1780, whilft I was examining a well at Montolier, on the road from Dole to Poligny, in fearch of coal, I found, at the depth of 35 metres, a bank of gypfum, containing zones of a fine and very lively red. I gave a defcription and an analyfis of it in the Journal de Phyfique for the month of December in that year, and I inferred from my trials that it was a fulphate of lime coloured by the oxide of iron.

The late difcovery of feveral new metallic fubftances, fome of which have the property of affording colours of great intenfity, led me to think that the foffil of Montolier deferved a re-examination with the view of fearching for one of thefe oxides. For this purpofe, at the beginning of the prefent year, I appropriated a piece to thofe experiments of the mineral analyfis which make a part of my courfe in the polytechnical fchool. They have been purfued with no lefs accuracy than intelligence by M. Deformes, formerly a pupil and operator at the laboratory of the fecond clafs.

After having afcertained that this mineral does not contain any carbonate of lime, ten grammes of it, reduced to a powder, were ignited in a crucible, by which treatment the colour became deeper, and changed to a yellow brown. There was a lofs in weight of 22.3 per cent.

A. Muriatic acid was repeatedly digefted on the 77.7 parts remaining after calcination; they did not become difcoloured, and the acid only deprived it of a very minute portion of iron.

B. The refidue was then boiled in a folution of carbonate of pot-afh, and a very fmall part only has yet been decompofed.

C. What remained was mixed with charcoal, and treated in the crucible. A fulphuret being formed, it was decompofed by the muriatic acid, which feized the lime and iron. The filiceous earth remained mixed with the fuperabundant coal.

* Beytrage, &c. I. 201.

D.

D. The liquids of the three preceding operations were put together, and then divided into two equal portions. From the firſt the iron was ſeparated, and was found to weigh 10.5, and it was aſcertained that it contained no other earth except lime. From the ſecond, eight decigrammes or hundredth parts of lime, were obtained.

The whole of the ſolution, therefore, contained:

Oxide of iron	-	-	21.
Lime	-	-	16.
Sulphuric acid	-	-	29.1

the laſt according to the proportions determined by Klaproth.

E. It remained to examine the portion of earth left by the acid, and which was mixed with the ſuperfluous coal. It is here that the operations began to preſent unexpected phenomena, and which, by deviating from the uſual courſe, adviſe the attentive chemiſt that he is on the eve of diſcovery.

This reſidue was at firſt calcined in the open air, in order to burn the coal, but the ſiliceous earth remained black: its weight was 16.5.

It was treated with pot-aſh in a crucible of platina, and yielded a fuſible maſs of a ſuperb blue colour.

Water poured upon it aſſumed the ſame colour.

Nitric acid made it totally diſappear.

The ſiliceous earth ſeparated by evaporation to dryneſs weighed only 8.6.

F. It became of conſequence to know the effect of the different re-agents on the acid which had been applied to the maſs fuſed in the crucible, which might diſcover the ſubſtance which afforded its properties. The following is the reſult of theſe trials:

1. With pruſſiate of pot-aſh this fluid yielded a precipitate of a yellowiſh green colour, which the addition of acids cauſed inſtantly to diſappear inſtead of reviving the blue.

2. With the gallic acid there was no precipitate.

3. With ſulphurated hydrogen there was no precipitate. This, indeed, was to be expected upon the ſuppoſition that iron alone was preſent, but it was proper in this manner to exclude the other metallic ſubſtances which are precipitated by this re-agent.

4. With hydro-ſulphuret of ammoniac there was a fine green precipitate.

5. With ammoniac there was a white precipitate.

6. With pot-aſh there was a light blue precipitate.

The two laſt colours changed, in the drying, to a yellow.

Some comparative experiments were made at the ſame time with a ſolution of nitrate of iron, and the reſult of them was totally different.

G. That there might remain no doubt reſpecting the nature of the ſubſtances by whoſe preſence the reſults of theſe operations was affected; ſyntheſis was called in to the aſſiſtance of analyſis. Sulphuret of iron was prepared in the direct way; a ſufficient quantity of nitrous acid was poured upon it; the filtered liquid was diluted with much water, in order that the exceſs of acid might no longer precipitate the ſulphurated hydrogen

which

which was poured upon it. In this ftate the fame re-agents, which had been employed in the former experiments prefented the fame phenomena. It might be imagined that fulphate of unoxided iron would, in the fame circumftances, afford fimilar effects; but pruffiate of iron afforded only a white precipitate, as had been announced by Profeffor Prouft.

H. It was now eafy to draw a conclufion refpecting the analyfis of the red fulphate of lime of Montolier. It confirmed the obfervation I had made that it contained nothing but oxide of iron, and determined befides the quantities of its conftituent parts, which are as follows:

Sulphuric acid - - -	29.1
Lime - - -	16.
Oxide of iron - - -	21.9
Siliceous earth - - -	8.6
Water driven off by the firft calcination -	22.3
Lofs - - - -	2.1
	100.0

The termination, however, of this fubject opened a path to new enquiries, of which it is now time to give an account.

I. From obferving the blue colour which the fulphate of lime of Montolier had affumed by the addition of pot-afh, the effects of the re-agents upon the acid, in which its iron is held in folution, and more particularly the difappearance of the green precipitate formed by pruffiates by the addition of an acid, M. Deformes immediately recollected that he had obferved phenomena perfectly fimilar in fome experiments performed during laft year with Citizen Clouet upon the lapis-lazuli. This fact already afforded a ftrong indication that the ftone in effect contains no other colouring metallic oxide than iron. It remained therefore only to purfue the coincidence of the facts in all their circumftances, to determine the particular ftate in which this metal is found when it produces this beautiful blue compofition. Some experiments added to the obfervations already known upon the properties of the lapis-lazuli will form this connection.

The lapis-lazuli may be urged to a red heat, and even lofe 0.2 of its weight, without any vifible alteration in its colour; but with a ftronger heat, fuch as that of the furnace of an enameller, its colour changes to grey. By ftill encreafing the intenfity of the fire, it is reduced to a brownifh glaffy fcoria, with a diminution of from 10 to 11 hundredths of its weight.

L. When the lapis is pulverized, a fmell of mufk is fometimes perceived, which alumine and magnefia equally afford when they are united with a little fulphur.

M. The lapis is difcoloured with more or lefs quicknefs by the three acids which are called mineral. The nitrous acid acts almoft inftantaneoufly; the muriatic acid is lefs fpeedy; and the fulphuric acid acts the moft flowly. Thefe acids often difengage the fame fmell as is produced by trituration.

If

If the nitrous acid be concentrated, it affords nitrous gas, and sometimes when the lapis contains carbonate of calx, carbonic acid gas. The liquid, when tried by the prussiates, affords a precipitate whose colour resembles that of Prussian blue, but visibly inclining towards a green which is destroyed by acids. The hydro-sulphuret of ammoniac occasions a precipitate inclining to black.

When nitrous acid, diluted with water, is used, there is a disengagement of a small quantity of sulphurated hydrogen. The prussiates then form only a bright green precipitate in the liquid, which is instantly destroyed by acids: with hydro-sulphuret of ammoniac the precipitate is of a fine green.

When the lapis has been previously subjected to calcination, the dilute nitrous acid disengages a little of the sulphureous acid gas.

N. These facts not only prove that the lapis-lazuli contains a small portion of sulphur, but they also demonstrate the idenity of the colouring principle of this stone with every composition in which earthy substances enter into combination with sulphuret of iron. For we have seen (F. G.) the sulphate of lime containing iron, and converted by charcoal to the state of sulphuret of iron; and the sulphuret of iron, prepared in a direct manner, present the same phenomena under the same circumstances.

O. Before I conclude this memoir, I shall make a few observations, to guide those persons who may be disposed to repeat these experiments.

The composition of every specimen of lapis-lazuli is not essentially the same. In many pieces, and even in what is called the oriental, we distinctly perceive the sulphuret of iron in crystals with the metallic brilliancy; sometimes it is disseminated in small portions; and this undoubtedly is the cause which has hitherto prevented the most accurate chemists from ascertaining the true colouring principle of this substance. They have considered the sulphur merely as an accidental product foreign to the subject of analysis, without suspecting that there existed a blue sulphuret of iron. It is obvious that in experiments of this kind the greatest attention should be paid to the choice of fragments absolutely exempt of all pyritous admixture, or sulphuret of iron of a metallic yellow.

The presence of this last sulphuret is not the only difference which is found in the specimens of the lapis. Out of three kinds which were subjected to experiment, one contained sulphate of lime and siliceous earth, with crystalized sulphuret of iron, and the blue sulphuret of the same metal: the second also contained barytes: the third, which was absolutely free from pyritous admixture, contained also alumine and carbonate of lime in its composition, like that which was analysed by M. Klaproth.

The fact of crystals of alum being rapidly formed upon a fine plate of the lapis, as before remarked, proves beyond all doubt that some specimens contain accidentally a small quantity of pot-ash. It will be proper, therefore, to seek the colouring principle amongst those component parts which are essential to the stone, and not to be diverted too much by these accidental variations. We must not, however, suppose that the affinity of the earths to each other, or to the colouring principle, is without their influence upon the nature of the compound.

compound. A very ftrong proof to the contrary offered itfelf in one of the fynthetical operations. Sulphate of lime abounding with iron having been treated with powdered charcoal, and then kept in digeftion in nitrous acid; the pruffiate of pot-afh at firft had no other effect but to turn the fluid green, without affording any precipitate; the addition of a folution of alumine immediately determined a green precipitate, which was taken up by acids, and was acted upon in all refpects like that which was obtained from the decompofition of the lapis.

CONCLUSION.

I return to the confequences which appear to me to refult from the facts laid down in this memoir.

1°. The fulphate of lime of Montolier is coloured by a red oxide of iron, which adheres fo ftrongly to the filiceous earth as to refift the action of acids.

2°. This fulphate, treated with charcoal, produces a fulphuret of iron, in which this metal is lefs oxided, which being diffolved in acids, no longer affords a Pruffian blue by the pruffiates, but a green precipitate, which is deftroyed, inftead of being brightened, by acids, and which preferves its peculiar blue colour in pot-afh itfelf, and in the fire which its dry fufion requires.

3°. In operating upon fulphuret of iron prepared in the direct way, a product is obtained which manifefts the fame properties in the fame acids, and by the fame re-agents.

4°. Thefe phenomena are exactly fimilar to thofe which the lapis prefents when it is fubjected to the fame operations.

5°. So that we can at pleafure form the blue colouring principle of the lapis, with the only difference which neceffarily refults in the natural product, from the flow combination of the principle with the earths and the fulphate of lime.

6°. In a word, the blue fulphuret of iron is the true and only colouring principle of all the varieties of the lapis, and probably alfo of the mineral known by the name of blue ftone of Vorau.

V.

Experiments made with the Metallic Pile of Signor Volta, principally directed to afcertain the Powers of different Metallic Bodies. By Lieut. Col. HENRY HALDANE.

LETTER III.

To MR. NICHOLSON.

S I R,

THE apparatus I have ufed in the following experiments is of a very fimple conftruction. A deal board, of about fix feet long, three inches wide, and one inch in thicknefs. The middle of the board is fluted longitudinally, the channel being 0.6 inch wide. The

metallic pieces, which were ufed, being circular, and about 1.3 inch diameter, refted upon the edges of this groove, and which, with a fmall block of wood at each extremity or bafe, ferved to keep them in their places. The board being inclined to the horizon, the channel carried off the water that oozed from the wetted difcs.

In thofe experiments in which mercury was combined with other metals, it became neceffary, on account of the fluidity of the mercury, to adopt fome other contrivance for the apparatus, by which the flowing of the mercury might be prevented. To effect this, I procured 20 flat pieces of mahogany of two inches fquare, and of 0.3 inch in thicknefs: in the middle of each of thefe pieces was made a circular hole, of one inch diameter on the lower fide, and of 1.5 inch diameter on the upper fide, forming a rabbet at about two-thirds of the thicknefs of the wood from the upper furface. The lower fide was then covered with a piece of leather attached to it with a cement, which was not eafily acted upon by the water ufed in the experiment; and the leather was turned up the fides of the wood to make it more fecure. The cement I ufed is made of melted wax, rofin, and red ochre.

When this apparatus was to be ufed, the pieces of wood were put into pure water for a fufficient time to foak the leather; and when taken from the water, the mercury was poured into the fmall cifterns, filling them to the edge of the rabbet; the folid metal was then laid upon the rabbet, having its lower furface in contact with the mercury. If this folid metal was not of fufficient thicknefs to be level with the furface of the wood, a wetted difc was laid upon it to fill up the vacant fpace. Thefe fmall blocks of wood, thus prepared, being piled upon each other, the metallic fubftances they contained became capable of acting as a galvanic apparatus.

The order in which thefe experiments are arranged is according to the degrees of influence, the different metals feemed to poffefs, in forming the oxydating bafes or poles of the apparatus, when any oxydable metallic wires, placed in tubes of water, were fubmitted to examination; the other metal conftituting the oppofite or gafeous bafe or pole. The difcs, that were ufed, were fometimes of card, and fometimes of leather, and were foaked in pure water. Though the epidermis of an animal is obvioufly an infulating fubftance, with refpect to the galvanic influence (but lofes a part of that property by being wetted) yet I have not difcovered any difference in the effect, whether wetted card, or wetted leather, was employed; the leather has the advantage of retaining the moifture, which is fo abfolutely requifite in the whole operation of a galvanic apparatus, a longer time than the card; but it difcolours very much the adjoining furfaces of the metallic plates. It feems to be only neceffary that each pair of plates fhould be feparated from the adjacent pairs by fome fubftance that is not a perfect conductor of the galvanic influence, a property which feems to be confined to metalline fubftances; and that the feparating fubftance or difc fhould be conftantly wet. The wire ufed in thefe experiments was of copper, and the glafs tubes were filled with pure water.

3

Experiment

Experiment 1.—Zinc, combined with gold — filver — iron — copper — lead — tin — mercury.

The refults of all thefe combinations were :—the cauftic fenfation felt by the tongue; the irritation and galvanic fhock; and the oxydation of the copper wire. The zinc always forming the oxydating bafe or pole, and the other metals conftituting the gafeous bafe or pole.

In the combination with gold, (which was 40 guineas, and the zinc, the dimenfions of an half crown piece) the galvanic apparatus feemed to act with more vigour than under any other combination, and which was made apparent by comparing it with an apparatus compofed of fhillings inftead of the half crowns, which were employed in all the other combinations in which filver was ufed. In the combination of zinc with filver, the apparatus, which was compofed of 40 pairs of zinc and half crowns, acted with about the fame power as the gold apparatus; but this difference might be obferved, that in the gold apparatus the wire connected with the zinc bafe or oxydating pole began, after having depofited much green oxide of copper, to give out bubbles of air, when, in the filver and other apparatus, the wire connected with the gafeous bafe, after a fhort time, began to depofit oxide; that is, in the gold apparatus the power of producing gas feemed to predominate, and in the other combinations with zinc, that of producing oxide.

The combinations of zinc with the other metals produced much oxide and gas, and their powers of acting feemed to be in the order of iron, copper, lead, tin, mercury. In the combination with mercury, the effects were the moft feeble. The furface of the zinc in contact with the mercury became, of courfe, much amalgamated.

The green oxide of copper in the tubes was examined by putting fome drops of ammoniac *(aqua ammoniæ puræ)* into each of the tubes; the green oxide was diffolved, and the water acquired a blue tinge, but fome flakes of a brown fubftance remained in the tubes, not acted upon by the ammoniac. This brown matter was not always formed by all combinations, but the filver and copper always produced it.

Experiment 2.—Iron, combined with gold—filver—copper—lead—tin—mercury.

In thefe combinations the refults were nearly fimilar to thofe of the firft experiment, the iron forming the oxydating bafe or pole, and the other metals conftituting the oppofite or gafeous bafe or pole; but thefe apparatus acted more feebly, than the combinations with zinc, excepting when iron was combined with mercury, with which it feemed to act as powerfully as any combination in the firft experiment. With gold and filver, iron feemed to act tolerably well, produced much oxide, and gas. With copper it acted more feebly, and very minute bubbles only of air were produced. With zinc and tin, it only formed a cloud in the water, and no gas appeared at the extremity of the oppofite wire. The powers of thefe combinations with iron appeared to be in the order of mercury—gold—filver—copper—lead—tin.

The different colours and quantity of ruft formed upon the furfaces of the iron pieces when combined with mercury, gold, and filver, compared with what was formed in

the

the other combinations of iron, were worthy of notice. With gold, the ruſt was very red and very bright. The part of the iron in contact with the mercury retained its metallic luſtre.

Experiment 3.—Lead, combined with gold—ſilver—copper—tin—mercury.

In theſe combinations the operations of the galvanic apparatus were very feeble. The lead formed the oxydating baſe. When combined with gold or ſilver, oxide was depoſited from the wire connected with the lead, and the other baſe produced ſome gas. With copper, the oxide was more ſparingly formed, and very minute bubbles of air appeared at the extremity of the wire connected with the copper. With tin, only a ſmall cloud appeared at the extremity of the wire connected with the lead, and no gas was formed by the oppoſite wire. With mercury, 20 pieces of lead acted very feebly; ſcarce any effect was perceptible, a ſmall ſpeck of a cloud was viſible at the extremity of the wire connected with the lead, and no gas at the oppoſite wire: but from the cauſticity which at firſt was perceptible, it appeared that theſe metals in greater quantities might be capable of acting. The lead was, of courſe, much amalgamated by the mercury.

Experiment 4.—Tin, combined with gold—ſilver—copper—mercury.

Theſe combinations acted more feebly than any in the former experiments. The tin, when any effect was apparent, was always the oxydating baſe, but it produced only a cloud in the water in the glaſs tube. With 44 pieces of gold (guineas) a ſmall quantity of gas appeared, but with ſilver, or copper, no gas was viſible at the extremities of the wires connected with thoſe baſes, till the apparatus was increaſed to 160 pairs of metallic plates. With mercury, no effect was produced, not even cauſticity was perceptible, and the tin, which was 20 circular pieces of tin-foil, was ſoon amalgamated, and nearly diſſolved by the mercury.

Experiment 5.—Copper, combined with gold—ſilver—mercury.

In theſe combinations, no effect was viſible, excepting in the combination of 67 pairs of copper and ſilver. The wire which was connected with the copper, was ſurrounded with ſmall bubbles of air, and formed a cloud in the water, but the wire connected with the ſilver baſe, produced no gas. With 56 guineas the copper (penny pieces that have been always uſed) produced no oxyde and no gas, but when the apparatus was put together, a cauſtic ſenſation was perceptible upon touching it with the tongue, and which ſeemed to indicate a tendency to act as a galvanic apparatus if the number of metallic plates had been increaſed. With mercury, 20 plates of copper produced no effects whatever.

Experiment 5.—Silver, combined with gold.

In this combination of 30 pairs of plates, no cauſticity, and no effect whatever, was perceptible.

Judging from the reſults of former experiments, it was not thought neceſſary to combine either ſilver or gold with mercury, as no effect could be expected that was likely to compenſate for the certain amalgamation of thoſe metals; but it would have been very deſirable to have extended theſe enquiries to the effects which the other metallic ſubſtances

combined

combined in the form of a galvanic apparatus would produce, particularly *platina*; whether it would act with the other metals in a manner similar to gold or silver, forming always the gaseous base of the apparatus; or whether it would be capable of acting with those metals like zinc, iron, &c. and form the oxydating pole of the combinations; but *platina*, particularly in a malleable state, was found, upon enquiry, to be too expensive an article to be employed by an individual for this purpose, especially at a time, when money is so frequently required for many other experiments; indeed it might, in some degree, be submitted to examination in its granular form, by means of the apparatus which I used in the experiments with the quickfilver.

Experiment 6.   The object of this experiment, was to examine the effects produced by increasing the number of pieces that compose a galvanic apparatus; and to ascertain whether its power increases in a greater proportion, by augmenting the number of metallic plates, or by extending their surface.

The first apparatus consisted of 200 half crown pieces, combined with 200 pieces of zinc of the same diameter; and the discs were leather soaked in pure water. This apparatus was placed upon the board before described, extending in length 3 feet 9 inches. The effects of this combination were very feeble in proportion to its dimensions; when compared with an apparatus of forty pair of plates, its power was not increased in the proportion of 5 to 1. It was then re-constructed, and put together with leathern discs soaked in a solution of muriate of ammonia (*sal ammoniac*).

The result was, that this apparatus acted very powerfully; the caufticity, and the galvanic shock were more severe; the oxidation of the copper wire was more rapid; and the bubbles of air more copiously formed in the tubes of water. When the tongue was made part of the circuit, the sensation, which in the smaller apparatus seemed to be rather cauftic, resembled in this a pulsation, upon every repetition of the shock; when the shock was received upon the cheek, a flash of light appeared before the eyes, as when the tongue had formed a part of the circuit. When two wires connected with the bafes of this apparatus, were immerged in separate vessels of water, and the fingers of each hand were brought into contact with the water only, a galvanic shock was very perceptible, and a continued numbness was felt, if the fingers were permitted to remain in the water. From this experiment it is obvious, that the increased effects of a galvanic apparatus, depend more upon the nature of the substances which enter into its composition, than upon the number of metallic plates.

PART II.   EXPERIMENT 6.

In this experiment, it was proposed to examine the effects of a galvanic apparatus composed of 20 pairs of metallic plates, each six inches diameter, but being disappointed in obtaining the plates of zinc, the experiment has been deferred. The combinations of lead, iron, lead, and tin, have been examined, and from the increased effects which they exhibited, and which are always very feeble, there is little reason to expect, that the in-

creased

creafed furfaces of the plates in a galvanic apparatus, will be productive of proportionably increafed power.

Experiment 7. This experiment is not very diffimilar to that which you have related in your Journal, (page 185) in which you propofed to examine the aeriform matter, feparately, that was produced by each of the bafes or poles of a galvanic apparatus. In this experiment, an electric explofion was paffed through the gafes which iffued from the two extremities of gold wire connected with the oppofite bafes of the apparatus.

The apparatus that was ufed was compofed of 200 plates of zinc, and 200 half crown pieces, the difcs were of leather foaked in a folution of muriate of ammonia; the extremities of two pieces of gold wire were introduced into a fmall glafs veffel filled with pure water; having an interval between them of $\frac{1}{4}$ of an inch. This fmall veffel being completely immerged in a larger veffel of water, was inverted, and the projecting ends of the gold wire attached to the bafes of the apparatus.

The refult was, that gas iffued moft copioufly from both the extremities of the wire, immerged in the water, and afcended to the upper part of the veffel, where it was collected in a large bubble, in the courfe of the time the apparatus continued to act, which was about thirty hours. The wire connected with the zinc bafe was 3 feet 6 inches in length, and the wire connected with the filver bafe, about one foot. The former became much tarnifhed, gave out much air, but not fo rapidly, or for fo long a time, as the oppofite wire. The part of the wire connected with the filver bafe, that paffed through the water in the outer veffel, alfo gave out much air, and became at laft incrufted with fome white fubftance; upon the bubbles which were conftantly afcending from this wire, a fmall quantity of tincture of turnfole was dropped, but it underwent no change of colour. No oxide, or even a cloud, was produced in the water by either of the wires.

The gas thus collected was decanted into a glafs tube, which was bent, the branches being about four inches long, and two inches afunder; the interior diameter was 0.3 inch, and the gas occupied 1.5 inch in the curved part of this tube. The branches being then placed in feparate veffels of water, and two metallic wires having been previoufly fixed in this tube, having a fmall interval between their extremities at the part where the bubble refted; the difcharge of a fmall electrical jar was paffed through thefe wires, and the explofion which took place in the bubble, had fufficient force to raife the tube, the weight of which, with the wire and water it contained, was about three ounces. The bubble of air was reduced to lefs than half its original dimenfions, either by a diminution of its quantity, or of its elafticity. Many electrical explofions were then paffed through the remaining air, and alfo through it, when mixed with atmofpheric air, but without producing any motion in the tube, or any other effect.

The tube was then removed, and a fmall quantity of a mixture of oxygenous, and hydrogenous gas, which happened to be ready prepared, was admitted into it, fo as to form a bubble of the fame dimenfions as before; and upon paffing the difcharge of the electrical jar through it, the effects were precifely the fame, as were obferved in the air extricated from the gold wire, by the operation of the galvanic apparatus.

Experiment

Experiment 8. By the 7th experiment contained in my letter of the 24th of June, it appears that a galvanic apparatus is not capable of acting in a *Boylean vacuum* *. In this experiment, I introduced into three separate glaſs receivers (of 1 foot 3 inches in height, and 5.5 inches diameter) placed over water, three galvanic apparatus, each of forty half crowns with zinc, and the diſcs ſoaked in pure water. The firſt receiver was full of atmoſpheric air; the ſecond had been filled with oxygenous gas prepared from manganeſe and ſulphuric acid; and the third contained azotic or nitrogene gas, which had been obtained by leaving a mixture of 1.5 ounces of iron filings, with ſulphur in it for five days, during which time the water had riſen in it about four inches. Three glaſs tubes filled with pure water as before, and with copper wires, were joined to braſs wires, which had been previouſly fixed to the baſes of the apparatus, and paſſing under the bottom of the receiver, were extended above the ſurface of the water in the ciſterns.

The reſult of this experiment was, that the copper wires attached to the apparatus in the receiver filled with atmoſpheric air, produced oxide and gas as uſual, but not in ſuch quantities as when the apparatus has free acceſs with the external air. The wires attached to the apparatus in the receiver of oxygenous gas, formed gas and oxide moſt copiouſly, even part of the braſs wire attached to the zinc baſe, depoſited much oxide within the receiver. The wires attached to the receiver of azotic gas, produced neither oxide or gas, and the galvanic apparatus had no apparent effect.

The tubes after remaining twenty hours were removed, and new wire attached to the apparatus in the receivers containing atmoſpheric air, and the oxygenous gas; but they produced only a faint cloud in the water. The water had riſen in the receiver, which manifeſtly indicated a diminution of the original quantity of air.

CONCLUSION.

From this experiment, and from the examination of the ſtate to which atmoſpheric air is reduced in a glaſs receiver, placed over a galvanic apparatus and confined by water, I think we may venture to agree in opinion with Cit. Fabroni, (Phil. Journal, vol. III. p. 308) that the effects of galvaniſm depend on a chemical operation, and are produced principally by the attraction of oxygen from the atmoſphere, and therefore, on the preſent theory, the whole operation can be received only as a combuſtion ſimilar to that which ariſes from the combination of ſulphur, and iron filings with water.

<div align="center">

I remain, Sir,

with much eſteem,

your moſt obedient,

humble Servant,

</div>

Croydon, Aug. 3, 1800.   HENRY HALDANE.

I have ſubmitted to examination ſulphur and iron filings, in the apparatus I uſed for the quickſilver; but I do not perceive that that combination produces any effect as a galvanic apparatus.

* Vide Mr. Boyle's Letter to Lord Dungarvon, page 10, Quarto.

<div align="right">

Inveſtigation

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VI.

Investigation of the Powers of the Prismatic Colours to heat and illuminate Objects; with Remarks that prove the different Refrangibility of radiant Heat. To which are added, an Inquiry into the Method of viewing the Sun advantageously, with Telescopes of large Apertures and high magnifying Powers, and Experiments on the Refrangibility of the invisible Rays of the Sun. By WILLIAM HERSCHEL, *L. L. D. F. R. S* *.

THIS eminent philosopher begins his first paper by observing, that it is sometimes of great use in natural philosophy, to doubt of things which are commonly taken for granted; especially as the means of resolving any doubt when once it is entertained, are often within our reach. Whence it may be affirmed, that any experiment which leads us to investigate what was before admitted upon trust, may become of great utility to natural knowledge. Thus for instance, when we see the effect of the condensation of the sun's rays in the focus of a burning lens, it seems to be natural to suppose, that every one of the united rays contributes its proportional share to the intensity of the heat which is produced; and we should probably think it highly absurd, if it were asserted, that many of them had but little concern in the combustion or vitrification which follows when an object is put into that focus. From these considerations, he has thought fit to mention what led him to surmise, that the power of heating and illuminating objects might not be equally distributed among the variously coloured rays.

In a variety of experiments occasionally made, relating to the method of viewing the sun with large telescopes to the best advantage, he used various combinations of differently coloured darkening glasses. What appeared remarkable was, that when he used some of them he felt a sensation of heat, though he had but little light; while others gave much light, with scarcely any sensation of heat. Now as in these different combinations the sun's image was also differently coloured, it occurred to him that the prismatic rays might have the power of heating bodies very unequally distributed among them; and as he judged it right in this respect to entertain a doubt, it appeared equally proper to admit the same with regard to light. If certain colours should be more apt to occasion heat, others might, on the contrary, be more fit for vision by possessing a superior illuminating power; and at all events, it would be proper to recur to experiments for a decision.

In the first series of experiments *on the heating power of the coloured rays of the sun,* a piece of pasteboard was mounted in a frame, so that its obliquity could be varied with respect to the horizon, nearly in the same manner as in the common table looking glass. Through this pasteboard was cut a notch, or slit, of width a little larger than the ball of a thermo-

meter,

meter, and of fufficient length to permit the whole of one of the prifmatic colours to pafs through; fo that in the actual exhibition, this flit was placed parallel to the axis of a prifm through which the folar rays paffes, and the furface of the pafteboard was adjufted at right angles to the ray itfelf. Three delicate thermometers with their balls blacked, and fufficiently detached from their refpective fcales, were placed on the platform of the frame, at fuch a diftance beneath the opening in the pafteboard, that any one, or all of them, might at pleafure be expofed to the coloured rays, or fhaded from the light by the interpofition of the unperforated furface. When one of thefe was expofed to the rays, its mercury rofe, while that in the other two remaining ftationary, proved, that the acceffion of heat was produced by the mere action of the folar light. Eight fets of experiments were tabulated, from which it is feen, that the mean rife in one of the thermometers in red rays, during ten minutes was $6\frac{7}{8}$ degrees; and in the green rays $3\frac{1}{4}$ degrees; and in the violet rays two degrees. With a fmaller thermometer the fame effect followed, but the alteration was lefs, probably from the cooling agency of the afcending ftream of air, which would act more ftrongly where the proportion of furface to bulk was greater. The mean refults from both thermometers were, that the degrees of elevation in the red, green, and violet rays, proved nearly as the numbers $3\frac{1}{2}$, $1\frac{1}{2}$, and 1 refpectively.

The fecond courfe of experiments defcribed in this paper, was made *on the illuminating power of coloured rays;* in which the author had two ends in view, the firft with regard to the illumination itfelf, and the fecond with refpect to the aptnefs of the rays for giving diftinct vifion: properties which, though there did not feem to be any reafon why they fhould not have the fame meafure, appeared neverthelefs to deferve to be feparately attended to.

The microfcope offered itfelf as the moft convenient inftrument for this inveftigation; and this was ufed as upon opake objects, in order to avoid any effect that might be expected to arife from tranfmiffion through the parts of coloured tranfparent bodies.

The regifter of a number of experiments, in which different objects were viewed in the coloured rays by a magnifying power of 42 times, is given; among thefe a nail is mentioned as peculiarly fuited for refearches of the prefent kind. It was chofen on account of its folidity and blacknefs, as being moft likely to give an impartial refult of the modifications arifing from an illumination by differently coloured rays; but on viewing it, Dr. H. was ftruck with the fight of a bright conftellation of thoufands of luminous points, fcattered over its whole extent, as far as the field of the microfcope could take it in. Their light was that of the illuminating colour, but differed confiderably in brightnefs: fome of the points being dim and faint, while others were luminous and brilliant. The brighteft of them alfo admitted of a little variation in their colour, or rather in the intenfity of the fame colour; for in the center of fome of the moft brilliant of thefe lucid appearances, their light had more vivacity, and feemed to deviate from the illuminating tint towards whitenefs, while on and near the circumference it feemed to take a deeper hue.

The

The obfervations which agreed uncommonly well together, afforded the conclufion that the red making rays were far from poffeffing an eminent degree of illumination : that the orange poffefs more of it than the red ; the yellow ftill more ; the maximum of illumination lies in the brighteft yellow, or the paleft green ; the green itfelf is nearly as bright as the yellow ; but from the full deep green the illuminating power decreafes very fenfibly. That of the blue is nearly upon a par with the red ; the indigo much lefs than the blue ; and the violet is very deficient. With regard to the principle of diftinɛtnefs, none of the colours appeared to be deficient ; that is to fay, that though for want of illumination in the lefs powerful colours in this refpeɛt, fewer bright fpots could be difcerned, yet thofe which were vifible were perfeɛtly diftinɛt.

Before the Dr. proceeds to the next part of his fubjeɛt, he digreffes for a moment to remark, that the foregoing refearches ought to lead us on to others. " May not" fays he, " the chemical properties of the prifmatic colours be as different as thofe which relate to light and heat ? Adequate methods for an inveftigation of them may eafily be found ; and we cannot too minutely enter into an analyfis of light, which is the moft fubtle of all the aɛtive principles that are concerned in the mechanifm of the operations of nature. A better acquaintance with it may enable us to account for various faɛts that fall under our obfervation, but which have hitherto remained unexplained. If the power of heating, as we now fee, be chiefly lodged in the red-making rays, it accounts for the comfortable warmth that is thrown out from a fire, when it is in the ftate of a red glow ; and for the heat which is given out by charcoal, coke, and balls of fmall coal mixed up with clay ufed in hot houfes ; all which it is well known throw out red light. It alfo explains why the yellow, green, blue, and purple flames of burning fpirits mixed with falt, occafion fo little heat, that a hand is not materially injured, when paffed through their corufcations. If the chemical principles of colours alfo when afcertained, fhould be fuch, that an acid principle, for inftance, which has been afcribed to light in general, on account of its changing the complexion of various fubftances expofed to it, may refide only in one of the colours, while others may prove to be differently invefted, it will follow, that bodies may be varioufly affeɛted by light, according as they imbibe and retain, or tranfmit and refleɛt, the different colours of which it is compofed."

That radiant heat is of different refrangibility, is alfo deducible from thefe experiments, whether it be one and the fame thing, or different from light ; becaufe, as the author obferves, if this were not the cafe, the whole of the heat would be confined in a fpace equal in breadth to the prifm itfelf ; the contrary to which is proved by the faɛts. He alfo ftates, as is more fully developed in the fubfequent paper, that as the maximum of light is found at a much higher degree of refrangibility than that of heat in the vifible fpeɛtrum, the abfolute maximum lies even fhort of the limits of vifible radiation in the fpeɛtrum. And admitting, as is highly probable, that the organs of fight are only adapted to receive impreffions from particles of a certain momentum, an explanation will be had, why the maximum of illumination fhould be in the middle of the refrangible rays, as thofe which have

4 greater

greater or lefs momenta are likely to become equally unfit for the impreffions of fight. Whereas in radiant heat there may be no fuch limitation to the momentum of its particles. From the powerful effects of a burning lens, however, the information is gathered, that the momentum of terreftrial radiant heat is not likely to exceed that of the fun; and that confequently the refrangibility of calorific rays cannot much extend beyond that of calorific light. Hence alfo it is inferred, that the invifible heat of red hot iron gradually cooled till it ceafes to fhine, has the momentum of the invifible rays, which in the folar fpectrum, viewed by day light, go to the confines of red; and this will afford an eafy folution of the reflection of invifible heat by concave mirrors.

The refults of the foregoing inveftigation, originally fuggefted by the phenomena of vifion by telefcopes directed to the fun, became ufeful in their turn to direct the proceffes, by which a method might be obtained of *viewing that luminary with telescopes of large apertures and high magnifying powers.* The focal heat in the large telefcopes ufed by the Doctor, was fufficient fpeedily to break the darkening flips of wedge-formed glaffes, commonly ufed with achromatic telefcopes. It was a Newtonian reflector of nine inches aperture, which he wifhed to adapt for folar infpection, and his aim was to employ the whole furface of the fpeculum. Two red glaffes intercepted full as much light as was neceffary; but the eye could not bear the irritation arifing from a fenfe of heat. Green glaffes, one of which was fmoked, ftill gave a brighter illumination than the red, but they remedied the inconvenience of the heat. Various trials for intercepting the red, and other rays from a prifm, with differently coloured glaffes, were made; of which the particulars are given, and alfo of the effects of a coating of fmoke and of pitch, and of coloured fluids, the latter of which when denfe enough to ftop much light, were not found fufficiently pure to be ufed.

Thefe laft trials, however, were not fufficient to afcertain the very effential particular of diftinctnefs afforded by thefe feveral media. It was neceffary to try the feveral glaffes and combinations in the actual inftrument, previous to the account of which the Doctor defcribes an eafy way of uniformly fmoking glaffes, which I fhall proceed to copy.

" With a pair of warm pliers take hold of the glafs, and place it over a candle at a fufficient diftance not to contract fmoke. When it is heated, but no more than ftill to permit a finger to touch the edges of it, bring down the glafs at the fide of the flame as low as the wick will permit, which muft not be touched. Then, with a quick vibratory motion, agitate it in the flame from fide to fide; at the fame time advancing, and retiring it gently all the while. By this method you may proceed to lay on fmoke to any required darknefs. It ought to be viewed from time to time, not only to fee whether it be fufficiently dark, but whether any inequality may be perceived; for if that fhould happen, it will not be proper to go on.

" The fmoke of fealing wax is bad: that of pitch is worfe. A wax candle gives a good fmoke: that of a tallow candle is better. As good as any I have hitherto met with, is the fmoke of fpermaceti oil. In ufing a lamp you may alfo have the advantage of an even flame extended to any length."

<center>T t 2</center>

<div align="right">The</div>

The telescopic experiments, with various darkening glasses applied in the eye piece, are numbered and described. Difficulties presented themselves, from the heat which passed through some combinations, and the want of light and distinctness in others. The heat which was intercepted when the glasses were placed near the focus of the pencils, was also found to break them by its partial action. To remedy this, one of the glasses was fixed near the small speculum, in order that the light might be spread over a larger surface; but here also the heat was too strong, and produced the same inconvenience. The Doctor therefore at last placed his apparatus close behind the eye glasses, as follows in his best combination:

" No. 25. I placed a very dark green glass behind the second eye glass, that it might be sheltered by both glasses, which in my double eye-piece are close together, and of an equal focal length. Here, as the rays are not much concentrated, the coloured glass receives them on a large surface, and stops light and heat in the proportion of the square of its diameter now used, to that on which the rays would have fallen, had it been placed in the focus of pencils. And for the same reason, I now also placed a dark green smoked glass close upon the former, with the smoked side towards the eye, that the smoke might likewise be protected against heat by a passage of the rays through two surfaces of coloured glass."

" This position had moreover the advantage of leaving the telescope, with its mirrors and glasses, completely to perform its operation, before the application of the darkening apparatus; and thus to prevent the injury which must be occasioned, by the interposition of the heterogeneous colouring matter of the glasses and of the smoke."

" No. 26. I placed a deep blue glass, with a blueish green smoked one upon it, as in No. 25, and found the sun of a whiter colour than with the former composition. There was no disagreeable sensation of heat; a little warmth might be felt."

These two are the combinations through which the Doctor has seen uncommonly well, and in a long series of very interesting observations upon the sun, which will soon be communicated, the glasses have met with no accident. However, when the sun has considerable altitude, he finds it adviseable to lessen the aperture a little in telescopes, which have so much light as his ten feet reflector, or (which will give more distinctness) to view the sun earlier in the morning, and later in the afternoon; because the light intercepted by the atmosphere in lower altitudes, will reduce its brilliancy much more uniformly than it can be softened, by laying on more smoke on the darkening glasses. And as few instruments in common use are so large as that to which this method of darkening has been adapted, he expresses his hope that it may be of general use in solar observations.

In the second paper *on the refrangibility of the invisible rays of the sun,* the author first describes his apparatus, which is delineated in Plate XIV. where A. B. represents a small stand covered with white paper, upon which are drawn five lines parallel to each other, at half an inch distance asunder, but so that the first is only a quarter of an inch from the edge. These lines are intersected by three others at right angles, the second and third of which are respectively at $2\frac{1}{2}$ and 4 inches from the first; 1, 2, 3, represent the thermo-

meters

meters used in the former experiments, mounted upon their small inclined planes, and placed so that the centres of the shadows of their balls fall upon the intersection of these lines. C D is the prism at the window, and E is the spectrum thrown upon the table, so as to bring the last quarter of an inch of the red colour upon the stand. In this arrangement all the spectrum, except the vanishing last quarter of an inch, passed by the edge of the stand, and falling upon the table, could not interfere the experiments. The room was darkened.

The trials of heat were made by causing the spectrum to fall either upon or opposite the outer thermometer, while the other two were kept as standards out of the plane of the refraction; and when it was judged necessary, in repetitions, the thermometer, which had been subjected to the heat, was changed for one of the others. In three experiments with the same thermometer, the heat produced—by rays affording no illumination, but falling half an inch beyond the extreme confine of the red colour, was $6\frac{1}{2}$ degrees in 10 minutes; —by rays falling one inch beyond that confine, it was $5\frac{1}{4}$ degrees in 13 minutes;—and by rays falling $1\frac{1}{2}$ inch beyond that confine, it was $3\frac{1}{8}$ degrees in 10 minutes. At the other extremity of the spectrum there was no augmentation of heat produced beyond the confine of violet rays. The distance of the prism was 52 inches.

Hence it followed clearly that there are rays coming from the sun which are less refrangible than any which affect the sight; and that they are invested with a high power of heating, but with none of illuminating bodies, which explains the reason why they have hitherto escaped unnoticed.

As the heat was before found not to correspond with the measure of illumination in the spectrum, and in these experiments it extended beyond that limit, it became an object of interest and importance to ascertain the place where the calorific power is greatest. *This maximum of heat was found by experiment to be about half an inch distant from the boundary of the red colour*, and the heat at one inch was fully equal to that of the middle of the red colour itself. The boundaries of what may be called the calorific spectrum lie between the extreme of violet to an undetermined spot at least $1\frac{1}{2}$ inch, or $1\frac{1}{2}°$ beyond the boundary of the red colour.

. In his concluding summary, besides some remarks and recapitulation, which are included in the preceding account, the author adds, that if we may infer the quantity of the efficient from the effect produced, the invisible rays of the sun probably far exceed the visible ones in number; and that if we call *light* those rays which illuminate objects, and *radiant heat* those which heat bodies, it may be inquired, whether light be essentially different from radiant heat? In answer to which he suggests, that we are not allowed, by the rules of philosophizing, to admit two different causes to explain certain effects, if they may be accounted for by one. A beam of radiant heat, emanating from the sun, consists of rays that are differently refrangible. The range of their extent, when dispersed by a prism, begins at violet coloured light, where they are most refracted, and have the least efficacy. These calorific rays have been traced throughout the whole extent of the

3

prismatic

prifmatic fpectrum; and their power was found to be increafing, while their refrangibility was leffened as far as to the confines of red-coloured light. But their diminifhing refrangibility and increafing power did not ftop here; for they have been purfued a confiderable way beyond the *prifmatic fpectrum*, into an invifible ftate, ftill exerting their increafing energy, with a decreafe of refrangibility up to the maximum of their power; and have alfo been traced to that ftate where, though ftill lefs refracted, their energy, on account, as may be fuppofed, of their now failing denfity, decreafed pretty faft; after which the invifible *thermometrical fpectrum* foon vanifhed.

If this be a true account of folar heat, for the fupport of which he appeals to his experiments, he concludes that we muft admit that fuch of the rays of the fun as have the refrangibility of thofe which are contained in the prifmatic fpectrum, by the conftruction of the organs of fight, are admitted under the appearance of light and colour; and that the reft, being ftopped in the coats and humours of the eye, act upon them, as they are known to do upon all the other parts of our body, by occafioning a fenfation of heat.

VII.

Additional Experiments on Galvanic Electricity. By Mr. DAVY, Superintendant of the Pneumatic Inftitution.

To Mr. NICHOLSON.

SIR,

THE earlier experimenters * on animal electricity noticed the power of well burned charcoal to conduct the common galvanic influence.

I have found that this fubftance poffeffes the fame properties as metallic bodies in producing the fhock and fpark † when made a medium of communication between the ends of the galvanic pile of Signor Volta.

I have likewife found that perfectly well made charcoal, when connected with water or aqueous folutions in the galvanic circuit, effects changes in them analogous to thofe produced by metals; but connected with peculiar appearances.

1. Two long and thin flips of dry charcoal were connected with filver wires attached to the ends of a galvanic pile of 60 pieces. The points of the charcoal flips were immerfed in a glafs of water, at the diftance of half an inch from each other; and the globules of air adhering to them being carefully removed, the communication made fure.

In about a minute, particles of gas began to form and evolve themfelves round the point of the charcoal connected with the filver fide of the apparatus. Near a quarter of an hour

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* The inventor of the galvanic pile difcovered the conducting power of charcoal. His experiments were confirmed by Creve and Schmuck. See Pfaff on Animal Electricity, page 48.

† The fpark is moft vivid when the charcoal is hot.

elapfed,

elapfed, before any gas was produced from the zinc fide; the gas that was produced adhered to the charcoal in large globules, and did not pafs through the water. As long as the communication was kept up, the filver charcoal gave out gas very rapidly.

2. Reafoning from the common phenomena of the action of red-hot charcoal on water, and on the analogous galvanic facts, it was reafonable to conclude that the gas evolved from the charcoal on the filver fide of the apparatus was hydrocarbonate; and that carbonic acid had been produced on the zinc fide, which had been chiefly abforbed by the water.

To afcertain if this conclufion was true, two fmall open tubes, about one-fourth of an inch in diameter, and three inches long, were provided. Into one end of each of them a thin piece of hard and polifhed charcoal was introduced, and faftened by cement. They were then filled with diftilled water, and inverted in a glafs containing that fluid; the tops of the pieces of charcoal being made to communicate with the ends of a pile.

The procefs was carried on for more than fourteen hours; at the end of which time the quantity of gas produced from the charcoal on the filver fide was at leaft fifty times greater than that produced on the zinc fide. The tube from the zinc fide, with its water and gas, was introduced into a veffel of lime water. On agitation the water became clouded, but the gas was not perceptibly diminifhed: mingled with twice its bulk of nitrous gas, it gave fuch an abforption, as denoted that it contained nearly the fame quantity of oxygen as common air.

The gas produced from the filver fide of the pile did not at all diminifh with nitrous gas; twelve meafures of it, mingled with eight meafures of oxygen, in a detonating tube, and acted on by the electric fpark, inflamed and left a refiduum equal to rather more than three meafures. Lime water introduced to thefe became a little clouded, and a flight abforption took place. After this abforption, at leaft two meafures and a quarter of gas remained, which, mingled with nitrous gas, gave red fumes and diminution. Hence they evidently contained oxygen.

3. Surprifed at thefe refults, from which it appeared that the gas from the filver fide of the apparatus held very little charcoal in folution, and required nearly the fame quantity of oxygen to deftroy it as the inflammable air from the metals, I repeated the experiment, making ufe of water that had been long boiled, and was yet warm. In this cafe no gas was given out from the zinc fide during the whole of the procefs, and more than half an hour elapfed before any was produced from the filver fide. What was produced, however, gave nearly the fame diminution, when fired with oxygen, as common inflammable air, and the refiduum produced but a flight precipitate admitted to lime water.

It was eafy to account for the deficiency of gas on the zinc fide in this procefs, by fuppofing that the gas produced in the former experiment was air previoufly diffolved by the diftilled water, and liberated in confequence of the ftronger attraction of carbonic acid for that fluid; but as I had before found that in the common galvanic procefs with the metals, the hydrogen was immediately evolved, even in boiled water, it was difficult to

conceive

conceive why fuch a length of time was required for the production of the inflammable gas. When I introduced charcoal connected with the zinc fide, and filver wire connected with the filver fide, into boiled water, gas was almoft immediately given out from the wire; though when I connected filver with the zinc fide, and charcoal with the filver fide, no gas was liberated for many minutes.

4. A flip of charcoal was connected with the zinc end, and a filver wire with the filver end, and both plunged into a veffel of lime water. Gas was immediately given out from the filver wire; a few globules only formed round the charcoal: they were apparently covered with a white cruft. As the procefs advanced, white clouds fell from the charcoal, and diffufed themfelves through the fluid.

5. Two pieces of charcoal were connected with the ends of the pile, and plunged into a ftrong folution of cauftic pot-afh. During two hours no gas was given out from either of the pieces, and no change of colour was perceived in the fluid, though the communication was perfect. When a filver wire was connected with the zinc, and charcoal with the filver, gas was produced from the wire, but none from the charcoal. When charcoal was connected with the zinc, and filver wire with the filver, gas was very rapidly produced round the point of the wire; but not an atom formed round the charcoal.

6. When the flips of charcoal connected with the pile were introduced into folution of ammoniac, gas was given out from the zinc charcoal; but none from the filver charcoal. When filver wire was connected with the filver fide, the charcoal being ftill connected with the zinc fide, gas was given out from both, but moft rapidly from the wire. Thefe gafes were caught and examined. That from the charcoal gave no diminution with nitrous gas. An accident prevented me from examining it by other tefts; the gas from the filver equalled twenty times the volume of the other gas, and appeared to be pure hydrogen.

I fhall, at prefent, offer no theoretical conjectures concerning thefe experiments. The two laft will probably lead to interefting conclufions. I am, at this moment, engaged in examining fmall quantities of folution of pot-afh and ammoniac which have been long galvanifed in contact with charcoal: the refult of this examination, if at all important, I fhall take the liberty of communicating to you at fome future time.

<div style="text-align:center">I remain,</div>

<div style="text-align:center">SIR,</div>

<div style="text-align:center">Very refpectfully,</div>

<div style="text-align:center">Your's, &c.</div>

<div style="text-align:right">HUMPHRY DAVY.</div>

Dowry Square, Hotwells, Sept. 22, 1800.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

BOARD OF LONGITUDE OF FRANCE.

Prize of Astronomy.

THE tables of the moon are equally interesting to astronomy and navigation. The most celebrated geometers have assiduously cultivated the theory on which these tables are founded. The most important care of the practical astronomer is to observe all the motions of this planet, without which there would be no truth in geography, and which furnishes the navigator with the surest means of knowing the situation of his vessel, of directing his course, and safely arriving at any determined spot of the globe. In proportion as the Newtonian theory has been investigated, and the greater the perfection to which instruments and the method of observing have been brought, in the same proportion have the lunar tables been improved. Mayer, joining his own researches to those of the geometers of his time, and making choice of those observations which were the most to be depended upon, succeeded in forming tables which, by subsequent comparison with nearly twelve hundred unpublished observations, agreed with the most astonishing precision. Mason, under the direction of Dr. Maskelyne, rendered them still more perfect, by restoring several equations which had been omitted, though pointed out by Mayer, and by modifying the coefficients of all the others.

In spite of all this care, these tables, which in the middle of the century were so accurate, began progressively to lose their exactness. On recurring to the theory, the cause of the error and its remedy were seen. The prize memoirs, which two years ago were sent to the National Institute, and were published in the session of the fifteenth of *Germinal* last, have placed in the clearest point of view both the necessity and precise quantity of the equations lately discovered for the motions of the apogee and the node. Astronomers were not then invited to employ themselves on all the elements which compose the lunar tables ; the labour was too much disproportioned to the time appointed for the concurrence. One successful discovery often gives the wish and frequently the means of making another. That which has been so happily performed, has proved the possibility of doing still more, and of procuring for astronomy lunar tables of greater precision as well as durability. Nothing more remains after the fixation of the epochs, the secular movements, and their inequalities, than to discuss anew, by comparison with a great number of accurate observations, the precise quantity of the different equations which enter into the calculation of the moon's place.

This is the problem which the Board of Longitude of France now proposes for solution to the astronomers of all countries. The conditions are :

1. To difcufs, by comparifon with a great number of good obfervations, the value of the coefficients of the lunar inequalities, and to give for the longitude, latitude, and parallax of that luminary, formulas ftill more accurate and complete than thofe which have been ufed as the bafis of the tables at prefent in ufe.

2. To conftruct, according to thefe formulæ, tables of fufficient extent for the convenience and certainty of computation.

The prize is fix thoufand franks (250l.)

The Board of Longitude appoints no term for the decifion, but will award the prize to the firft piece which fhall fulfil the conditions of the program. It declares likewife to aftronomers and geometers that it will not ceafe to folicit the encouragement of the French government in favor of the perfection and theory of the tables of the moon.

The pieces muft be fent *au Bureau des Longitude*, Palais National des Sciences et Arts, à Paris.

They muft not bear the name of the author, but merely a fentence or device. If the author thinks fit he may fend alfo a fealed billet, containing not only the device, but the name and addrefs of the author, which billet will not be opened unlefs the piece itfelf fhall have gained the prize.

The prize will be given without any formality to the bearer of the receipt which the fecretary fhall have given.

(Signed)

DELAMBRE, Prefident.
LALANDE, Secretary.

Tranflation of a M S. Note of Cit. G. Cuvier, relative to Foffil Subjects of Natural Hiftory.

I am engaged on a large work upon the bones of quadrupeds found within the earth. I have already collected a large quantity of thefe bones as well from France as from foreign countries, and I have proved that feveral of them belong to fpecies at prefent unknown to naturalifts.

This is certainly the fact, not only with refpect to thofe foffils which were before known, fuch as the crocodiles or cetaceous animals of Maeftricht, the elephants and rhinocerofes of Siberia and Germany, the bears of Anfpach, the elks of Ireland; but alfo with refpect to others which were firft difcovered by me, fuch as the tapirs of the ufual fize, and the tapirs of gigantic magnitude of France; the animals intermediate between the tapir and the rhinoceros, three fpecies of which are found buried in the gypfum in the environs of Paris, whofe figures accompany this note *, to convey an idea of them to thofe perfons who may intereft themfelves in my refearches.

* The engravings, which are five in number, and will undoubtedly compofe part of the work of the learned author, could not, for many reafons, be given with this tranflation.---N.

4

Several

Several learned men in Germany, Holland, and Italy, who approve the object of my labours, have supplied me with valuable information, accompanied with drawings relating to the foffil bodies which are found in their respective countries, or which are depofited in their cabinets. On account of the war, I have not, till now, had an opportunity of foliciting the like affiftance from the philofophers of England; but I hope I fhall experience the fame liberal affiftance from them.

Under this fuppofition, I would requeft an account of fuch foffil bones of quadrupeds, as may be contained in the Britifh Mufeum, or in other collections in London; whether derived from Great Britain, or from other parts. I am particularly defirous of thofe which were buried in the mountain of Gibraltar, of which there muft be numerous fpecimens in London. I am ready to make my acknowledgements in return to thofe who may thus favour me by informing them of every thing which can be interefting to them in this country on the fubjects of natural hiftory and comparative anatomy.

I fhall be ftill more obliged if they will, at the fame time, fend me drawings of thefe foffils executed in a manner nearly fimilar to thofe which I now fend as fpecimens of engraving, and I engage to defray every expence which may be occafioned by fuch drawings.

It is needlefs to obferve that I fhall, in my intended work, publicly acknowledge my obligations to thofe who may have enriched it by their contributions.

<div style="text-align: right;">

G. CUVIER,

Member of the National Inftitute,

and Secretary to the Clafs of

Natural Philofophy.

</div>

Au Jardin des Plantes
à Paris. 15 *Therm.*
An. 8. (3 *Aouft* 1800.)

Hydraulic Engine operating by Mercury.

The refpectable correfpondent who has forwarded a defcription and fketch of an engine for raifing water by mercury, has rightly conjectured that the expedient was ufed before. Mr. Jofhua Hafkins, at the beginning of the century, made an engine of this kind, which was improved by Dr. Defaguliers, and is defcribed very much at large in the fecond volume of his Courfe of Experimental Philofophy. The mercury is merely a fubftitute for leathering, and has lefs friction: but it has many difadvantages, which are evident from that defcription, particularly that it cannot with convenience be ufed when the column or re-acting force is great; and it may alfo be expected that the mercury would foon be converted into a black duft or oxide, and carrried off in the water. I fuppofe this to have been the caufe of its being abandoned.

Philofophical Tranfactions of the Royal Society of London for the Year 1800. *Part the Firft. Quarto,* 238 *Pages, with eight Plates, and a Meteorological Journal.* London, fold by Elmfley.

This part contains:—1. The Croonian Lecture. On the Structure and Ufes of the Membrana of the Tympani of the Ear. By Everard Home, Efq. F. R. S. 2. On the Method of determining from the real Probabilities of Life, the Values of contingent Reverfions, in which three Lives are involved in the Survivorfhip. By William Morgan, Efq. F. R. S. 3. Abftract of a Regifter of the Barometer, Thermometer, and Rain, at Lyndon, in Rutland, for the Year 1798. By Thomas Barker, Efq. 4. On the Power of penetrating into Space by Telefcopes; with a comparative Determination of the Extent of that Power in natural Vifion, and in Telefcopes of various Sizes and Conftructions; illuftrated by felect Obfervations. By William Herfchel, LL. D. F. R. S. 5. A fecond Appendix to the improved Solution of a Problem in Phyfical Aftronomy, inferted in the Philofophical Tranfactions for the Year 1798, containing fome further Remarks and improved Formulæ for computing the Coefficients A and B; by which the Arithmetical Work is confiderably fhortened and facilitated. By the Rev. John Hellins, B. D. F. R. S. and Vicar of Potter's Perry, in Northamptonfhire. 6. Account of a Peculiarity in the Diftribution of the Arteries fent to the Limbs of flow-moving Animals; together with fome other fimilar Facts. By Mr. Anthony Carlifle, Surgeon. 7. Outlines of Experiments and Inquiries refpecting Sound and Light. By Thomas Young, M. D. F. R. S. 8. Obfervations on the Effects which take Place from the Deftruction of the Membrana Tympani of the Ear. By Mr. Aftley Cooper. In a Letter to Everard Home, Efq. by whom fome Remarks are added. 9. Experiments and Obfervations on the Light which is fpontaneoufly emitted, with fome Degree of permanency, from various Bodies. By Nathaniel Hulme, M. D. F. R. S. and A. S. 10. Account of a Series of Experiments, undertaken with the View of decompofing the Muriatic Acid. By Mr. William Henry. (Philof. Journal, IV. 209.) 11. On a new fulminating Mercury. By Edward Howard, Efq. F. R. S. Ibid, IV. Appendix. Meteorological Journal kept at the Apartments of the Royal Society by order of the Prefident and Council.

Royal Society of Copenhagen.

The Royal Society of Copenhagen not having received anfwers to its queftions on hiftory, mathematics, and natural philofophy, which were propofed laft year, repeat the fame queftions for the prefent year.

1. *Hiftory.*—What people difcovered America, and travelled into that part of the world before the Norwegians? How far did the difcoveries of the Norwegians extend in America,

particularly

particularly towards the weft? Proofs and conjectures derived from authors, or from exifting monuments, fuch as buildings, language, or the traditions of the Americans muft be quoted.

2. *Mathematics.* To difcover the function of all the quantities which, jointly, determine the magnitude of the effect of the caloric afforded by every kind of combuftible made ufe of in manufactories, whether wood, pitcoal, or charcoal. When the equation is found, it muft be determined in the four following cafes: 1°. When wood, pitcoal, or charcoal are burnt in a ftove or veffel to heat the confined air; for example of a room. 2°. When thefe materials are ufed to boil all forts of liquids over a fire place. 3°. When they are ufed to harden foft fubftances, as, for inftance, to make bricks. 4°. and laftly, when employed in melting metals.

Supported by experiments, the authors muft difcover and eftablifh the different equations analytically; in order that by their application, the effect and œconomy of each of thefe three kinds of combuftibles, wood, coal, and charcoal, may be calculated.

3. *Natural philofophy.* To find by experiments the greateft degree of heat which aqueous vapours, when heated, can communicate to other bodies? Can that part of the water which, in the digefter of Papin, is not reduced to vapour by the heat, acquire more than 212 degrees of Fahrenheit's thermometer?

4. The Society having had the fatisfaction to crown an anfwer to its philofophical queftion for the preceding year, propofes the following queftion for the prize of the prefent year.

What new difcoveries have been made in philofophy fince the time of Plato and Ariftotle, in its refearches into, and its explanation of, the nature of human knowledge with refpect to exifting beings?

· The prefent queftion here, is not to afcertain what progrefs has been made in thofe fciences which treat exiftances, whether corporeal or incorporeal; but it relates to our internal knowledge (which fome philofophers call *fubjective*) to know what, in general, is the nature and the power of this knowledge, its origin and caufe, the principles and reafons upon which this manner of knowing is determined and eftablifhed, and upon which is founded that truth which is therein found, or imagined to be found. It is expected that an hiftorical expofition fhould be given, of what has been produced on this fubject by the meditations of philofophers fince the time of Plato and Ariftotle; how much we are indebted to them for new difcoveries, or for facts better eftablifhed and defined; or on the contrary, if it fhall appear that philofophy has not made any progrefs in this refpect, it is expected that this fhould be demonftrated from the hiftory of the dogmas of philofophers.

The anfwers to thefe queftions are to be written in Danifh, in French, or in German, and forwarded with the ufual formalities, before the end of June 1801, to Profeffor P. C. Abildgaard, Secretary to the Society.

3

Decompofition

Decompofition of the fixed Alkalis.

Van Mons, in a Letter to C. de la Metherie, inferted in the Journal de Phyfique, Vol. VII. p. 390, acquaints him, that *Craaner*, an apothecary at Amfterdam, has made an experiment, by which it appears that the alkalis contain carbon. He includes pot-afh, or foda, moiftened with water under an inverted glafs with oxygen gas. The gas is ab-forbed, and the alkali becomes effervefcent. Moreover, he finds that an alkali, treated a certain number of times (*quelquefois*) with this gas, becomes exhaufted of carbon, and can no longer be made effervefcent.

V. M. long ago publifhed, that pot-afh is formed during the combuftion of hydro-carbon, and during the fermentation of grapes, by the intervention of the azote of the atmofphere. He adds, that when thefe operations are made in pure oxygen, neither pot-afh, nor the acidulous tartrite of that alkali are obtained; but the wine becomes too four to be drank. He conjectures, that the fixed alkali may be carbonated azote.

But he mentions two experiments which militate againft this theory. In the firft he expofed cauftic pot-afh, and the red oxide of mercury by fire, to red heat in a retort, and obtained nothing but oxygen gas, nitric acid and water. In the fecond, he triturated the oxymuriate of pot-afh with cryftals of cauftic alkali, and poured the mixture into a phial, which he kept exactly clofed for three days. At the end of this period he poured muriatic acid upon it, but had no effervefcence.

As carbon is not combuftible in the air but at a red heat, our author remarks, that if this principle enter into the compofition of alkalis, it muft be united with hydrogen, unlefs azote ferves as the medium of its union with oxygen.

In the fame Journal for Meffidor VIII. I find a note, page 82, in which it is afferted, that Guyton Morveau, on the 6th floreal, read to the French National Inftitute a memoir on the conftituent principles of the fixed alkalis. He made his experiments jointly with Deformes pupil of the polytechnic fchool, and their conclufions are,

1. That pot-afh is compofed of lime and hydrogen.
2. That foda is compofed of magnefia and hydrogen.
3. That lime is compofed of carbon, azote, and hydrogen.
4. That magnefia is compofed of lime and azote, and confequently of carbon, hydrogen, and azote.

Separation of Butter.

Brugnatelli has feparated butter from cream without the affiftance of oxygen. He pours four parts of hot water to one of cream, and paffes the mixture through a ftrainer of coarfe cloth in a clofed veffel, in which feveral ftrainers of the fame kind are ftretched. In twenty-four hours the whey has paffed the ftrainers, and the butter is perfectly feparated.

J. de Phys. VII. 391.

Beet

Beet Sugar and ardent Spirits.

The National Inftitute of France having named a commiffion to repeat the experiment of Achard on Beet Sugar, Citizen Deyeux has lately made their report, the refult of which is, that the beft fugar can be afforded at 15 fous per livre, or 7d. per lb. avoirdupois. The Inftitute therefore invites the Government to form an eftablifhment on a large fcale for this important objeft of economy.   Mag. Encycl. An. VIII. tom. ii. p. 97.

Brandy and arrack are made at Berlin from the refufe of the beet root. Both are highly praifed, and the former is on fale in the market.

Fourcroy's Synoptic Tables of Chemiftry.

The method of exhibiting the whole of a fubjeft, with the relations of its parts to the eye in tables, is of moft admirable ufe for the perfpicuity of communication, and its impreffive effeft on the memory. It feems as if the caprice of fafhion, which during the century nearly elapfed was inimical to every appearance of formality, had in a great meafure fuppreffed thefe valuable helps to learning. The Tableaux Synoptiques of Fourcroy will be not only of the higheft fervice to the individual fcience of chemiftry, but may tend to fhew the advantages of pofition and arrangement in other fciences, which greatly want it. Thefe tables are printed on twelve large fheets, twenty-two inches by thirty, and confequently with the misfortune of two folds in each. Every fheet contains one table, and the author's arrangement is governed by the chemical properties of bodies. The firft table contains the generalities of the fcience, its order, methods, hiftory, divifions, explication to medicine, &c. 2. The fecond prefents (*a*) the firft clafs of fimple, or undecompofed bodies, as light caloric, oxygen and azote, and others according to their order of combuftibility, as hydrogen, carbon, fulphur, diamond, and the metals; and (*b*) thefe bodies burned or united to oxygen, or the feries of oxides or acids, claffed by their attraftions to the principle of combuftion, and the difficulty of decompofing them. 3. The third exhibits firft the falifiable bafes, namely earths and alkalis; and fecondly, the firft claffes of falts. 4, 5, the fourth and fifth tables are alfo employed in the claffification and defcription of falts 6, 7, 8, 9, 10. The five following tables are engaged on the twenty one metallic fubftances hitherto difcovered. 11, 12. and the eleventh and twelfth prefent fketches of the vegetable and chemical chemiftry.

Thefe tables may be confidered as the fynopfis of the *Syfteme général des Connaiffances chimiques* of the author, now in the prefs, and after its appearance he will publifh a fuller feries of tables on the fubjefts of the two laft.

The prefent tables are on fale at De Boffe's, in Gerard Street, London.

Note

Note from a Correspondent respecting the Bolognian Phosphorus.

To Mr. NICHOLSON.

SIR,

I AM in poffeffion of fome ftones from Monte Paderno, near Bologna, of which the phofphorus, called there Spongia di luce, and Pietra cuminabile, is made. I have confulted all the books of chemiftry which mention the method of making this phofphorus, and feveral experienced chemifts have exactly followed their directions, but in vain, as no one has been able to caufe the ftone to imbibe light.

Your inferting this in your valuable Journal, may perhaps be the means of procuring fome information on the fubject, and the exact method of preparing it; likewife if it can be procured in London. I brought fome from Bologna fome years ago, but it foon loft its property of retaining light.

<div align="center">

I am, Sir,
Your humble Servant,
</div>

Sept. 24, 1800.   R. T.

Extract of a Letter from Mr William Henry, dated Sept. 25, 1800, *to correct an Inference in his Paper on Galvanifm.*

" The inference which I had drawn, from the experiments defcribed in your Journal for Auguft, refpecting the decompofition of the vegetable alcali, I had found to be erroneous, before the interefting facts of Mr. Davy were known to me; and I had fully intended a recantation, along with an account of fome other experiments on the fame fubject, in which I was engaged, above a month ago. Thefe, however, various circumftances have prevented me from completing; and I think it proper, therefore, not to delay making an acknowledgment, and ftating the caufe, of my miftake.

" The fact which firft led me to fufpect, that I had drawn too hafty a conclufion, was, that the black precipitate proved, on examination, to be merely a metallic oxide, and not charcoal, as I had fuppofed probable. Varying, therefore, the circumftances of the experiment, I tranfmitted the galvanic influence through liquid cauftic alkali, without the contact of mercury, and the black powder then ceafed to appear. The gafes alfo turned out to be a mixture of hydrogen and oxygen, in the proportions, pretty nearly, that might be expected from water. This oxygen, the imperfect metals, contained in the quickfilver, had before prevented from appearing in a gafeous form. From ammonia, the contact of mercury being excluded, gafes were obtained correfponding, in kind and proportion, with thofe ftated in your Journal (p. 261), the quantity of oxygen gas appearing to diminifh as the alcaline folution was more completely faturated."

Mr Close's Apparatus for raising Water by its lateral Action.
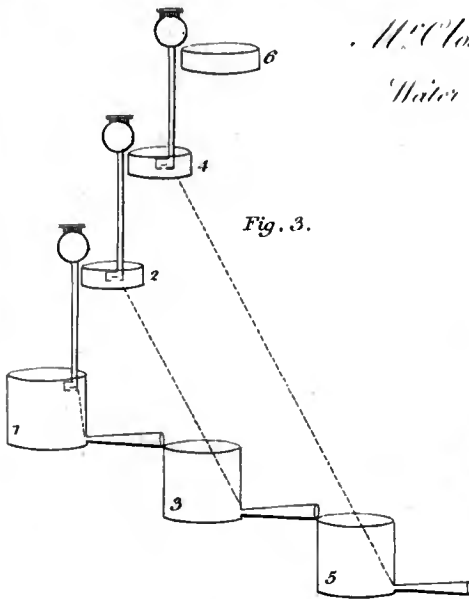
Fig. 3.
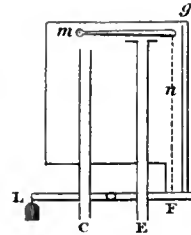
Fig. 2.
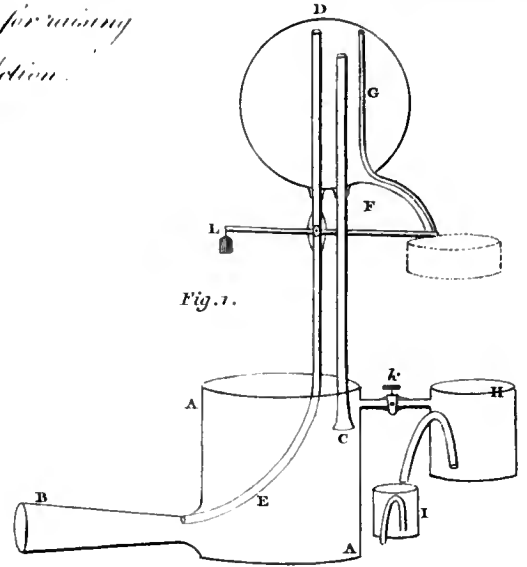
Fig. 1.

Dr Wollaston's explanation of double images caused by atmospherical Refraction.
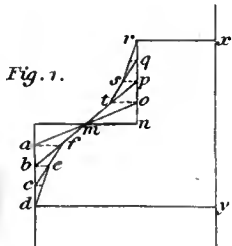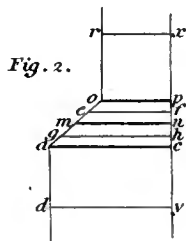
Fig. 1.

Fig. 2.
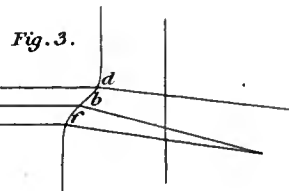
Fig. 3.
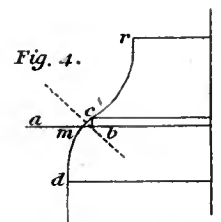
Fig. 4.
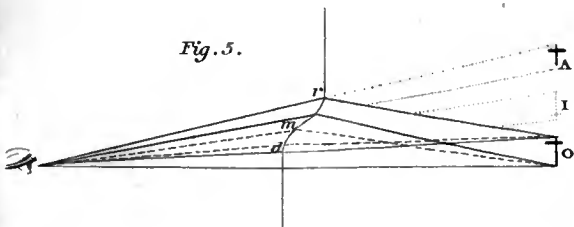
Fig. 5.
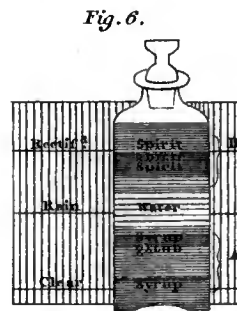
Fig. 6.
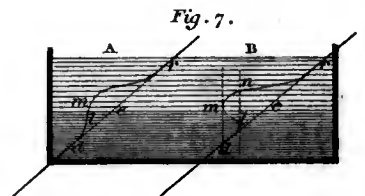
Fig. 7.
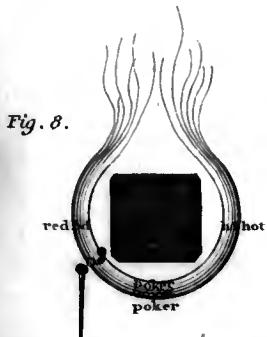
Fig. 8.

Fig. 9.

Fig. 10.

Fig. 11.

red hot   hot

Poker

poker
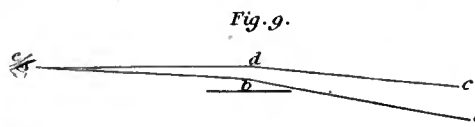
Dr. Herschel's Experiments on the
Invisible Rays of the Sun.

D

C

Violet Blue Yellow Red

Indigo Green Orange

E

A

B

Mutlow sc.

A

JOURNAL

O F

NATURAL PHILOSOPHY, CHEMISTRY,

A N D
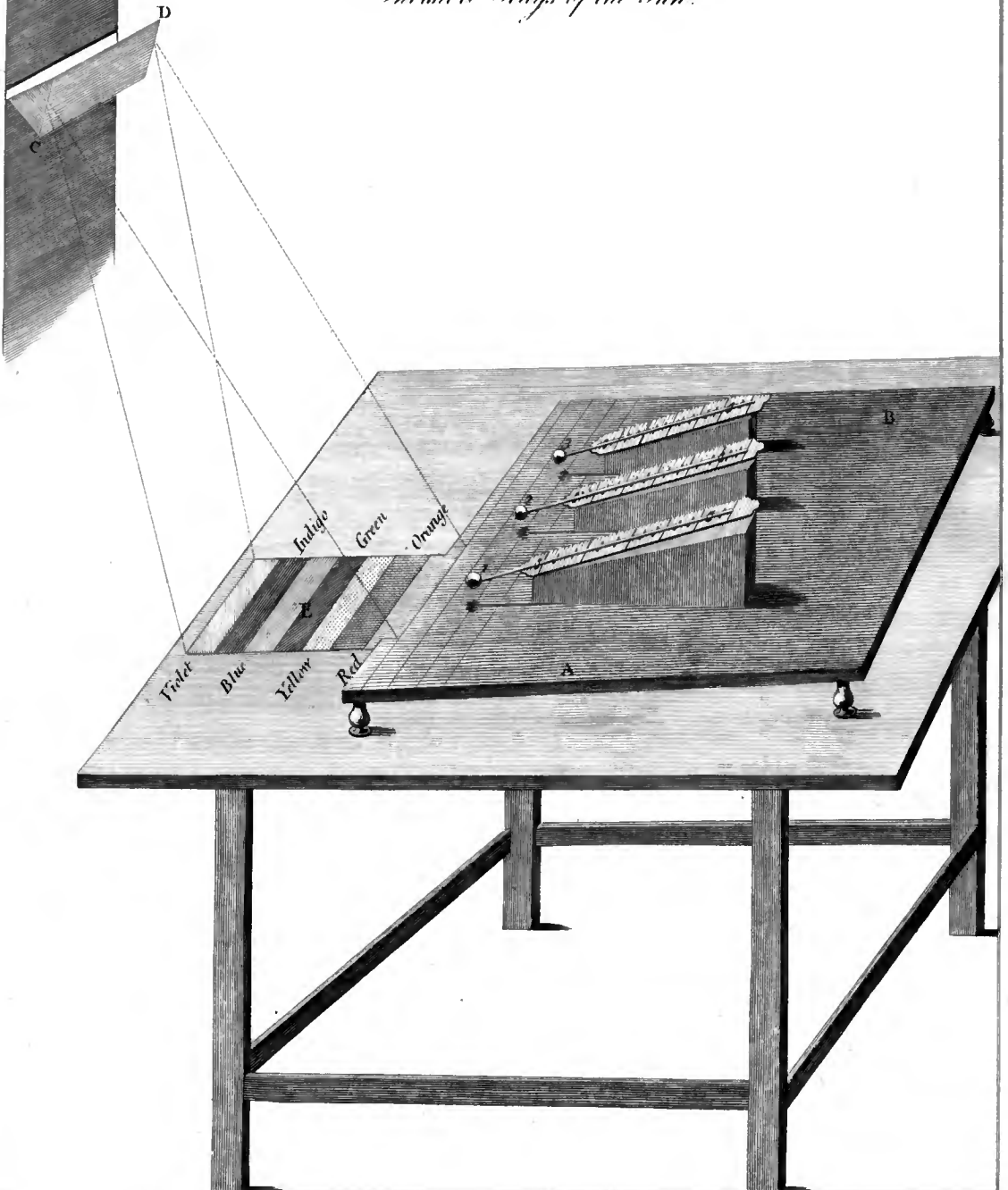
THE ARTS.

ARTICLE I.

Notice of some Observations on the Causes of the Galvanic Phenomena, and on certain Modes of increasing the Powers of the Galvanic Pile of Volta. By Mr. DAVY, Superintendant of the Pneumatic Institution. Communicated by the Author.

THE nature of this communication is incompatible with a detail of the opinions prevailing amongst philosophers, respecting the causes of the galvanic phenomena : they have been generally supposed to depend on the different powers of bodies to conduct electric fluid ; Fabroni was the first who systematically attempted to prove that they were chemical effects [*].

Immediately after I had perused an interesting observation of Lieutenant-Colonel Haldane [†] on the non-excitement of galvanism in the vacuum of an air pump, I began an investigation with the view of ascertaining precisely the influence of the atmosphere on the phenomena. In carrying on this investigation, I have met with some new facts, which are capable of arrangement, and which will probably lead to a complete explanation of the galvanic effects.

[*] Phil. Journal, III. 308.　　　　[†] Phil. Journal, IV. 242.

The piles that I employed for afcertaining the influence of factitious airs on the galvanic phenomena, were erected horizontally in the ufual mode; but to prevent the plates from feparating when in an oblique pofition, their fides were joined together by refinous cement at two or three points, fufficient interftice being preferved to admit of a free circulation of air. The gafes, when any were produced, were received in fmall tubes filled with diftilled water, containing wires covered externally with wax, and communicating with the ends of the pile. The piles were introduced into the airs through water*, and elevated above the water by a metallic plate cemented to their lower extremities.

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1. *Zinc, whether connected with Silver in fingle Galvanic Circles, or conftituting the Plates of the Galvanic Pile, feems to undergo no Oxidation at common Temperatures, as long as the Water in contact with it is pure.*

By pure water, is more immediately meant water holding in folution no oxygen gas, no nitrous gas, and no acids. It has long been known that certain metallic bodies, which oxidate flowly in water expofed to the atmofphere, effect no change in pure water†: this I have obferved is particularly the cafe with regard to zinc. That zinc, when in contact with filver in the atmofphere, and forming with it a circuit by means of water, becomes oxidated much more rapidly than when fimply in contact with water, was obferved by Dr. Afh ‡.

Suppofing the more rapid oxidation to be the effect of a peculiar electrical influence produced by the contact of the metals, it would be reafonable to conclude, that zinc in fingle circles with filver and pure water, or at leaft in the galvanic pile erected with cloths moiftened in pure water, would undergo oxidation. Fabroni §, however, has advanced that fingle galvanic circles do not oxidate in water, unlefs it be expofed to the atmofphere. That the zinc of the galvanic pile does not oxidate in contact with pure water, will appear from the following obfervations:

a. A fmall pile of filver and zinc, having its pafteboards moiftened with water, that had been juft boiled, was introduced into a veffel of water that had been long boiling, and was yet warm. Refinous cement was poured upon the furface of the water, and faftened to the glafs as it cooled, to preferve it effectually from the contact of air ‖. The apparatus, after remaining for two days, was examined; the zinc plates were fcarcely at all tarnifhed; no oxide was depofited in the water, and no gas had been evolved through it. A fimilar

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* Before thefe experiments were made, I had found, by numerous trials, that a pile acted in the atmofphere immediately after its immerfion in water without being wiped, though more feebly than before: I had likewife found that after the firft immerfion, the powers were not diminifhed by fubfequent ones.

† See Fabroni, Phil. Journ. III. 309.

‡ And Humboldt, fee Refearches Chem. & Phil. page 568.

§ Phil. Journ. III. 309.

‖ Oil of turpentine, and even common oil, as will be feen hereafter, is ineffectual for this purpofe.

pile

pile, expofed for nearly the fame time to water in contact with the atmofphere, had depofited much white oxide, and given out fome gas, and the zinc plates were whitened both internally and externally [*].

b. No influence can be communicated from an elaftic atmofphere, enabling the zinc of the galvanic pile to decompofe pure water; for piles were introduced into hydrogen, nitrogen, nitrous oxide, and hydro-carbonate, and fuffered to remain in them for different lengths of time; but in none of thefe gafes was the zinc more oxidated than if the pile had been immerfed in pure water.

c. A galvanic pile was fuffered to remain in vacuo for fourteen hours, the gage being about $\frac{9}{10}$, the zinc plates were flightly tarnifhed, but no white oxide had formed upon them.

2. *The Oxidation of the Zinc Plates of the Galvanic Pile takes place whenever the Water in contact with them holds Atmofpheric Air, or Oxygen, or Nitrous Gas, or Nitrous Acid, or Marine Acid, &c. in Solution.*

a. The oxidation of the zinc of the pile in the atmofphere, and the protufion of carbonate of foda, has been accurately defcribed by Meffrs. Nicholfon and Carlifle. I have found that the phenomena of oxidation takes place much more rapidly in pure oxygen than in the atmofphere. I have likewife found that it takes place more flowly in nitrous gas than in the atmofphere. That zinc is oxidated in folutions of nitrous and marine acids, is a well known fact.

3. *When the Zinc in contact with Water, holding in folution Subftances containing loofe Oxygen, or Acids, is oxidated, thefe Subftances are altered, or they exert fome Chemical Affinities.*

a. A fmall galvanic pile, expofed to about twenty-one cubic inches of oxygen gas for fix hours, effected a diminution of it which might have equalled one-fourth.

b. A fimilar pile, expofed to an equal quantity of atmofpheric air for two days, diminifhed it one-fixth: the refiduum being fuffered to pafs into the atmofphere, gave a fmell which ftrongly refembled that of ammoniac; and the pile held over marine acid produced denfe white clouds.

In another experiment, an equal quantity of air, expofed to the pile for three days, gave but a very flight diminution with nitrous gas, and could not be inflamed with atmofpheric air.

c. I have noticed, *Ref. Chem. & Phil.* that wetted zinc, placed in contact with nitrous gas over mercury, flowly converts it into nitrous oxide and ammoniac. A fmall pile, ex-

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[*] The temperature in thefe experiments was from 54° to 61°. It is impoffible to free water of *all* its diffolved air by boiling: it is likewife impoffible to prevent a few globules of air from paffing through the cement whilft it is cooling: the very flight tarnifh of zinc kept in boiled water may be owing to the minute quantities of common air exifting in the water from both thefe caufes.

pofed

pofed to about twenty-two cubic inches of nitrous gas for three days, diminifhed it one-fourth, and fome of the gas was rendered abforbable by water.

d. The formation of nitrous gas, nitrous oxide, and ammoniac, when very weak folutions of nitrous acid are made to oxidate zinc, demonftrate both the decompofition of the acid and the water.

e. The oxidation of zinc in folutions of marine and fulphuric acids in water, appears to be owing to the affinity which has been called predifpofing.

4. *The Galvanic Pile of Signor Volta feems incapable of acting when the Water between the Pairs of Plates is pure.*

The word *pure* is here ufed in the fame fenfe as in 1.

a. I have found, by numerous experiments, that the galvanic pile introduced into hydrogen, through common water, ceafes to evolve gas in the tube, and to act in about five or fix minutes, *i. e.* in about the time required to confume the atmofpheric air diffolved in the common water between its plates. The phenomenon is exactly the fame in nitrogen, nitrous oxide, and hydro-carbonate. The action of a pile, as known by its power of evolving gafes from water, is diminifhed immediately on its introduction into thofe gafes. It ceafes in them nearly in the fame time, and cannot be reftored by admitting frefh gafes of the fame kind, though, as will be feen in the next fection, it is immediately reftored by immerfing the pile for a moment in water faturated with atmofpheric air.

b. I have found, by numerous experiments, made with the affiftance of my friend, Mr. King, that the galvanic pile ceafes to act in vacuo when the gage is at about $\frac{6}{10}$ of an inch, even though the poles are connected by wires with the atmofphere, and the water it is made to act upon in contact with the atmofphere.

5. *The Pile acts when the Water between the double Plates holds in Solution Atmofpheric Air, or Oxygen, or Nitrous Gas, or Nitrous Acid, or Marine Acid.*

This fact I have proved by numerous experiments.—*a.* A pile acted in atmofpheric air, included in a glafs cylinder over water for two days, till nearly all the oxygen of the air was confumed.—*b.* A pile decompofed water much more rapidly in oxygen than in common air, and lefs rapidly in nitrous gas than in common air.—*c.* That the influence of an elaftic oxygenated atmofphere is not effential to the galvanic effects, is evident from the fact of the conftant reftoration of the powers of a pile after they had ceafed to appear in the hydrogen, nitrogen, &c. by momentary immerfion in water faturated with atmofpheric air. In thefe experiments the piles were plunged into the water confining the gafes, and again immediately elevated into the gas without being expofed to the atmofphere : and the phenomenon could be owing to no other caufe than the impregnation of the water with atmofpheric air, becaufe when piles were plunged into water faturated with nitrous oxide *,

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* This gas expels much common air from water ; fee *Refearch. Chem. & Phil.* R. II. S. 1.

their

their powers were not reftored.—*d.* I have proved by many experiments * that water, deprived of air, is capable of attracting it from the fpirit of turpentine when the laft is in contact with the atmofphere. The galvanic pile acts in fpirits of turpentine for a great length of time, and nearly as well as in the atmofphere, the water between its plates being conftantly fupplied with air from the fpirits. It acts but for a fhort time in fpirits of wine, on account of the combination of this fluid with its water.—*e.* After a pile had ceafed to act in hydrogen, its powers were uniformly reftored by a momentary immerfion in very diluted marine acid. They were likewife reftored, and rendered more intenfe than in the atmofpher ebymomentary immerfion of the pile in diluted nitrous acid, though they continued to be exerted for a fhort time only, *i. e.* till the acid was decompofed or faturated.

6. *The Power of Action of the Pile of Volta appears to be proportional to the Power of the conducting fluid Subftance between the double Plates to oxidate the Zinc.*

This feems fufficiently proved from the facts in the following fections. The zinc oxidates lefs rapidly in nitrous gas than in atmofpheric air, and lefs rapidly in atmofpheric air than in oxygen: and the power of action of the pile as known by its evolving gas from water is greater in oxygen than in atmofpheric air, and greater in atmofpheric air than in nitrous gas. The power of the pile to decompofe water, and to give the fhock is wonderfully increafed after it has been dipped in marine acid, and ftill more increafed after it has been dipped in weak nitrous acid; and thefe bodies only enable the zinc to oxydate itfelf more rapidly. A feries of plates, in which the oxidating conducting fluid was ftrong nitrous acid, acted, as will be feen hereafter, infinitely more powerfully than any other combination; fo that it would feem that the power of a pile is not much connected with the evolution of hydrogen from water.

7. *Conclufions.*

Of two phenomena, or of two feries of phenomena, we can only affirm that the one is the caufe of the other when it uniformly precedes it, and when their modifications are connected. But it appears from all the foregoing facts, that the galvanic pile of Volta acts only when the conducting fubftance between the plates is capable of oxidating the zinc; and that in proportion as a greater quantity of oxygen enters into combination with the zinc in a given time, fo in proportion is the power of the pile to decompofe water, and to give the fhock greater. It feems therefore reafonable to conclude, though with our prefent quantity of facts we are unable to explain the exact mode of operation, that the oxidation of the zinc in the pile and the chemical changes connected with it are *fome how* the caufe of the electrical effects it produces.

* Thefe experiments will be hereafter detailed.

8. *Of*

8. *Of a new Mode of constructing a Pile.*

Assuming the truth of this conclusion it was easy to conceive, that a pile much more powerful than any hitherto constructed might be made, particularly supposing that the decomposition of water was not essential to the process, plates of zinc and silver, 1.2 inches square, were fastened in pairs by resinous cement: eighteen of these pairs were connected to each other by cement, and so inclosed by it as to leave water-tight partitions open at one side only between each pair of plates. When muriatic acid was poured between the partitions of this machine, the plates being perpendicular, it acted very powerfully; its capability of decomposing water and giving the shock, being at least equal to that of a common pile of seventy plates. Diluted nitrous acid made it act still more powerfully. When the partitions were filled with water, its action was barely perceptible. Concentrated nitrous acid was poured into them. In this case the first shock was so powerful as to benumb my fingers for some seconds, and I did not dare to take another. I was almost immediately obliged to throw the pile into water to prevent it from being destroyed, so that there was no time to ascertain its power of decomposing water.

In a second experiment, with strong nitrous acid, I used only five pairs of plates, when the shock was full as powerful as from the common pile of thirty plates.

Three pairs of plates, with nitrous acid, gave a very sensible shock.

I have procured (on account of the loss of the silver when this substance is part of the pile with nitrous acid) a number of plates of copper, iron, and zinc. They have not yet been disposed in the apparatus; but I expect by means of nitrous acid, to produce effects from them, equal to those of the strongest electrical battery.

II.

On raising Water by the Engine [*] *of H. GOODWYN, Esq. through double or treble the Space of the descending Column, and on the proper Arrangement to make it require n o. Attendance. By a Correspondent.*

To Mr. NICHOLSON.

S I R,

AFTER considering various methods of raising water with little trouble and expence, I am decidedly of opinion that Mr. Goodwyn's engine will answer this purpose the best. It is formed upon a very elegant principle, and operates by the assistance of only a small quantity of water: it may be made in various forms, either to raise the fluid above the descending column, or from below it to a level with the bottom; and the height may be

* Described by the inventor, in this Journal IV. 163.

doubled

doubled or trebled, by proportionably encreafing the defcending mafs, and raifing feveral columns of water from different elevations at the fame time.

To extend therefore the utility of the invention by illuftrating what has been here premifed is the intention of the prefent communication.

A and B, in Pl. XV. Fig. 1. reprefent the veffels which contain the rifing and defcending bodies of water: they are fpherical in the original drawing, (Pl. VIII.) but to leffen the lofs of fpace in defcent, they are here made flat and cylindrical.

E the higher, and C the lower ciftern of the original figure.

F, a veffel the fame as A, with tubes 1, 3, and 6; it communicates with the veffel B, by the tube 1; and is intended to raife water out of the ciftern E into a higher and additional ciftern G.

2. enlarged tube of the original drawing.

5. hole in the top of B, inftead of a tube. This hole and the tubes 2, 4, 6, 6, muft be provided with valves, all which, while water is rifing muft be kept clofe by weights or fprings, except the valve to tube 2, which muft be open.

The tubes 3, 3, may alfo have valves to fupport the raifed columns.

To make the engine operate without attendance, a veffel containing a fyphon muft be fixed to the fide of the ciftern C, and fupplied with a proper quantity*. Every external valve, except the loweft, muft have a lever joined to it; and while clofing their refpective tubes, they fhould be connected together, and to the lever of the valve to pipe 4, which muft fufpend another empty veffel and fyphon.

The valve of pipe 2, muft alfo be fufpended by a chain or wire from the underfide of valve to pipe to 4, and when one clofes the other muft open.

Fill now the cifterns C and E with water, and let the lower be conftantly fupplied: the veffel on the outfide will foon become full, and the fyphon will empty the water into the lower veffel, which, by its weight will open the valves of the tubes 4, 5, 6, 6, and clofe the valve of tube 2: the veffel B, being filled through the hole 4; the defcent of water through the higher fyphon having ended; and the lower veffel alfo being nearly emptied by its fyphon; the valves will clofe the tubes 4, 5, 6, 6, and the valve of tube 2 will open; the water will then begin to defcend out of B, and raife one body of water out of C into A, and another out of E into F. When B is nearly empty, or A and F are full, water will again defcend through the fyphon of the higher veffel into the lower, which will open the tubes 4, 5, 6, 6, and clofe 2: B will fill a fecond time, and the veffels A and F will empty themfelves into their refpective cifterns E and G. And thus the reciprocations continue without interruption.

Another body of water may be raifed out of G into a higher ciftern by additional apparatus, and by proportionally encreafing the dimenfions of B and the tube 2.

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* The author evidently means to refer to the papers of Mr. Bofwell and Mr. Clofe, at pages 117 and 293 of our prefent volume. N.

The

The dotted lines reprefent apparatus for raifing water below the bottom of tube 2, to be ufed inftead of thofe above the ciftern C.

This arangement places the utility of the engine in a very ftriking point of view, particularly when we wifh to employ a fmall ftream to operate upon machinery; where the defcent is great, but the quantity of water inadequate to the purpofe even with the affiftance of a refervoir. In this cafe let nearly the whole of the water defcend through the veffel B and tube 2, before it enters the refervoir; and let the water after it has turned the wheel be collected in a lower bafon. If the defcent from the higher refervoir be fo great as to allow part of the lower body to be raifed into it again at once, the quantity raifed may be made nearly equal to that which has defcended through the tube 2; if it can be done at twice, it will be lefs than half that quantity. Still however I think it will be very advantageous to employ fuch a method, as the operation of the engine is conftant and diminifhes no power of the machinery *.

In fituations where this machine can be erected it may be of confiderable ufe, for raifing water out of mines, for draining pieces of ground, or elevating the water employed in domeftic purpofes.

<div style="text-align:center">

I am Sir,

Your humble Servant,

</div>

Sept. 16, 1800.   L.

<div style="text-align:center">

III.

Obfervations and Experiments on Light and Heat, with fome Remarks on the Enquiries of Dr. Herfchel, refpecting thofe Objects. In a Letter from Mr. JOHN LESLIE.

To Mr. NICHOLSON.

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S I R,

<div style="text-align:right">

London, Oct. 9, 1800.

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I AM induced to trouble you with this communication in confequence of the account inferted in your laft number, of two papers lately read at the royal fociety. Aware that the celebrity which their author has fo juftly acquired as a fkilful and indefatigable obferver will, from the indolence of our habits, confer a certain extrinfic weight, even when he embarks in philofophical fpeculation,—I cannot help feeling, anxious, left *that* authority fhould in the prefent inftance retard the progrefs of fcience by giving currency to opinions which, I am fully convinced, are inacurate or unfounded. It is not my intention to cenfure the efforts of genius; but I would fuggeft how liable are men of talents to commit miftakes in matters

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* The reafoning in this paragraph appears to be defective, becaufe the machine engages part of the fall, and thus diminifhes the effect more than the raifed water can augment it.   N.

<div style="text-align:right">

of

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of experiment, efpecially if they difregard analogy and the general concurrence of eftablifhed facts. It would appear that this able aftronomer, on entering into a new line of enquiry has neither employed apparatus fuited to the nicety of the fubject, nor guarded fufficiently againft the numerous and latent fources of error. I confider myfelf entitled to fpeak with the greater confidence, becaufe I had long directed my refearches in the fame channel, and had fortunately hit upon a fimple contrivance, improved fince by perfevering attention which afforded to fuch inveftigations a degree of precifion and facility not hitherto attained. I do not hefitate, therefore, to maintain that Dr. Herfchel's capital propofition originates in fallacious obfervations. To fupport this affertion I will briefly and with reluctance ftate the refult of my enquiries, and will then offer fome remarks or ftrictures on the article in queftion.

In defcribing my *photometer* I noticed, in a curfory manner, the feveral points in optics and chemical philofophy for the examination of which it was, by its peculiar delicacy, fo happily calculated. It was not then my defign to enter into details, or relate the fuccefs of the experiments. Thofe interefting deductions I referved for a work which has been feveral years in contemplation, but of which I have deferred the publication from time to time as the materials accumulated, from a defire ftill to extend and correct my views, and perhaps from a growing indifference to the voice of public opinion, which I am forry to obferve is, in this country at leaft, notorioufly fervile and undifcerning. Nor will I at prefent anticipate more than what immediately concerns the fubject under difcuffion.

After conftructing the inftrument one of my firft objects was to determine, by their calorific effects, the relative intenfities of different colours. The problem had indeed been attempted before, by Franklin, Watfon, and others; but from fuch rude trials nothing in any degree accurate could be expected. I therefore coated the abforbent ball of a photometer fucceffively with a feries of water-colours, and, after allowing it to dry, I placed it with another photometer in the fame fteady light, whether expofed to the diffufive influence of the fky, or to the direct impulfion of the folar beams. The ratio of the quantities marked by the two inftruments gave the proportional abforption of the coloured fubftance, and confequently its power of reflection. Inftead of paint, I fometimes covered the ball with bits of coloured filk; and when two colours fhowed nearly equal effects, I had recourfe to an expedient which rendered their difference very fenfible. I made the balls of the photometer larger than ufual, brought them ro the fame elevation, and bent them back in oppofite directions; the glafs-cafe confifted of the larger fegment of a hollow fphere furmounting a cylinder; the zero of the fcale was placed about the middle of one of the branches, and the divifions were marked both in afcent and defcent: on applying the approximate colours to thofe counteracting balls the difference, whether in excefs or defect, of their abforbent powers was eafily determined. Thofe experiments were likewife varied by reflecting the rays of the fun, at a given angle from a coloured furface, againft the dark ball of a photometer. I need not defcend to particulars, but I was ftruck with the copious reflection from fcarlet, little inferior to that from a white furface. Blue, and next to it green, reflected

the fmalleft quantities of light. Hence the explanation of feveral facts which occurred in the application of the photometer. Expofed to the fun-beams only, it indicated about one hundred degrees at noon during the fummer months in our climate; fheltered from thofe direct rays it gives feventy or eighty degrees, when the fky is covered with fleecy clouds, but only ten or fifteen degrees if the atmofphere is clear and of azure hue. On the fummits of elevated mountains, the fun would be more refplendent while the vault. of heaven, of the deepeft blue, would emit a proportional feeble light. The red glofs of a fire was found to make at leaft three or four times as much impreffion as the flame of a candle, under the vifual angle. And language, that grand monument of human thought and feeling, teftifies the conformity of thofe remarks with common fentiment. Red and orange are termed *warm* tints, and we admire the *glow* of the Italian landfcape. Among the colours fcarlet is reckoned glaring, green, foft, and blue, faint. The pleafurable fenfation excited by the fight of green, independent of its affociation with verdure and vegetable life, has been referred to the circumftances of its occupying the middle rank of the fyftem of colours. But green has more affinity to the dark and abforbent fhades, and the eye feems more gratified with deficiency than excefs of light.

On a hafty trial, I perceived that, the coloured fpaces of the prifmatic fpectrum manifefted analogous properties; but as the quantities were fmall and required nicer obfervation, and as the feafon was already far advanced (it was in October 1797), I refolved to poftpone the whole till the next fummer. I prepared accordingly to refume the inveftigations with every advantage; but I am forry to fay that my plan was not accomplifhed fo foon as I expected, owing partly to our variable fky, fo difcouraging for optical experiments, and partly to various unforefeen interruptions, efpecially that of undertaking a tour through the north of Europe. In the beginning of laft fummer, however, my wifh was realized, yet not without a tedious wafte of time; for fuch is the nature of this infular climate, that the fky at mid-day is feldom cloudlefs, and, even then, its azure is generally adulterated by a milky haze. I cut a broad horizontal flit in the window-fhutter, fufficient to admit freely an excellent prifm of flint glafs, which was fixed with wedges at the ends in a pofition to produce the greateft refraction. Behind was placed a ftand about $1\frac{3}{4}$ inches broad, $\frac{3}{4}$ of an inch thick, and of the height of 5 or 6 feet, to which an arm projecting 2 or 3 inches from the edge was adapted to flide upwards or downwards at pleafure, and to this the photometer was attached; and, to avoid as much as poffible every chance of deception, I annexed to that arm a flender branch ftretching three or four inches towards the window, and bearing a fmall fcreen capable of being raifed or depreffed as occafion required, fo as to intercept at a diftance the whole of the coloured rays but thofe fubmitted to examination.

I fhall omit mentioning other precautions ufed; to thofe who are acqainted with accurate experimenting it would be fuperfluous, and to the reft of your readers it would appear difguftingly minute. The ftand was approached as near to the window as was compatible with the developement of the feveral coloured fpaces, which was at the diftance of about two

feet;

feet; and though the fhutter was clofed, fufficient light entered by the edges of the prifm for obferving the inftrument. In two or three minutes, as the colour travelled along the black ball, the fluid generally attained the point of equilibrium. Photometers of both conftructions were employed with fimilar refults. The obfervations were frequently repeated, and from the mean of them all it appears; that, diftinguifhing the fpectrum into four equal portions, which coincides with the blue, the green, the yellow, and the red the correfponding intenfities or calorific meafures, are 1, 4, 9, and 16 degrees, as in fig. 2. Pl. XV. Hence the gradation of thofe intenfities will he denoted by a parabola referred to its vertical tangent. A variety of curious propofitions may hence be deduced. Thus, fince the complimentary fpace of the parabola is the third part of the circumfcribing rectangle, it follows that the intenfity of the red rays feparated by a prifm of flint glafs is triple that of the compound white beam; and for the fame caufe the blue rays are more than five times feebler than the common ftandard. It is not without reafon that I ftate the particular fort of prifm ufed, becaufe with one of another fubftance the colours might be expanded after proportions materially different, which would of courfe give different refults; for the force of a pencil of coloured light muft evidently depend on the denfity, as well as on the fpecies of the rays.

The important difcovery of the achromatic telefcope decifively evinces that refrangibility is not a property inherent effentially in the particles of light, but refults from the fpecific relation or affinity between thofe various particles and the refringent medium. In what proportions the feveral rays enter into the compofition of the folar beam has not been determined, nor does any mode of folving the problem eafily occur.

It is remarkable that all what is really valuable in Newton's optical difcoveries was announced at an early age, but encountered fuch oppofition or experienced fuch neglect, as to affect that great man with chagrin, and to extinguifh completely the defire of communicating his views at large to the world. Nor did he refume that tafk till near the clofe of life, when worn down with infirmities, and his choiceft faculties wafted in the compofition of laborious works, which, for his credit with pofterity, had better been fuffered to reft in oblivion. Yet even thefe (fuch is the effect of exalted reputation!) were now accepted with indifcriminate applaufe. Had his treatife of optics been publifhed at an earlier and happier period, it would certainly have been more fober and correct. The feptenary and mufical divifion of the prifmatic fpectrum is devoid of foundation, and too plainly betrays a tincture of the myfticifm of the age. It is equally ftrange and mortifying to obferve the moft objectionable part of that fyftem, and which is directly confuted by the theory of achromatic glaffes, ftill repeated in popular books, and even admitted by fome authors of a higher clafs.

While I was occupied with thofe obfervations in Fifefhire, fome vague accounts reached me of a paper communicated to the Philofophical Society at London. And whatever my fentiments were refpecting the validity of the conclufions, I refolved calmly and impartially to fubject the pretended facts to the teft of experiment. When a pho-

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tometer was placed beyond the fpectrum, though it approached the margin, whether above, below, or at the fides, no effect whatever was perceived. And as that inftrument greatly furpaffed, in fenfibility and regularity of performance, the fineft thermometer, it is only what might be expected, that I could not perceive any alteration, if I employed thermometers of uncommon delicacy and of various conftructions, with black or pellucid bulbs, having their fcales fubdivided into tenths of a degree, and capable of indicating ftill fmaller differences. There is a circumftance, however, which being overlooked, might lead to egregious errors. If the fpectrum be received on the ftand, the inftrument brought near it will be very fenfibly affected; owing partly to the light reflected, but chiefly to the action of the heated air accumulated over the illuminated furface. In fhort, the operations are deranged in every cafe where the inftrument is not completely infulated, and is remote from all folid materials which might detain the light. And it is worthy of remark, that the afflux of light muft raife the temperature of a flat furface of imperfect conducting fubftance, fuch as wood or pafteboard, more than four times as much as it will the blackened ball of a thermometer; for, in the former cafe, the heating and cooling caufes are exerted on the fame fpot, but in the latter, the quantity of light or heat received is only as the central fection of the ball, while the whole furface, which is quadruple that fpace, is expofed to the cooling influence of the ambient air. There are other circumftances, too, which tends to augment that difference of effect, but which I fhall not ftop to mention. On the whole it is moft difficult, where an active fource of heat exifts, to obtain an uniform temperature, particularly in a fmall room; and I might point out caufes, not yet apprehended, which will notably affect a thermometer.

Thefe hints will ferve as a clue for detecting Dr. Herfchel's miftakes. The firft circumftance that begets fufpicion is the large quantities marked by his thermometers, and which are not much inferior to the full force of the fun-beam. To procure the colours of the fpectrum diftinctly evolved, the folar image muft be extended to ten times its breadth; nor can it be imagined that rays fo attenuated could produce fuch palpable effects. The Doctor, indeed, admits that a fmall thermometer was lefs affected. Yet it is obvious that, if the action was due merely to the afflux of light, the fize of the ball ought to occafion no alteration, fince, if it prefents lefs furface to the beam, it likewife, in the fame ratio, expofes lefs furface to the furrounding air. A very curious reafon is affigned—" the *cooling* agency of the *afcending* ftream of air, which would act more " ftrongly where the proportion of furface to bulk was greater *." The idea of an

afcending

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* I muft in fairnefs take notice that the words here quoted from Philof. Journal IV. 321, are not **Dr.** Herfchel's but mine, as the abridger or narrator of the contents of his paper. The Doctor's obfervation (Philof. Tranf. Y. 1800. page 261.) ftates in very general terms that the *cooling caufes* may, he fuppofes, have a ftronger affect on the fmall than the large thermometer. I do not here quote more fully, becaufe the point feems not of fuch importance as to demand it, and becaufe the only miftake refpecting it appears to lie with the ingenious author of the prefent communication. His fecond reflections will not, furely, permit

him

afcending cold ftream is certainly original; at leaft, it ftands in direct oppofition to all the eftablifhed principles of philofophy, though it is not calculated to infpire the moft favour‐able opinion of the' author's acumen in phyfical inquiries. The tardy progreffion from violet to red compared with the numbers which I have affigned, feems to betray the ac‐ceffion of extraneous influence. Befides, the dark ball of a thermometer will, for the moft part, receive its full impreffion in the fpace of one or two minutes; but the Doctor's thermometers required, or at leaft were allowed, an expofure of ten or thirteen minutes. This fact affords a ftrong prefumption that other matters were brought into action, capable of a more prolonged abforption, and of a greater accumulation of heat, than a mercurial bulb. Nor can I hefitate to impute the principal derangement to the platform on which the thermometers were laid, which, receiving light, though partially, would, as before obferved, acquire heat in a quadruple ratio, and communicate this to the contiguous ftratum of air. The fame confideration will explain the origin of the miftake on which the paradoxical affertion is grounded, of " invifible refrangible light." Indeed, a more ob‐jectionable plan of conducting experiments could fcarcely have been devifed. The prifm was fixed in an inverted pofition, to direct the rays downwards upon a table, which was covered with white paper; and the thermometer was placed a little beyond and above the edge of the red light. In fuch a fituation no wonder that the bulb was notably affected, being immerfed in a warm atmofphere which extends to a certain diftance over the illu‐minated fpace, and receiving likewife a large portion of the feveral coloured rays reflected from the paper. That joint effect would be ftill further increafed by the thermometer having its bulb directly oppofed to the afcent of the heated air. The action might even be greater than if the bulb were placed in the courfe of the red rays, fince it would then in‐tercept that light which would have produced an accumulated effect on the wood below. But why is the *maximum* of invifible light ftated at half an inch beyond the red rays? Does it move in ftraight lines? Does it diverge at a certain angle, or has it various degrees of refrangibility? Thefe were queftions to be refolved. And after all, what has *invifible* matter to do with that peculiar ftructure of the furface of bodies which conftitutes black or white?

Should any doubt remain after this long examination, there is a fingle fact which at once demolifhes the whole fabric. If thofe invifible calorific rays had any real exiftence, the action of a burning glafs would be moft powerful, not at the proper focus, but a con‐fiderable way beyond it. For the fame reafon, the hole burnt in a black piece of cloth would not be confined to the lucid focus, but would include a ring fwelling all round to more than double that diameter.

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him to deny that every current of air which afcends, in confequence of the proximity of an heated body, muft be cold when compared with that body itfelf; or (to keep more ftrictly to the expreffion alluded to) that it cannot receive the heat which makes it afcend, without cooling, *or exerting a cooling agency on,* the body which heats it. N.

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I have already extended this paper beyond the limits I firſt propoſed: I will not, therefore, fatigue your readers by attempting the refutation of ſome obſcure and verboſe arguments advanced by Dr. Herſchel, which involve a ſingular ſpecies of logic, and ſeem repugnant to the fundamental principles of dynamics. The hypotheſis of inviſible light is not new in the ſcientific world. It was ſuggeſted by the radiant heat of Scheele and the radiant cold of Pictet. It was propoſed or adopted by the late very ingenious Dr. Hutton. The facts, indeed, on which it was grounded may be ſatisfactorily explained from known principles. It was not conſiſtent with ſtrict metaphyſics; and to beſtow the property of being reflected on inviſible light, was ſurely ſtretching the limits of probability. Yet was the hypotheſis, in ſome degree, plauſible and alluring. The little improvement of aſcribing to it likewiſe refrangibility, by rendering the whole abſurd, has diſſolved the charm. What is the eye itſelf but a compound priſm? And is not the expanſion of the optic nerve adapted by its conſtitution to receive impreſſions through the diaphanous ſubſtance of the humours and coats, and to convey their appropriate ſenſations? But thoſe are only ſenſations of light. Senſations of heat are confined to no particular claſs of nerves. Refrangibility is therefore correlative with viſion, and " inviſible refrangible light" ſeems a contradiction of terms. But all metaphyſical conſiderations apart; if the image of the ſun be not encircled with a broad lucid ring, on fixing our eye on that luminary, the ſenſation of heat ought not to reſide in the correſponding ſpot on the retina, it ſhould be more intenſely felt over the ſurrounding ſpace.

Let me conclude by recommending to your inquiſitive readers two works of very ſuperior merit. I mean the *Traité d'Optique* of Bougner, and the *Photometria* of Lambert. The public has a right to expect that authors have previouſly ſtudied the labours of their predeceſſors; but it would be charity to believe that ſome late writers had not conſulted thoſe excellent models, which might have prevented much unneceſſary repetition, and corrected ſeveral groſs errors.

<div style="text-align:center">

I am, SIR,

Your moſt obedient ſervant,

JOHN LESLIE.

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IV.

Account of a Memoir of M. PROUST, on ſeveral intereſting Points in Chemiſtry*.

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THE firſt article relates to the *converſion of camphor into oil* by repeated diſtillations from a bolar earth. This operation, he obſerves, was long ſince deſcribed by Neumann, and he appears to reproach Citizen Lagrange that he has not quoted this author in his Memoir

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* Tranſlated from the Report made to the firſt Claſs of the Phyſical and Mathematical Sciences of the Inſtitute of France by Cit. Vauquelin; Guytôn and Vauquelin being commiſſioners. The report is inſerted in the Annales de Chimie, XXXV. 32.

<div style="text-align:right">upon</div>

upon Camphoric Acid*, in which he has treated of the converfion of camphor into oil. Excepting this, nothing is faid upon the fubject by M. Prouft but what is already univerfally known.

In the fecond article, which is more interefting than the firft, he communicates fome methods, more fimple than thofe which he had previoufly pointed out, of *obtaining pure tannin*, the principle of which he has obtained from the excellent Memoir of Citizen Deyeux upon the Nut-gall.

The procefs confifts in pouring a folution of carbonate of pot-afh into an infufion of galls. By the mixture of thefe two liquids, a yellowifh white precipitate is formed, in the form of a cogulum, which Ribaucourt took for an earth, and which it is only neceffary to wafh with a little cold water to obtain the pure tanning matter. But care muft be taken not to wafh it with too much, nor with warm water; for however little foluble this matter may be, it is neverthelefs enough fo, to difappear totally in a tolerably large quantity of water. Hence it follows, that in order to enfure complete fuccefs in the operation, the infufion of galls muft not be too much diluted with water, fince in that cafe there will be no precipitation.

It is no lefs effential that the alkali fhould be perfectly faturated with the carbonic acid, becaufe an excefs would favour the folution of a certain quantity of the tannin, which would be greater in proportion as the alkali was more cauftic. Thus it is feen that the tannin is feparated from its folution by an alkaline carbonate, not as might be fuppofed by faturating the gallic acid but by engaging its water of folution; for all the falts which have a certain degree of affinity with this liquid in the cold produce the fame effect.

But however fmall the quantity of water that may be ufed, and whatever its temperature may be, fome tannin always remains in folution; becaufe it is foluble in a certain proportion even of the coldeft water. The greateft part of this matter, which thus remains in folution, may be obtained by evaporating any portion of the liquid, and part of the tannin feparates by cooling, which is more or lefs in proportion to the quantity of water evaporated. The tannin, when it has been thus feparated from the other fubftances which accompanied it in the galls, has the appearance of a glutinous pafte, of a yellowifh grey colour, the parts of which adhere to each other in a confiderable degree. It dries with difficulty; but when it is fpread in thin layers upon earthen plates, and afterwards expofed to the heat of a ftove, it firft melts, then becomes dry, and at laft acquires the appearance of a kind of yellow refin, with a vitreous fracture. This tannin, fubmitted to diftillation, affords a faline liquid, in which the fmell of ammoniac is diftinguifhed, and which poffeffes the property of blackening folutions of the red oxide of iron; a property which muft be attributed to a fmall quantity of tannin volatilized without alteration, and not to the gallic acid, becaufe it does not afford a green colour with alkalis.

* Philof. Journal, II. 97.

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The oil which tannin affords by diftillation is very fmall in quantity, and fo thick, that it remains attached to the neck of the retort. Its coal is bulky, and forms $\frac{1}{38}$ or 0.026 part of the diftilled mafs.

The liquid, from which the tannin has been feparated by the carbonate of pot-afh, foon becomes green in the air, the oxygen of which it abforbs.

Sulphurated hydrogen deftroys this green colour by uniting itfelf, in its turn, to the oxygen. Though the gallic acid renders tannin more foluble in water, as was remarked by Citizen Deyeux, neverthelefs M. Prouft does not believe that it is by combination with this acid that alkaline carbonates precipitate the tannin, fince they alfo feparate this matter from a fimple folution in water, and becaufe moft of the very foluble neutral falts alfo pre-cipitate it. It is, therefore, fimply from a ftronger affinity of thefe falts to water that thefe different fubftances feparate the tannin from that fluid. The gallic acid poffeffing, how-ever, the faculty of diffolving a certain quantity of this matter, and the carbonate of pot-afh poffeffing, at the fame time, the double power of faturating this acid and of uniting itfelf clofely to the water, it ought, under like circumftances of folubility, to leave lefs tannin in folution than the other falts.

M. Prouft obferves, that thefe facts, unimportant as they may feem to be, fhould put us upon our guard againft the effects which take place between the folution of the tanning principle and animal fluids; indeed, fince the faline materials, of which thefe liquids are never deprived, are able to precipitate the tanning principle, it ought not henceforth to be concluded from the appearance of the precipitate, at leaft not till the nature of the depofi-tion has been examined, that animal fluids contain gelatin.

A piece of the mufcle of an ox, or of raw hide, fpeedily difcolours the infufion of gall-nut, and leaves nothing but gallic acid in the liquid. When thefe fubftances are faturated with the tanning principle, they eafily dry without putrefaction; their fibres then feparate and crumble into duft between the fingers, like wood which is worm-eaten. M. Prouft very rationally confiders the galling of wool, filk, and cotton, as a fpecies of tanning; in which, by taking care not to carry the combination too far, the animal fubftance is made to acquire confiderable hardnefs and refiftance to the action of water, without lofing too much of its flexibility, befides rendering it unfit to become the food of infects. He alfo demands whether the folution of tan would not anfwer the purpofes of embalming better than any of the ingredients which have yet been made ufe of.

Citizen Dizé is the firft perfon who obferved that by pouring fulphuric or muriatic acid upon a rather ftrong decoction of galls, a confiderable depofit is formed. M. Prouft availed himfelf of this property to feparate the tanning principle from the gallic acid with which it is mixed in the galls. It is, in fact, this fubftance which, by combining itfelf with the acids, becomes infoluble, and is precipitated from the water in the form of an adhefive pitch. When the precipitation is complete, the liquid which contains the gallic acid is decanted; the precipitate is wafhed with cold water; it is then diffolved in boiling water; the fulphuric acid is faturated with carbonate of pot-afh, and the tanning principle

becomes

becomes precipitated as the pot-afh unites itfelf to the acid, and the fulphate of pot-afh, which has been formed, diffolves in the water. In order to obtain the whole of this fub- ftance, the liquid muft be reduced by evaporation, and the depofit muft be fuffered to become cold, and wafhed with cold water. It is the pure tanning principle.

After defcribing the proceffes, which we have juft related, for feparating the principles of the nut-gall, namely, the tanning principle and the gallic acid, M. Prouft next examines what takes place between thefe fubftances and iron in the making of ink. He remarks, firft, that the reafon why ink is not formed with a folution of iron at the minimum of oxidation is, becaufe the tanning principle and the gallic acid have lefs affinity with the iron than the fulphuric acid has. 2dly, That when a folution of iron at the maximum of oxidation immediately produces a black colour with an infufion of galls, the reafon is, that in this cafe the red oxide of iron has a greater attraction to the principles of the gall-nut than to the fulphuric acid. He proves this by diffolving the combination of iron with the tanning principle in fulphuric or muriatic acid, which gives a blueifh tranfparent liquid, which would lofe its colour if decompofition took place. He, therefore, confiders ink as a folution of the tannate and the gallate of iron in fulphuric acid. 3dly, That when ink, fpread upon paper, quickly becomes black, it is becaufe it abforbs an additional quantity of oxygen, which renders it more infoluble in water. The combination of the oxide of iron with the gallic acid and with the tanning principle, which forms the bafis of ink, contracts only a flight adherence with the acids, and feparates itfelf from them by repofe at the end of fome time. In this cafe it is affected nearly in the fame manner as metallic falts diffolved in a foreign acid; and for this reafon it is that when a plate of iron is put into a folution of ink, this metal precipitates the black particles, abfolutely in the fame manner as iron feparates the phofphate of iron from acids.

Alkalis, cautioufly added to the folution of ink, precipitates its black parts, but an excefs of alkali diffolves the atramentary combination, and produces a fluid of the colour of wine, more or lefs intenfe.

Gallic acid produces no effect on ink, becaufe the oxide of iron is faturated. The fame acid is not very active in fhewing the prefence of red oxide in a folution containing an excefs of acid, becaufe the combination is diffolved in proportion as it is formed. But by carefully faturating the excefs of acid, the liquid is rendered black.

The combination of the gallic acid, and the tanning principle with iron, does not become changed into pruffiate of iron by means of pruffiate of pot-afh, even with the affiftance of heat. The nitric acid diffolves the ink, and effects no change in its nature in the cold; but by heat it deftroys it, and the yellow oxalate of iron is precipitated by the addition of am- moniac. Whenever iron filings and an infufion of galls are mixed together, a difengage- ment of hydrogen gas takes place; which proves, fays M. Prouft, that the oxidation of iron begins at the expence of the water, and that it is completed by the air, an event which alfo takes place with vegetable acids, which, not fufficiently attracting the oxide of iron oxided to the minimum, cannot counterpoife the affinity of the oxigen which tends to ad-

vance it to the maximum; and this is the reafon why the acetates of iron pafs fo rapidly to a red colour, that it is impoffible to obtain them green in a folid ftate.

M. Prouft prefers, both for durability and beauty, ink which is made by a folution of iron in an infufion of the gall-nut, to that which is prepared from fulphate of fteel. His reafons are plaufible. The infufion of acorns, concentrated and digefted with iron filings, prefented the fame phenomena as the infufion of the gall-nut, and afforded M. Prouft an ink, which, when mixed with a fmall quantity of vinegar and a fufficient quantity of gum, was inferior to none in its good qualities.

From the experiments of M. Prouft, it follows, that there is nothing except the red oxide of iron which can make ink with the principles of the gall-nut; that the fulphates of iron, which are made ufe of for that purpofe by ink-makers, containing always different quantities of red oxide, there as many different kinds of ink as there are recipes for making of it; but let thefe inks be only once fpread upon paper, and the air reduces them all to nearly the fame ftate.

Every one is acquainted with the property poffeffed by inks which are too much diluted; of becoming black after they are fpread upon paper by the pen; but this phenomenon may be produced in a much more ftriking manner. It is only neceffary to pafs fome fulphurated hydrogen gas into the ink, and the black colour will inftantly difappear; but afterwards, in writing, it is very pleafant to obferve this clear liquid become very fpeedily black.

Such are the principal facts which have been communicated by M. Prouft refpecting the combination of iron with the principles of the gall-nut; namely, the tanning principle and the gallic acid. They ferve to illuftrate the theory and the practice of the arts of dyeing and of ink making. He has promifed the Inftitute to purfue his refearches on this fubject ftill further.

In another article, M. Prouft propofes *a method of obtaining fulphuric acid from the refidues of the diftillation of fulphuric ether.* This fimple procefs confifts in diluting the refidue with two parts of water, filtering through a cloth to feparate the coaly fubftance from it, and fubjecting the liquid to diftillation in a glafs retort. When the acid has acquired 1.84 of fpecific gravity, from 4 to 6 grammes (parts) of faltpetre to each demikilogram (five hundred parts) are added; and the diftillation is continued till the fluid is perfectly white, and weighs from 1.86 to 1.87, water being 1.00.

M. Prouft prefers an iron frame to a fand bath to place the retort in for thefe diftillations; and he gives very good reafons for the preference. He computes that the profit of purifying the refidues of ether in this manner will be confiderable; fince it affords between a fourth and a third part of fulphuric acid concentrated to 1.87 fpecific gravity.

The fulphuric acid, feparated from the butuminous matter before it is rectified, has been fubjected to fome experiments by M. Prouft. He obferved, that alkalis, as well as the pruffiates, did not precipitate any thing. It abundantly decompofes hydro-fulphurated

water

water by virtue of the fulphureous acid it contains : oxigenated muriatic acid difcolours this brown liquid ; which, in his opinion, proves that this colouring material refembles the juices of vegetables. The coaly fubftance which is feparated during the formation of fulphuric ether has alfo been made an object of M. Prouft's refearches : when dried it re-fembles lamp black (*noir de fumée*) as well in its black colour, its brittlenefs, and its fhining colour. If expofed to heat, it emits a fmell of fulphureous acid mixed with that burning lac. Boiling water diffolves 0.24 parts of this fubftance, alcohol as many as 0.44, and this folution is not rendered turbid by water. Thefe two folutions are proper for the formation of brown dyes by means of the muriate of tin.

The folution of the coaly fubftance of the ether in alcohol affords, by evaporation, a black friable and nearly infipid powder. 5.31 grammes (100 Fr. grains) of this fubftance diftilled produced an aromatic water, light and empyreumatic oils, vegetable acid mixed with a little fulphuric acid, and carbonated oily hydrogen gas, mixed with about a third of carbonic acid. The coal which remained in the diftilling veffel weighed 0.64 grammes (12 Fr. grains) ; it left, after combuftion, fome afhes, in which analyfis difcovered the pre-fence of lime and magnefia. M. Prouft, indeed, fufpects that there is a fmall quantity of filiceous earth and alumine in it. Here, then, obferves this fkilful chemift, we have earthy afhes, which accompany carbon even into alcohol.

M. Prouft concludes that the principles of the alcohol in forming ether by the action of the fulphuric acid become united in other orders, and produce a fubftance analogous to the juices of vegetables, fince, during the fummer, it is covered with mouldinefs like a vegetable decoction.

M. Prouft alfo difcovered that, during the diftillation of phofphorus, a combination of this fubftance with charcoal is conftantly formed, and that it is this combination which remains in the chamois leather after phofphorus has been paffed through it for its purifica-tion, after the manner of Pelletier. It is of a red colour, and does not melt like pure phof-phorus. If it be diftilled by a gentle heat, a portion of phofphorus is feparated, which exceeded the point of faturation ; but the true combination is not decompofed, unlefs the degree of heat be confiderably augmented. When the veffels become cool, a powder re-mains of lively orange red, light, floculent, and very homogeneous through its whole mafs : M. Prouft confiders this product as an intimate union of carbon and phofphorus. The following are fome of its properties which he defcribes. If, whilft it is ftill in the retort, the temperature be fufficiently raifed to ignite the bottom, a frefh quantity of phofphorus is fublimed, and the refidue is then mere coal. It burns rapidly when placed upon a plate of hot metal ; but the coal, by imbibing phofphoric acid, eludes the com-buftion. By expofure to the air, it foon lofes this difpofition to take fire, and may then be preferved without any danger. It is without fmell or tafte. It is this combination, according to M. Prouft, which forms the red refidues which remain without alteration in the apparatus in which phofphorus is prepared. Its properties ferve to explain the origin of a black powder which fome chemifts have obferved in phofphorus. There cannot be a

ftronger

ftronger proof, continues M. Prouft, that the phofphuret of carbon is a combination formed by virtue of the laws of affinity, than its unchangeablenefs by the folution of cauftic pot-afh, even with the affiftance of heat. Time did not permit the author to determine the proportions of the elements of the phofphuret of carbon. He promifes a plentiful harveft of new facts to thofe who may be difpofed to purfue this fubject, and follow its combinations with metals and other combuftible bodies.

On this occafion M. Prouft corrects or recalls an opinion which was given in the Journal des Mines, refpecting the fmell of hydrogen gas which becomes difengaged during the folution of certain kinds of caft iron and fteel in acids. He conceives that it is owing to the prefence of an effential oil rather than to that of phofphorus, as had been fufpected. He gives the following obfervations in fupport of his prefent notion: 1°. The neck of the matraffes, retorts, and receivers, in which the inflammable gas is prepared, are greafed with little drops of this oil. 2°. If 15 ounces of black crude iron be diffolved in fulphuric or muriatic acid, drops of oil are obferved floating on the water of the receiver. 3°. If the wafhed carburets, which are obtained from crude iron, be gently diftilled, water comes over along with an oil of the fame fmell; and alcohol, in which fuch crude iron has been macerated (in powder) becomes white on the addition of water. M. Prouft thinks that if the inflammable gas, which is obtained from crude iron, were carefully examined, it would be found to hold oil in folution, and that it is this fubftance which gives it its fetid fmell. If the fact be accurately fo, of which we have no doubt, it is, as M. Prouft obferves, a grand ftep towards the explanation of the phenomena of vegetation, and the underftanding the tranfition of mineral fubftances into vegetable products.

M. Prouft thence paffes to the examination of the native of the iron of Peru. This fpecies of iron, which has been noticed by Rubin de Celis, is of a greyifh white colour. It bears a rather ftrong refemblance to certain native filvers, for which it was for fome time miftaken. It poffeffes a confiderable degree of ductility, does not ruft like common iron, and has nearly the fame habitude under the file.

5.31 grammes (100 grains) of this iron, diffolved in fulphuric acid, only afforded 3520 centimetres (176 Fr. inches) of hydrogen gas; whilft the fame quantity of common iron wire gave 200 inches. At the commencement of the folution, a fmall quantity of carbonate of iron was feparated, which difappeared towards the end.

The colour of the folution, which was much greener than ufual, led M. Prouft to fufpect that there was copper in this iron; but fulphurated hydrogen not having precipitated any thing, and the liquid having preferved the fame colour as before, it appeared to him that no other fubftance but nickel could produce thefe effects. In order to have a certain proof of this, he oxided the iron ftrongly by means of nitric acid, and afterwards precipitated it by the gradual addition of pot-afh. The iron being thus feparated, the green colour of the liquid affumed a greater degree of intenfity, and by completing the precipitation, after filtering, he obtained a fubftance which, with fulphuric acid, yielded

3   fulphate

fulphate of nickel. This method of analyfis, which M. Prouft has indicated, in combination with the ufe of fulphurated hydrogen, was found by him to be equally fuccefsful in the complete feparation of nickel from iron, arfenic, and copper, which are almoft always met with in the ores of this metal.

The native iron of Peru is therefore, according to the experiments made by M. Prouft, an alloy of iron and nickel; a new difcovery of the moft interefting nature. The prefence of nickel in this alloy, obferves the author, appears to announce that it is the product of art; but when it is confidered that there exifts a mafs of more than 1363 myriagrams (300 quintals) in a plain of more than 100 leagues in circumference, where there is neither mountain nor water, nor fcarcely a ftone is to be found, the difficulty of the problem ftill remains in all its force. Laftly, adds M. Prouft, if the power of uniting thefe metals in fuitable proportions can be obtained by metallurgifts, they will have obtained an alloy which will poffefs many advantages over other iron, and more particularly that of not being liable to ruft.

M. Prouft's memoir is concluded with fome detached facts, as well on animal as on mineral fubftances. 1ft. A myriagram (20 Fr. pounds) of beef, of which 25 hecotograms (5 pounds) were bone, produced only one pound of common brown extract, which was elaftic, and of a tafte fimilar to concentrated or very ftrong foup. It would be impoffible to convert this extract into portable paftels without the addition of a rather large quantity of the jelly of bones. Whenever beef is boiled in filver, this metal becomes black, as is the cafe when it is ufed for the boiling of whey, frefh urine, &c. 76 hectograms (15 Fr. pounds) of meat were reduced, by boiling, to ten pounds; but the bones loft none of their weight: whence it follows, that in order to extract as much nutriment as is poffible from thefe fubftances they muft be broken to pieces.

The frefh decoction of flefh is acid, and reddens the tincture of turnfole. The acid which it contains appears to be the phofphoric; for lime water and ammoniac form a rather abundant precipitate with it. Alcohol diffolves a portion of the extract of flefh, and this part being extremely falt, M. Prouft fufpected the prefence of muriate of ammoniac; but lime in powder not having feparated any ammoniac, he mixed a folution of platina with it, which immediately furnifhed him with muriate of platina and pot-afh. The decoction, therefore, contains muriate of pot-afh in abundance. M. Prouft had not time to examine the other falts contained in the decoction, nor the fubftance foluble in alcohol; and he, with reafon, complains that a fubftance fo neceffary for the nourifhment of mankind, has not been made the object of ferious chemical refearch.

He made fome trials upon the pyrites of the Incas, to difcover if it did not contain gold or fome other fubftance which might ferve to explain the caufe of the pale colour which diftinguifhes it. He only obtained, as the refidue of his folution, a black powder mixed with fand. This powder was coal, which caufed a ftrong detonation with nitre.

It has been thought, and it is ftill generally fuppofed, that the black duft which is depofited during the folution of zinc in acids is carburet of iron; but M. Prouft found that it was

a mixture

a mixture of arfenic, copper, and lead which the difoxidating action of the zinc precipitates in the metallic ftate. This alfo happens to thefe metals when a folution is made of tin with which they are alloyed. Arfenical lead, fuch as is made ufe of at the mines of Limares for the making fmall fhot, depofit on the immerfion of a fheet of lead, part of the metallic arfenic they contained. Thefe are not the only metals which adulterate the purity of the zinc. Iron and manganefe are fometimes found in great quantities: whence it is not furprizing that the art of watch‑making has fo much reafon to complain of the bad qualities of brafs made in the common way. The following facts will fhew the enormous difference between purified zinc, and that of commerce. To purify zinc, Mr. Prouft propofes diftilling it in a ftone retort, the neck of which muft be inclined at leaft 45 degrees, that the metal may flow with greater eafe as foon as it becomes volatalized. A mixture of fand, and of the oxides of iron, lead, copper, and zinc remain in the retort, the oxidation of which is attributed by M. Prouft to the porofity of the veffels.

The zinc thus purified does not differ either in colour or in weight from the zinc of commerce: 55 decigrams (100 Fr. grains) of the latter yield by folution in fulphuric acid, in lefs than hour, 3440 centimetres (from 172 to 174 Fr. inches) of hydrogen gas; whereas more than 8 days are requifite to obtain the fame refult from diftilled zinc. This is the difference which M. Prouft obferved between the two metals. The black matter which feparates from the zinc, during its folution in fulphuric acid, is foluble in nitric acid; and its folution on the addition of hydro‑fulphurated water affords arfenic; and if that arfenic had been mixed with copper or lead, the fame re‑agent would have equally difcovered it. For by adding it gradually to the folution of thofe three metals, the copper is feen to precipitate itfelf firft of a brown colour the lead, the next of a black colour, and the arfenic laft of a yellow colour; and by a careful management they may be feparated by filtration.

M. Prouft juftly confiders the purification of zinc by fulphur as an abfolute deception.

55 decigrams (100 Fr. grains) of zinc, diffolved in nitric acid, leave, after the decompofition of the nitre by fire, 69 decigrams (from 125 to 126 Fr. grains) of oxide of a light yellow colour. A folution of the fame quantity of zinc, decompofed by carbonate of potafh, furnifhed 99 decigrams (180 Fr. grains) of carbonate of zinc, which alfo left 125 grains of oxide after calcination.

The folution of this metal by fulphuric acid gave the fame refults; which proves that zinc, in all cafes, combines itfelf with a fpecific quantity of oxygen. M. Prouft obferves how fingular it is that copper and zinc, which attract the oxygen with fuch different degrees of ftrength, fhould, neverthelefs, in their combination with acids, abforb exactly the fame proportion of this principle.

M. Prouft, in the next place, propofes the following formula to feparate zinc from copper. Suppofe the fame folvent to contain lead, copper, and zinc. Sulphate of pot‑afh will precipitate the lead, if the (nitric) folution does not contain too much acid. Hepatic water feparates the copper a long time before the zinc. The filtered liquid may be examined by

<div align="right">fulphurated</div>

fulphurated hydrogen. When it no longer becomes coloured, a greater quantity of hydro-fulphurated water muft be added to it, when the zinc will in its turn fall down of a light yellow colour. If there had been iron, cobalt, nickel, or manganefe in the folution none of them would have been precipitated by this re-agent.

From the preceding experiments on zinc, M. Prouft draws the following conclufions. 1ft. In whatever acid this metal be diffolved, it conftantly abforbs the fame quantity of oxygen. 2d. If it contain metals capable of fuperoxydation, they pafs to this ftate when the folution has been effected either by nitric or oxygenated muriatic acid. 3. In a muriatic or fulphuric folution this metal is as much oxided as it can be, but iron on the contrary is only at its minimum; and this is the reafon why this folution does not become coloured with gallic acid, unlefs it has been expofed to the air. 4th. To demonftrate the prefence of iron fome drops of oxygenated muriatic acid muft be added, or it may be boiled with a fmall quantity of nitrous acid. 5. The carbonate, which is very white whilft under water, becomes yellow as foon as it is brought into the air, becaufe the iron paffes quickly to the maximum of oxidation. 6th. Sulphate of zinc, which has had time to abforb in the atmofphere as much oxygen as is neceffary to elevate the iron to its maximum, immediately affords yellow carbonate of zinc. 7. Zinc, which is purified by diftillation, or by the following humid procefs, is alone proper to afford the true zinc-white for painting.

Repeated criftallization of the fulphate of zinc, and immerfion of plates of this metal into its folution appeared to M. Prouft, to be very infufficient means to feparate the foreign metals. To accomplifh this object, 30.57 grams (one Fr. ounce) of nitric acid are put into about 2 pounds of the faturated folution of fulphate of zinc, and the mixture flightly boiled. Pot-afh is then mixed with it to faturate the excefs of acid, and to precipitate nearly 15.28 grams (3 or 4 gros) of the matter; this mixture is boiled again, and the precipitate is foon obferved to pafs from white to yellow. If after fome minutes of ebulition fome white particles are remarked amongft the yellow depofit, it is a certain fign that not an atom of iron remains in the folution of zinc. But if by this method the iron has been entirely feparated from the zinc, manganefe may yet be prefent, if any portion exifted in the ore; and M. Prouft has frequently met with it.

To clear it of this new oxide the fulphate of zinc is diffolved in boiling water, and precipitated by carbonate of pot-afh, fo as ftill to leave a fmall quantity of oxide of zinc in folution. The depofit is kept feveral days in the liquor, in order that the oxide of manganefe, which has been precipitated, being more ftrongly attracted by the acid than the oxide of zinc, may precipitate that portion of the latter which had been left in the fluid, and become diffolved in its place. Then the fulphate of zinc affords an oxide of the moft perfect whitenefs which may be employed, very advantageoufly, for painting.

Thefe are the facts contained in M. Prouft's Memoir. They are numerous and interefting. Some of them are new to us; the moft part, though already known, are prefented under new relations and rendered applicable to the arts. The experiments, by the help of which he has difcovered thefe facts, are ingenious. His explanations, though concife, appear

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appear to us to be clear, and deduced immediately from the results of the experiments. They may therefore tend to the advancement of philosophical chemistry, and to the perfection of the arts and manufactures. We, consequently, recommend to the class (of the national institute) to order them to be printed in the volumes which it proposes to publish. We, cannot, however, agree with M. Prouft in some points. 1st. That nickel, cobalt, and manganese are not precipitated by sulphurated hydrogen. 2. That the black dust, which separates during the solution of zinc, does not contain carburet of iron. 3d. That zinc is separated from arsenic by distillation. The contrary is so well known by chemists, that it requires no proof.

V.

Experiments on the solar and on the terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same or different. By WILLIAM HERSCHEL, *LL. D. F. R. S* *.

THE word heat is most commonly used to denote a certain well known sensation. It is also employed to signify the cause of that sensation, as well by the vulgar as by men of science: some of whom have added certain distinctive terms, such as latent, absolute, sensible, radiant, while others have treated of the matter of heat, and calorie. Dr. Herschel has not thought fit to adopt any of these terms in his Memoir, but has chosen to treat of *the rays that occasion heat*; not meaning either to state that these rays are heat, or the manner in which they produce it. In his prefatory observations he also in effect remarks that his present research is confined to the agency of heat in its state of radiance; without entering into any considerations respecting the general nature of heat itself, whether it be matter or modification, or of radiance, whether it be the projection of particles, the undulation of a fluid, or any other habitude or thing.

His subject is reduced to three general heads: the first relates to the heat of luminous bodies in general as it comes directly from the sun, or from the terrestrial flames of torches, candles, lamps, blue lights, &c.—the second includes the heat of coloured radiants as from the sun, when its rays are separated by the prism, or from culinary fires openly exposed;— and the third division relates to heat obtained from radiants, where neither light nor colour in the rays can be perceived. This is to be had as before shewn †, directly from the sun, by means of a prism applied to its rays, and also from fires inclosed in stoves, or from red-hot iron, cooled, till it can no longer be seen in the dark.

* Abridged from the Phil. Transf. for 1800, page 293.
† Philof. Journal IV. 320.

Besides

Befides the foregoing general arrangement, the author gives a previous comparative view of facts relating to light and heat as follows:

1. LIGHT, both folar and terreftrial, is a fenfation occafioned by rays emanating from luminous bodies which have a power of illuminating objects; and according to circumftances of making them appear of various colours. 2. Thefe rays are fubject to the laws of reflection. 3. And alfo thofe of refraction. 4. They are of different refrangibility. 5. Are liable to be ftopped in certain proportions when tranfmitted through diaphanous bodies. 6. And to be fcattered on rough furfaces. 7. And laftly, they have hitherto been fuppofed to have a power of heating bodies, but this remains to be examined.

1. HEAT, on the other hand as well terreftrial as folar, is a fenfation occafioned by rays emanating from candent fubftances which have a power of heating bodies. Thefe rays have the fame affections as have been enumerated in the 2, 3, 4, 5, and 6th divifions of the laft paragraph, and 7. They may be fuppofed, when in a certain ftate of energy, to have a power of illuminating objects; but this remains to be examined.

The paper before us contains the firft part of the experimental inveftigation of the fubject under the three firft heads. The three next, and a difcuffion which will be brought on by the feventh article, are referved for the fecond part.

Experiment 1. Reflection of the heat of the fun. The fun's rays were received in a ten foot Newtonian telefcope, with a camera eye piece, with no eye glafs. By proper adjuftment the focus was made to fall on a fmall thermometer, which was raifed 58 degrees. The rays, therefore, whether of light or not, did occafion heat after three regular reflections.

Experiment 2. Reflection of the heat of a Candle. At the diftance of 29 inches from a candle a fmall fteel mirror, 3, 4 inches diameter, and about 2, 75 inches, focal length, was placed. Two thermometers were difpofed, one in the fecondary focus, and the other very near it, but out of the courfe of the reflected light. In five minutes that in the focus rofe $3\frac{1}{4}$ degrees, which it loft again in fix minutes when the candle was covered, and again recovered it in five minutes when the rays were fuffered to fall on the mirror. The other thermometer remained ftationary throughout.

Experiment 3. Reflection of the heat that accompanies the folar prifmatic colours. The folar fpectrum given by a prifm was admitted through pafteboard (which limited the vifible colours) upon the fteel mirror. The thermometer in the focus was raifed 35° in two minutes.

Experiment 4. Reflection of the heat of a red hot poker. The mirror was placed at the diftance of 12 inches from a red hot poker, and the thermometer placed in its fecondary focus. A fmall pafteboard fcreen was placed to guard the thermometer from the direct heat. In one minute and a half it rofe $38\frac{1}{2}$°, and fell 28° in the next minute and a half, when the mirror was covered.

Experiment 5. Reflection of the heat of a coal fire by a plain mirror. A fmall fpeculum (Pl. XVI. fig. 1.), fuch as the Doctor ufes with his feven feet reflectors was placed upon a ftand, fo as to make an angle of 45° with the front of it, and confequently with the bars

of a grate oppofite which it was placed. At a diftance of $3\frac{1}{2}$ inches from the fpeculum, on the reflecting fide of it, was placed a thermometer, and another clofe by it, out of the courfe of the reflected rays. The whole was guarded in front againft the influence of the fire by an oaken board, $1\frac{1}{2}$ inch thick, in which was a circular opening of $1\frac{1}{4}$ inch diameter, oppofite the plain mirror, to permit the fire to fhine upon it. There was alfo a wooden partition between the mirror and the thermometers in which was a hole that allowed the rays to fall on the firft mentioned thermometer, but prevented their accefs to the latter. When this apparatus was expofed to the fire the firft thermometer rofe 7 degrees in five minutes, while the other indicated a change in the temperature of the place, amounting to no more than half a degree.

Experiment 6.   *Reflection of fire-heat by a prifm.* When the laft defcribed experiment was repeated by fubftituting inftead of the mirror a prifm whofe angles were 90°; 45°; and 45°, fo placed that the radiation from the fire fhould pafs through one fide, and make its incidence on the hypothenufe within the prifm, the rife was $4\frac{1}{2}$ degrees in 11 minutes; but the temperature of the place, as fhewn by the other thermometer, having been raifed $1\frac{3}{4}$ degrees in the fame time, the effect of the reflection was only $2\frac{3}{4}$ degrees.

Experiment 7.   *Reflection of invifible folar heat.* On a board of about 4 feet, 6 inches long, a fmall plain mirror was fixed at one end, and at the other, two thermometers; as fhewn in fig. 2. Pl. XVI. where for the convenience of infertion the board is reprefented as broken. The diftance of one thermometer from the face of the mirror was 3 feet $9\frac{3}{4}$ inches; and the other thermometer was put at the fide of it facing the fame way, but out of the courfe of the rays, that were to be reflected from the mirror. The colours of the prifm were thrown on a fheet of paper, having parallel lines drawn upon it at half an inch from each other. The mirror was ftationed upon the paper; and was adjufted in fuch a manner as to prefent its polifhed furface in angle of 45°, to the incident coloured rays, by which means they would be reflected towards the ball of the firft mentioned thermometer. In this arrangement the whole apparatus might be withdrawn from the colours to any required diftance, by attending to the laft vifible red colour, as it fhewed itfelf on the lines of the paper. When the thermometers were properly fettled to the temperature of their fituation, during which time the mirror had been covered, the apparatus was drawn gently away from the colours fo far as to caufe the mirror, which was now open, to receive only the invifible rays of heat which lie beyond the confines of the red. The refult was, that in ten minutes the firft thermometer received four degrees of heat reflected to it in the ftricteft optical manner, and the great regularity with which thefe invifible rays obeyed the law of reflection was fuch that the very fenfible thermometer, which was chofen as a ftandard, and was within an inch of the other, remained all the time without the leaft indication of any change. The mirror was then taken away, and in ten minutes the meafuring thermometer again loft the heat it had required. *This therefore,* as the Doctor obferves, *is a moft decifive experiment in proof of the exiftence of invifible rays, of their being fubject to the laws of reflection, and of their power of occafioning heat.*

<div align="right">

Experiment

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Experiment 8. Reflection and condensation of the invifible folar rays. The fmall fteel mirror before defcribed was by a fuitable apparatus placed to receive the rays of the prifmatic folar fpectrum perpendicularly to its diameter or fubtenfe. Half the mirror was covered by a femicircular pafteboard with lines drawn thereon parallel to the diameter, by means of which the laft vifible red might be made to fall at any required diftance from the uncovered portion of the fpeculum. The thermometer was placed in the focus, and the whole furface of the mirror kept covered till the inftrument became ftationary at the temperature of the place. At this period the apparatus was moved till the laft vifible red fell on the graduated pafteboard at the diftance of one-tenth of an inch from its diametrical edge, when the other part of the reflecting furface being uncovered for the admiffion of the invifible rays, the thermometer in one minute was raifed 19 degrees. The mirror was then again covered, and in 3 minutes the thermometer fell 16 degrees.—Again, the mirror was opened, and in 2 minutes the thermometer rofe 24 degrees. And on covering the mirror once more, in one minute the thermometer fell 19 degrees.

By this alternate rifing and falling our author obferves, that there are three points clearly afcertained. Firft, that there are invifible rays of the fun. Secondly, that thefe rays are not only reflexible in the manner proved in the foregoing experiment, but that by the ftrict laws of reflection they are capable of being condenfed. And thirdly, that by this condenfation their heat is proportionally increafed.

Experiment 9. Reflection of invifible culinary heat. The fteel mirror was fixed upon a fmall board, and before it was a wooden fcreen, half an inch thick, which was of an height to intercept the rays which might elfe have fallen on the lower half of the mirror. Behind this fcreen were placed two thermometers; one of which was in the axis of the mirror, and the other, which was near it, was fcreened on the fide towards the mirror by a fmall flip of pafteboard tied to the fcale. This apparatus being duly placed oppofite a clofe ftove, well heated, the invifible rays reflected from the mirror raifed the thermometer in its axis 39 degrees in one minute, while the other, which had exactly the fame expofure to the ftove, but was defended from the reflected rays of the mirror, rofe only one degree.

Experiment 10. Reflection of the invifible rays of heat of a poker cooled from being red-hot till it could no longer be feen in a dark place. A poker of a proper black heat was placed at the diftance of 12 inches from the fteel mirror, and the effect of its condenfed rays was received upon the thermometer placed in the focus. The mirror was uncovered and covered alternately, one minute at a time, for fix minutes, and the alternations were, rife 7°; fall 7°; rife 3°; fall 5°; rife $2\frac{1}{2}$°; fall $6\frac{1}{2}$°.

Dr. Herfchell concludes his narration of facts concerning reflected heat with the following remarks:

" From thefe experiments it is now fufficiently evident, that in every fuppofed cafe of " folar and terreftrial heat, we have traced out rays that are fubject to the regular laws " of reflection, and are invefted with a power of heating bodies; and this independent of " light. For though, in four cafes out of fix, we had illuminating as well as heating rays,

" it

" it is be noticed that our proof goes only to the power of occafioning heat, which has been
" ftrictly afcertained by the thermometer. If it fhould be faid that the power of illuminating
" objects, of thefe fame rays, is as ftrictly proved by the fame experiments, I muft remark
" that from the cafes of invifible rays brought forward in the four laft experiments, it is
" evident that the conclufion that rays muft have illuminating power, becaufe they have a
" power of occafioning heat, is erroneous; and, as this muft be admitted, we have a right
" to afk for fome proof of the affertion, that rays which occafion heat can ever become vi-
" fible.　But, as we fhall have an opportunity to fay more of this hereafter, I proceed now
" to inveftigate the refraction of heat-making rays."

Experiment 11.　*Refraction of folar heat.*　The fun's light was received in a new ten feet
Newtonian telefcope; the mirror of which has 24 inches in diameter of polifhed furface.
The rays were made to pafs through a day-piece with four lenfes, and thence to the ball of the
thermometer placed in their focus.　As foon as the rays were brought to the thermometer
it rofe almoft inftantly through 70°, when the telefcope was turned away to avoid the dan-
ger of cracking the glaffes.　Here the rays which occafioned this fudden augmentation
of heat had undergone eight fucceffive refractions, fo that their being fubject to its laws
cannot be doubted.

Experiment 12.　*Refraction of the heat of a candle.*　A lens of about 1.4 inch focus, and
1.1 diameter, mounted on a fmall fupport was placed at the diftance of 2.8 inches from a
candle, and a thermometer was very carefully placed in the fecondary focus of the candle
behind the lens.　Not far from the lens towards the candle was a pafteboard fcreen with an
aperture of nearly the fame fize as the lens; and the lens itfelf was fupported on an ec-
centric pivot, fo that it could be turned away from its place and reftored at pleafure.　This
arrangement being made, the thermometer was for a few moments expofed to the rays of
the candle till it had affumed the temperature of its fituation.　Then the lens was turned
on its pivot, fo as to intercept the direct rays which paffed through the opening in the pafte-
board fcreen, and to refract them to the focus in which the thermometer was placed.　In
three minutes the thermometer received $2\frac{1}{8}$ degrees of heat by the refraction of the lens,
which it again loft in the fame time when the lens was turned away, and in three minutes
more the fame increafe of heat was produced by replacing the lens in its fituation. A greater
effect could be produced by a different arragement of the diftances.　Thus when the lens
was placed at $3\frac{1}{2}$ inches diftance from a wax candle, the thermometer in the fecondary
focus was raifed from 5 to 8 degrees, according to the burning of the candle and the accu-
racy of the adjuftment.

Experiment 13.　*Refraction of the heat that accompanies the coloured part of the prifmatic fpec-
trum.*　The coloured part of the prifmatic folar fpectrum being admitted through a perfo-
rated fcreen of pafteboard, upon a burning glafs, raifed the temperature of a thermometer
112 degrees in one minute.

<div align="right">*Experiment*</div>

Experiment 14. *Refraction of the heat of a chimney fire.* The fame burning glafs which is nearly 9 inches in diameter was placed before the clear fire of a large grate, at the diftance of 3 feet from the bars, and a thermometer was placed in the fecondary focus. Another thermometer was alfo placed behind a wooden fcreen at the fame diftance from the fire. The fituation of the feveral parts of the apparatus being thus adjufted the thermometers were taken away to be cooled, and then reftored to their places. The ftandard thermometer in the courfe of the firft feven minutes acquired a heat of $3\frac{3}{4}$ degrees, after which it remained fteady for the fubfequent 32 minutes duration of the experiment. The other thermometer was treated in the fame manner as that which received the refracted heat from a candle in experiment 12; that is to fay, the lens was alternately placed and removed, and the variations were—rife in the firft 9 minutes, $9\frac{3}{4}$ degrees; in the next 5 minutes, before the uncondenfed rays of the fire, fall $2\frac{1}{4}$ degrees;—in the next 10 minutes rife $1\frac{3}{4}$ degrees; in the next $5\frac{1}{4}$ minutes, fall 3 degrees; and in the next $4\frac{1}{2}$ minutes, rife $1\frac{1}{2}$ degrees.—So that in the courfe of 35 minutes, the thermometer was raifed and depreffed 5 times by rays which came from the culinary fire, and were fubject to laws of refraction, not fenfibly different from thofe which affect light.

Experiment 15. *Refraction of the heat of red hot iron.* A lump of iron was forged into a cylinder $2\frac{1}{2}$ inches in diameter, and $2\frac{1}{2}$ inches long. This being made red hot, was ftuck upon an iron handle fixed on a ftand, fo as to prefent one of its circular faces to a lens of the dimenfions ufed iu the 12th experiment, at the diftance of 2,8 inches. Before the lens, at fome diftance, was placed a fcreen of wood, with a hole of an inch diameter in it, to limit the object, and to keep the heat from the thermometers. One of thefe was placed in the fecondary focus of the lens, and the other, within $\frac{3}{10}$ of an inch of that focus, and at the fame diftance from the lens, fo that if there was any difference in the expofure, it was in favour of the laft thermometer, which ftood oppofite a thinner part of the lens. During the experiment, the thermometers were alternately fcreened from the effect of the lens, and expofed to it for the fame length of time, and the effects were, that both underwent variations of increafed and diminifhed temperature; but that which was in the focus gained 6 degrees in the firft two minutes, whilft that near the focus gained only 4; and the firft loft 3 degrees in the fecond two minutes during the time the lens was fcreened, whilft the fecond loft only 2; and fo in three other alternations the alternations of temperature in the focus were $2°$, $2\frac{3}{4}°$, and $1\frac{1}{4}°$, whilft the correfponding changes out of the focus were $1°$, $1\frac{1}{4}°$, and $0\frac{1}{2}$. To remove all doubt on the fubject, the thermometers were left in their refpective fituations, and a plain glafs fubftituted inftead of the lens. In thefe circumftances the laft related procefs of alternate covering and uncovering the glafs was repeated, and both thermometers were found to be affected alike.

Experiment 16. *Refraction of fire heat by an inftrument refembling a telefcope.* This inftrument confifted of a concave mirror, which received the heat from the fire, a plain mirror which reflected it to one fide, where there was a lens at the ufual eye aperture. A thermometer was placed in the focus of this lens, and another near the focus, and the fame

method.

method of comparative examination was adopted as in the last experiment. The result also was similar, for which reason I shall confult brevity so far as to avoid repeating the particulars in detail.

Experiment 17. *Refraction of the invisible rays of solar heat.* One half of the before mentioned burning lens was covered, and the prismatic spectrum thrown upon the cover, so that the last visible red colour falling one-tenth of an inch from the margin of the pasteboard, the invisible rays beyond the spectrum were suffered to fall upon the lens. In the focus of the red rays, or a very little beyond it, was placed the ball of one of the thermometers, and the other thermometer as near to it as convenient. The thermometer in the focus was raised 45° in one minute, whilst the other thermometer underwent no change.

As a little of the red colour was difcernible on the ball of the thermometer, the Doctor was induced to try if the invisible rays could not be rendered perceptible to the fight by condensation; and with this intention the next experiment was made.

Experiment 18. *Trial to render the invisible rays of the sun visible by condenfation.* The last defcribed experiment was repeated by withdrawing the lens till the extreme visible colour was $\frac{2}{10}$ from the margin of the femicircular pasteboard cover. In these circumstances there was no longer any tinge of colour or vestige of light to be seen on the ball of the thermometer; but, nevertheless, the effect of the invisible rays was fuch, that the thermometer in the focus received 21° of heat, whilst the other near the focus remained ftationary. The colour feen in the 17th experiment must therefore, as the Doctor obferves, have arifen from the imperfect refraction of a lens of so great a diameter; the difficulty of afcertaining the termination of a prifmatic spectrum in a room not perfectly dark, and the gradual diffufion of rays of the fame kind over a confiderable space in confequence of the breadth of the prifm.

Experiment 19. *Refraction of invisible culinary heat.* This experiment is attended with fome difficulties, on account of the feeble ftock of heat in a red hot lump of iron, or other fimilar thing, and the fpeed with which it is carried off. The Doctor was prevented from adopting fome contrivance to keep up this heat from the confideration that the alternate rifing and falling of a thermometer in the focus of a lens, fucceffively covered and uncovered, must be afcribed to the refraction of heat. The lens of 1,4 focus was placed before the cylinder of iron of experiment 15, made very red hot, and the thermometer placed in its focus was for each fucceffive two minutes first expofed to the heat, and then fcreened from it by a fmall piece of pasteboard. The iron was at the beginning very red hot. The changes, after the iron had ceafed to emit any light perceptible in a darkened room, were eight in number, and regular, amounting to one degree at the beginning, and at the end the effect of the condenfed rays exceeded but half a degree the lofs of thofe which were ftopped by the lens.

Experiment 20. *Confirmation of the laft experiment.* The 19th experiment was repeated with an affiftant thermometer. The principal thermometer being placed in the focus, as

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before the affiftant thermometer was placed $\frac{3}{4}$ of an inch diftant on one fide, and, confequently, was expofed to the direct action of the heated cylinder. The fcreen was never advanced before this laft. During eight minutes, the thermometer in the focus being uncovered for the firft minute, covered for the fecond, uncovered for the third, and fo forth, came to its maximum by alternate greater elevations and fmaller depreffions, and afterwards loft its heat by greater depreffions and fmaller elevations. But the other thermometer being out of the reach of refraction, acquired its maximum of heat gradually, in confequence of an uniform expofure to the heated cylinder, and afterwards began to decline.

After the firft eight minutes, the affiftant thermometer was removed to one fide of the focus, fo as to participate of the alternate fcreening, and alfo to receive a fmall fhare of one fide of the invifible heat image, which muft be formed in the focus of the lens. Here, in confirmation of the reafoning flowing from the general facts and doctrines, the affiftant thermometer fhould be affected by alternate rifings and fallings, but lefs confiderable than thofe in the focus. The changes of the focal thermometer were for fix alternations in ten minutes $- \frac{3}{4} + \frac{1}{2} - 1\frac{1}{4} + \frac{3}{4} - 1 + 1$, and thofe of the affiftant thermometer were $- \frac{1}{4} + \frac{1}{4} - \frac{3}{4} + \frac{1}{4} - \frac{1}{2} + \frac{3}{4}$, all which, as our author obferves, fo clearly confirm the effect of the refraction of the lens, that it muft be evident that there are rays iffuing from hot iron, which, though in a ftate of total invifibility, have a power of occafioning heat, and obey certain laws of refraction, very nearly the fame with thofe that affect light. From the whole retrofpect of his paper, he adds, that it will be eafy to perceive that he has made a good proof, in this divifion of his memoir, of the three firft of his propofitions.

VI.

On the Arragonite of Werner. By Cit. Hauy[*].

THE mineral which is the object of this Memoir prefents itfelf in the form of hexahedral cryftals of a dirty, uneven, violet colour; fome of them have their bafes fmooth, but commonly tarnifhed, whilft others inftead of bafes have a multitude of angular projections. Their fpecific gravity is 2,9455; their refraction double in a very ftriking degree. They eafily fcratch carbonated lime, and appear to poffefs the fame hardnefs as fluated lime; their powder thrown upon burning coals gives a violet coloured phofphorefcence. They are entirely foluble with a lively effervefcence in nitric acid. They are found in Spain between the kingdoms of Arragon and Valencia, which is the reafon of their being called Arragonites, by Werner. There are fome alfo to be met with near the Pyrenean Mountains.

Their phofphorefcence, together with their colour, induced Born to fufpect the prefence of phofphate of lime; but Klaproth found only carbonate of lime.

* Bulletin de la Soc. Philomath. No. 33. tome 2.

Hauy

Hauy having endeavoured to divide fome of thefe cryftals mechanically, obferved that they had natural planes of junction fituated parallel to their prifms which indicated a ftriking difference between their ftructure and that of carbonated lime. He has alfo remarked, that there is a difference of feveral degrees in the mutual incidences of the faces of the prifm, and that this inequality varies in different cryftals. This variation of the angle would be inexplicable on the hypothefis of a fimple cryftal where there fhould be unity of ftructure; but the afperities diverging from the center towards the faces, and the ftriæ which render the furface unequal, prove that they are formed of feveral cryftals grouped together in the following manner.

The cryftal which may be confidered as the element of the group is a cuneiform octahedron. The four trapeziums, which form the four large faces, incline 116° on one fide, and 64° on the other. The incidence of the triangular faces upon each other, at the place where they re-unite in a common edge perpendicular to the axis, is about 70°. Thefe octaedrons are divided by fections, parallel to their larger faces. Confidering thefe octaedrons as quadrangular prifms with dihedral fummits; and fuppofing that their planes incline to each other in an angle of 120°, it is evident that three of thefe prifms applied together will form a regular hexaedral prifm whofe bafe, inftead of being plane, will prefent three faliant edges which will unite at the axis of the prifm.

But fuppofing the faces of the prifm to incline in angles of 116° and 64°, as is the cafe in nature, then three of the prifms cannot exactly fill a fpace; but two of them, for example, A and C, fig. 3. Pl. XV, will leave a concave fpace between them of 12 degrees.

To fill this fpace the cryftallizing procefs employs a fourth prifm D, which appears to penetrate in part the prifm C, fo that in the folid which refults from this re-union the two faces which cut each other in the centre make an angle of 128°; but as real penetration is impoffible the fame thing probably happens here as in thofe groups of cryftals which croffed each other, and which at the place where they feem to enter each other, have a plane of junction fituated parallel to one of the faces which would be produced by virtue of a law of diminution.

The cryftals whofe bafe is covered with cuneiform projections are nothing elfe but much more numerous groups of quadrangular prifms, whofe refpective arrangement is fubjected to the fame conditions.

With refpect to thofe cryftals which have four angles of 116°, and two of 128°, the order of their ftructure will be underftood by the bare infpection of fig. 4. In this cafe the affemblage is compofed of four prifms, A, B, C, D, which fuppofing no modification of form, would leave in the interior of the prifm an empty fpace, indicated by the rhomb, *h, z, u, r,* but this is filled by an extenfion which each prifm receives always in confequence of a law of diminution; fo that the prifm A increafes in a quantity reprefented by the rectangular triangle, *h, o, z,* &c.

In the cryftals whofe bafes are fmooth a diminution takes place by a fingle row on the terminating edge of the component prifms.

The

The vertical section of the prisms of the arragonite present another singularity noticed by Born; it is a kind of cross, composed of four triangles, amongst which those whose bases are horizontal, are of a pale colour; and the other two, whose bases are vertical, are of a more intense violet colour.

Fig. 2. represents a prism of arragonite, where the four cuneiform octahedrons which compose it, are very distinct.

VII.

On the true Origin of Resin, known by the Name of Sandarac, *and on that of Gum Arabic, by M.* SCHOUSBOE *.

SANDARAC is an article of commerce, which is brought from the western provinces of the kingdom of Morocco. From six to seven hundred quintals are annually shipped in the ports of Santa Cruz, Mogador, and Saffy. This resin is called, in the language of the country *el graffa*. The tree which produces it is a thuia, which was also found in the kingdom of Tunis by M. Vahl, who has given a complete description, and a good figure of it in his work, entitled, Symbol. Botan. part the 2nd, page 96. Pl. XLVIII. by the name of Thuia Articulata. Shaw had formerly made it known, and called it *Cypreffus, fructu quadrivalvi, foliis equifeti inftar articulatis;* but neither of these learned men were acquainted with the œconomical use of this tree, probably because as it is seldom met with in the northern parts of Barbary, too little advantage can be derived from the resin which flows from it.

Till the present time this resin was attributed either to the Juniperus Communis, or to the Juniperus Lycia, or lastly to the cedar of Libanus, without confidering that the Juniperus Communis is not found in Africa; and that the Sandarac appears to come exclusively from that part of the world. M. Schoufboe, who has seen the species of thuia here alluded to, says, that its height never exceeds 8 metres (26¼ feet), and that the diameter of its trunk is not more than 20 or 22 (7 inches) centimetres. It is distinguished at first sight, from the two other species of the same kind of tree, which are cultivated in our gardens, by having a distinct trunk, and the appearance of a real tree, whilst in the others the branches issue out from the root, which gives them rather the resemblance of bushes. Its branches also are more articulate and more brittle; its bloffoms, which are not very apparent, display themselves in germinal, (April), and its fruits, the form of which is nearly spherical, ripen in Fructidor, (September).

By prefenting a branch of this vegetable to the light, it is observed to be covered with a multitude of transparent vesicles, which contain the resin. After these vesicles have

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* Extracted by Ch. Coquebert, from the Danish Journal, entitled, Bibliot. de Physique, medicine et economie, 3d cahier, 1799, and inferted in the Bulletin de la Soc. Philom. No. 31.

burſt in the ſummer months, a reſinous juice iſſues from the trunk and branches by exuda-
tion, as in other coniferous trees. This is ſandarac. The inhabitants of the country
collect it and carry it to the ports, whence it is conveyed to Europe. We make uſe of it
in the compoſition of ſealing wax, and for different kinds of varniſh. In 1793 an hundred
pounds weight of it coſt in the ports of Morocco from 13 to 13 $\frac{1}{2}$ piaſtres, which is equal
to about 75 (7 $\frac{1}{2}$ d.) centimes of our money per pound. The duty upon exportation was
about 9 francs per quintal.

Sardarac, if good, is of a clear yellow, pure and limpid. It is a commodity rather
difficult to adulterate. Care however muſt be taken that the Moors do not mix too much
duſt with it.

It is probable the ſame tree which produces ſandarac at Senegal, whence it is exported
in rather large quantities.

Another article of commerce, of which the kingdom of Morocco alſo partakes with
Senegal, is the gum called Arabic, which bears the name of al leilk. The tree which affords
it grows only in the weſtern provinces of that ſtate. The exportation of this ſubſtance to
the different ſtates of Europe, amounts, throughout the ports of Morocco, to 8 or 9 thou-
ſand quintals. M. Schouſboe ſays, that this tree is the *mimoſa nilotica*, called by the
natives *al thlah*, which is no reaſon why in the ſouthern parts of Africa, it may not be
gathered, as authors aſſert that it is, from the *mimoſa ſenegal*, and even from other ſpecies
of that genus.

In Barbary, a diſtinction is made between the gum of Senegal and that of the country.
The firſt has the preference, on account of its purity, its clearneſs, and its whiteneſs, which
are the qualities in general ſought for in this commodity.

The gum which I myſelf collected in the province of Mogador, obſerves M. Shouſboe,
exudes from the trunk and the branches of the tree, like that of our fruit trees. It is in
round pieces, of the bigneſs of a hazle nut, or at moſt of a walnut. Theſe pieces, indeed,
by adhering to each other, ſometimes form maſſes of the bigneſs of the fiſt, or even
of the head ; but that is only owing to the adheſion which takes place between the gum,
ſtill freſh, after it had been ſeparated, and principally from the part which adhered to
the bark, where the gummy juice has not had time to harden. When earth, ſmall ſtones,
or other foreign bodies, are ſometimes found in theſe maſſes, it is the effects of fraud.
M. Shouſboe ſuſpects that it is this circumſtance which has given riſe to the opinion
that gum was found at the foot of trees, and that it exuded from their roots ; (ſee
Bulletin des Sciences, No. 8.) for which he thinks there is no foundation. If that was
the caſe, it appears to him that beſides the ſand and the earth, with which the maſſes
of gum are accidentally contaminated, ſome of it would be found in the interior part
of the globules, and would be even ſo much blended with the mucilaginous ſubſtance,
that it would not be poſſible to purify it completely ; whereas, on the contrary, the gum
which comes from Senegal is ſtill purer than that from Barbary.

M. Schouſboe, nevertheleſs obſerves, that ſandarac, and the gums which are exported
                                                                                through

through the port of Saffi are of a brown or red colour; but he attributes that colour to the quantity of red oxide which is mixed with the foil of the province of Abda, in which this part is fituated. This oxide communicates its colour even to the whiteft wool, and the inhabitants of this province are known by the red tinge of their cloaths, which no procefs can totally deftroy.

In the months of Meffidor and Thermidor, when heavy dews fall, the gum lofes much of its clearnefs and other qualities for which it is defirable.

An hundred pounds weight of this fubftance coft, in the year 1793, at Mogador, about 48 francs of our money, exclufive of 5 francs 70 cents for cuftom-houfe duties.

The inhabitants of Morocco do not appear to apply this gum to any purpofe whatever; they fell all that they can gather to the commercial nations of Europe.

VIII.

Remarks on the Memoir in which M. Girtanner examines whether Azote be a fimple or a compound Body. By Cit. BERTHOLLET.*

SOME experiments were long fince made by Dr. Prieftley, which, at firft, induced him to believe, that water diftilled with lime, and particularly with clay, or indeed without any addition, in an earthen retort, or a retort of glafs, deprived of its polifh, became changed into air. But he found that this deduction was erroneous, and that it was merely a tranfmiffion of the furrounding air which gave rife to the circumftance. Guyton added fome obfervations to thofe made by Prieftley (Encyclop. Vol. I. p. 674), and the fact appeared to be clearly accounted for.

Neverthelefs, Wiegleb publifhed a memoir, in which he pretended to prove, by his own, and by fome other experiments, that the gas which is obtained by paffing water through red hot earthen tubes might be attributed folely to the combination of the aqueous vapour with the matter of heat, and that this combination formed azote gas.

This memoir occafioned a reply which was publifhed by Dieman, Van Tfootwyk, and Lauwerenburg. Thefe learned chemifts carefully examined every circumftance which had apparently eftablifhed the pretended production of azote gas from water; and I fhould have thought that there could exift no doubt refpecting the confequence which they drew from their experiments, which was, " that the azote gas which, in fome cafes, is ob-
" tained by paffing water in vapour through ignited tubes, proceeds merely from the
" external air, deprived of its oxygen gas by the fire in which the tubes are placed; and
" that, thus, the pretended converfion of water into azote gas, by its combination with
" the matter of heat, is overthrown."

* Annales de Chimie, XXXV. 23. The memoir referred to is inferted in our Journal, and is concluded at p. 275 of the prefent volume.

After

After all thefe difcuffions, Girtanner fupports the new ideas which he prefents in his memoir, upon feveral facts, for the moft part borrowed, and already accurately difcuffed, amongft which, the following muft be confidered as fundamental. " When water is boiled " in a retort of glafs, or other material, azote gas is obtained." The author prefcribes thefe precautions: " In order to obtain azote gas in the greateft quantity, the water muft " evaporate flowly, and over a very gentle fire, which muft not be raifed."

" It is invariably obferved in all thefe experiments, that when the laft drop of water " evaporates, the azote gas is no longer produced, though the heat be kept up."

An affertion fo pofitive engaged me to repeat the experiment, with the precautions pre-fcribed by Girtanner. It was made with water newly diftilled, and with the well wafhed precipitate of alum by pot-afh. It was likewife made with a very white clay, with which I was fupplied by Cit. Guyton; but though the quantity of water was confiderable, and, con-fequently, thefe two experiments were of long duration, no gas became difengaged, and the refult was the fame as that of the Dutch chemifts.

It is, neverthelefs, upon this fact, which is fo eafy to be repeated, that Girtanner prin-cipally eftablifhes the compofition of azote, and thence that of the atmofphere, " which " is not, as till now has been imagined, a mixture of oxygen gas and azote gas, but rather " a mixture of oxygen gas and hydrogen—water in the form of gas, if I may be allowed " to make ufe of fuch an expreffion. When, by thofe chemical experiments which very " improperly have been called eudiometrical, the oxygen is feparated from the hydrogen, " this feparation can never be entirely or completely made; a part of the oxygen remains " united to the hydrogen, and forms the chemical combination which we call azote, and " which we obtain in thefe experiments."

So that when we make a mixture of oxygen and hydrogen gas, we form atmofpheric air, and the differences of fpecific gravity and other properties in all our experiments, as well as the products of combuftion, are only flight circumftances, of which Girtanner takes no account.

The memoir of Girtanner induced Citizen Bouillon la Grange to make experiments more numerous than mine, but which lead to the fame conclufions. I fhall here prefent a fhort account of them, which he has kindly communicated to me.

He did not obtain any azote gas by boiling diftilled water either alone, or with clay or alumine in a glafs retort, to which he applied a tube of glafs or of porcelain.

In like manner no difengagement of azote gas took place when the fame experiment was made with filiceous earth obtained by fluoric acid.

Having placed lime, obtained from white marble, in a tube of the fame kind, he paffed the vapour of water through this tube, which had its extremity immerfed in lime water. A fmall quantity of carbonic acid was difengaged, which formed carbonate of lime, but no azote gas was produced.

If, inftead of lime, clay is made ufe of with the fame apparatus, a little carbonate of lime becomes formed, but there is no difengagement of azote gas.

3

If

If water be passed through a tube of porcelain containing lead, this metal is converted partly to the state of yellow oxide, and becomes vitrified ; but there is no disengagement of azote gas.

If, instead of lead, tin is put into the tube, that metal becomes oxidated, and hydrogen gas is obtained. Zinc affords the same result.

To ascertain whether this hydrogen gas did not contain azote gas, Citizen Bouillon la Grange caused it to pass over sulphur in fusion, and he obtained sulphurated hydrogen gas, but no azote. He afterwards subjected it to another trial.

He mixed the hydrogen gas which he had obtained by means of zinc, with oxygen gas ; he set the mixture on fire by an electric spark ; water was formed, but there was no azote gas.

The opinion of Girtanner agrees with that of Humboldt, respecting the absorption of oxigen by simple earths, and particularly by alum ; there is, nevertheless, this difference, that Humboldt considers the supposed phenomenon as a simple separation of the oxygen which becomes fixed from the azote which remains in the gaseous state ; but according to Girtanner, " the azote which is obtained in our experiments being always produced by " operation itself, and having no existence in the form of azote before the experiment in " the air which is examined, Von Humboldt, who is fond of drawing general conclusions " from solitary facts appears to be deceived, when he asserts that earths may be used for " the purpose of determining the quantity of azote contained in the atmospheric air : for the " earths do not indicate that azote is contained in atmosperic air ; they change this air into " azote."

The younger Sauffure has formerly contradicted the result announced by Humboldt, (Journ. de Phys. frim. an. 7.) He allows that the humus, which is the result of the mixture of decomposed vegetables with other vegetables which are not yet decomposed, absorbs the oxigen gas, and that this is a known fact ; but he attests, " that this effect never takes " place when the earths are pure and deprived of every vegetable substance." He describes several experiments which he made with alumine, lime, and silex.

In the journal of Pluviose, of the same year, there is a reply from Humboldt, written in a magisterial tone. It contains nearly the same assertions as are contained in the first Memoir with the addition, however, (as a kind of sanction), that his observations were made in the laboratories of Vauquelin and of Fourcroy. This authority would undoubtedly be of great weight, if these two learned chemists had co-operated in the observations ; but very far from that being the case it is visible from the account that the only experiments, which were attempted in their presence, did not succeed.

I have been assured that the celebrated Fabroni, about the same period, repeated the experiments of Humboldt, at Florence, without any success.

Champy, the son, a very accurate observer, repeated them at Cairo upon alumine, upon lime, and upon the mud of the Nile, with a temperature which varied from 30 to 36° degrees of the centigrade thermometer, but he obtained no absorption. It must be remarked, that

that the mud of the Nile contains fome remains of vegetable fubftances; fo that, when it is diftilled a certain quantity of carbonic acid and of carbonated hydrogen gas is obtained.

Chaptal repeated them at Montpelier, without obtaining abforption.

I have kept moift alumine for a confiderable time in contact with atmofperic air and oxigen gas, without obferving the leaft abforption. It having been mentioned by a chemift that abforption was caufed by agitation; the effect of agitation was patiently tried, but the refult was always the fame. The operation has been renewed with the white clay which had been fent to me by Guyton, but it alfo produced no abforption. This clay, neverthelefs, when urged by the fire, yielded a little carbonic acid and a fmall quantity of carbonated hydrogen gas.

IX.

On the Decompofition of the Muriatic Acid.   By Mr. John Pitchford, Junior.

To Mr. NICHOLSON.

S I R,

IF you think the following obfervations of fufficient importance, you will oblige me by inferting them in your ufeful mifcellany.

I am, Sir,

Your's, &c.

Norwich, Aug. 16, 1800.   JOHN PITCHFORD, Jun.

I have long confidered the analyfis of the muriatic acid to be a point of the firft importance in the prefent ftate of chemical knowledge. Among many experiments to afcertain its compofition, which have occurred to me, one was founded on Dr. Fordyce's experiments, recorded in the Philofophical Tranfactions for 1792, the object of which was to difcover whether, in the folution of zinc in dilute fulphuric acid, the metal derives the oxygen neceffary to render it foluble from the acid, or from the water. The refult of a feries of very accurate experiments is well known to have been, that the water furnifhes the whole of the oxygen, and that no portion of the acid is decompofed. The proof of this is that it requires as much pot-afh to precipitate the zinc from a certain quantity of acid as to faturate an equal quantity of acid in which no zinc has been diffolved.

Wifhing to afcertain whether this would alfo be the cafe with muriatic acid, I diffolved one ounce of tin in four ounces of this acid of fpecific gravity, 1178. During the folution a confiderable quantity of hydrogen gas was evolved. When the folution was completed I added pot-afh till the whole of the tin was precipitated, and the acid faturated, which took place when 1412 grains of pot-afh had been added. I next tried how much pot-afh four ounces of the fame muriatic acid would require for faturation, and found that 1717 grains were required. Now allowing that fome fmall portion of the muriatic acid, may have been

been volatilized during its folution of the tin, and carried off by the hydrogen gas during its efcape, this cannot be fuppofed to account for more than a very fmall proportion of the acid which difappeared. I therefore infer that when tin is diffolved in muriatic acid, a part of the acid is decompofed, and I think it not improbable that, it fupplies the tin with oxygen, and that its other component part hydrogen flies off in the form of hydrogen gas. I am well aware that many more experiments ought to be tried before it can be eftablifhed that oxygen and hydrogen are the conftituent principles of muriatic acid; but one thing is I think fully eftablifhed by this experiment, namely, that fome portion of this acid is decompofed during the folution of tin: For if in Dr. Fordyce's experiment, in which two equal portions of fulphuric acid, in one of which zinc had been diffolved, and in the other not, required equal quantities of pot-afh to faturate them, it was a fair inference, that therefore fulphuric acid is not decompofed in diffolving zinc; it is equally fair to infer from my experiment in which the muriatic acid, in which tin had been diffolved, required fo much lefs pot-afh to faturate it than an equal quantity of the fame acid in which no tin had been diffolved, that part of the muriatic acid is decompofed in diffolving tin.

I am alfo fenfible that the above experiment might be tried in a much better way : the quantity of water contained in the muriatic acid ought to be afcertained previous to the folution of the tin, and after its precipitation by the alkali, and a calculation then made, whether the portion of water miffing would account for all the hydrogen and oxygen produced : if it appeared that the quantities of thefe were greater than the water miffing would furnifh, then I think it would be eftablifhed that they entered into the compofition of muriatic acid. Another important experiment would be to expofe tin to muriatic acid gas, perfectly free from water (a method of obtaining, which appears in Mr. Henry's experiments, and will be hereafter mentioned), and examine whether it becomes oxidated, and to expofe not only tin but every other metal, and every other fubftance, likely to feparate the oxygen of this acid from the unknown radical. I am neither in poffeffion of an apparatus fufficiently delicate, nor have I opportunity at prefent to try thefe and fimilar experiments, but having feen in the Philofophical Tranfactions for 1800, page 188, that Mr. William Henry has been engaged in experiments on the decompofition of this acid, I have thought it right to fuggeft the above hints.

The chemical world has great obligations to Mr. Henry for the labour and patience he muft have expended on his very interefting experiments. I fhould wifh to be permitted to make a few obfervations which refulted from an attentive perufal of his paper.

The firft feries of his experiments confifted in paffing the electric fhock repeatedly through muriatic acid gas confined over mercury. The effect was the diminution of this gas, the production of hydrogen gas, and the oxidation of the furface of the mercury : all this he attributes not to any decompofition of the acid, but to that of the water, which in the dryeft form, in which the gas is ufually produced, ftill adheres to it. As thefe effects took place only for a limited time, after which continued electrifation produced them no longer, and as a portion of muriatic acid gas always remained entire, this inference

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is probably true, and the experiments prove that the electric matter at leaft is incapable of decompofing muriatic acid. But though no progrefs may have been made towards the decompofition of the acid by thefe firft experiments, they are yet far from being ufelefs, fince we are now fupplied with the means of obtaining the muriatic acid perfectly free from water, namely, that portion which, after repeated electrifation, remains unaltered. I have no further remark to make on the firft feries of experiments, except that there appears to be fome obfcurity in the 5th, in which 143 meafures of common air, and 116 of muriatic acid gas, were electrifed, and by 30 fhocks reduced to 111. " The remainder " confifted of muriatic acid and azote gafes, with a fmall proportion of oxygenous gas." What, then, became of the hydrogen gas?

The fecond feries of experiments was to afcertain the effect produced by electrifying the muriatic acid gas with inflammable fubftances. Mr. Henry previoufly obferves, that he has attempted the decompofition of this acid by paffing it over red hot charcoal. " An " immenfe production of hydrogenous gas took place, but it was not eafy to determine " whether it had its origin from real acid or from water." I cannot help being furprifed that Mr. Henry fhould not have decided this point by paffing muriatic acid gas entirely freed from water by electrifation, over red hot charcoal, as this was fo obvious a method of afcertaining whether charcoal be capable of decompofing muriatic acid.

In the 12th experiment, Mr. Henry paffed 200 fhocks through a mixture of 83 meafures of carbonated hydrogen gas, and 89 of muriatic acid gas. The permanent refidue, after the admiffion of water, was 101 meafures; the addition, therefore, amounted to 18; of thefe 6 may be accounted for by the decompofition of the water of the muriatic acid gas, and 10 by that of the carbonated hydrogen gas. There remain, therefore, only 2 meafures that can be fuppofed to be produced from the muriatic acid gas; a quantity, Mr. Henry obferves, too fmall to afford grounds for fuppofing them to arife from decompofed acid. Now, in the decompofition of the water of the carbonated hydrogen gas by the carbon, the oxygen of the water unites with the carbon, and forms carbonic acid, which being foluble in water, would on the admiffion of water, after the electrifation, wholly, or in part, difappear; fo that the increafe is greater than 2, by all the carbonic acid diffolved in the water. It is not, therefore, improbable that the muriatic acid gas was in part decompofed in this experiment, and that the reafon why more of it was not decompofed is owing to there not having been more charcoal prefent, part of what there was being employed in decompofing the water. Indeed, when it is confidered that the carbon in carbonated hydrogen gas is minute in quantity, and that it has to decompofe the water contained in the two gafes before it can act on the muriatic acid gas, it appears that this is not the beft method of afcertaining whether carbon can decompofe muriatic acid. There appears to me no better method than to pafs muriatic acid gas, perfectly freed from water, over red hot charcoal, and to examine whether carbonic acid and hydrogen are produced.

Obfervations

X.

Obſervations on the Action which the Sulphate of Iron exerciſes upon Nitrous Gas, and on the Formation of Muriatic Acid. By Citizen BERTHOLLET*.

M. HUMBOLDT (in the Bulletin des Sciences, No. 71 & 21) had aſſerted that the faculty which ſulphate of iron poſſeſſes of abſorbing nitrous gas without the azote which might be mixed with it, was a means of aſcertaining with preciſion the purity of the nitrous gas employed in eudiometrical experiments.

Citizen Berthollet, on the contrary, is of opinion, that in this caſe the nitrous gas is not only abſorbed, but that it is decompoſed; that the oxygen abandons a part of the azote, and forms, with the other part, nitrous acid; that this decompoſition equally takes place by means of water, mercury, ſolution of pot-aſh, or of hydrogenated ſulphuret of alkali; that it is more or leſs complete in proportion as the liquid, in contact with this gas, contains bodies which have a greater degree of affinity to nitrous acid, in the formation of which they aſſiſt, and nitrites are formed; that in the decompoſition by water, there is leſs nitrate of ammoniac and more nitrous acid formed than M. Humboldt imagined; which tends to prove that water did not furniſh the whole of the oxygen of the acid, ſince the quantity of acid thus formed, is far more conſiderable than may be preſumed from the exiſting quantity of ammoniac. It is to be remarked that the decompoſition becomes more difficult as it is more advanced, and the gas contains leſs oxygen. Citizen Berthollet attributes the differences which are obſervable in nitrous gas, to the different proportions in which the oxygen and azote are combined, and he does not think they are owing to azote in a ſtate of ſimple mixture.

It is a known fact, that oxigenated muriatic acid, which has no action upon azote, abſorbs nitrous gas very well. M. Humboldt obſerved in this abſorption a reſidue, which he attributed to azote mixed with the nitrous gas. Citizen Berthollet having repeated the experiment with nitrous gas, carefully prepared, found a reſidue ſo ſmall as not to deſerve notice.

In a word, Citizen Berthollet reſtores to hydrogenated ſulphuret of pot-aſh and to phoſphorus the property of taking up all the oxygen of the atmoſpheric air; a property which had been conteſted by M. Humboldt, who ſays, that nitrous gas always indicated the exiſtence of a reſidue of oxygen in air which had been ſubmitted to their action. Citizen Berthollet abſolutely maintains the contrary. Nitrous gas experienced only a ſlight diminution with the reſidue of atmoſpheric air decompoſed by phoſphorus acid; and he attributes this to the abſorption which takes place in the nitrous gas of that portion of phoſphorus which had been taken up by the azote.

* Abridged from the Memoirs communicated to the French National Inſtitute (in the Bulletin de la Soc. Philom. No. 40.)

· *On the Nature of · Muriatic Acid.*

Two circumftances induced Citizen Berthollet to entertain fufpicions, and to inveftigate the nature of this acid.

The one is that which is announced in the Bulletin, No. 17, page 165, where M. Humboldt fays, that muriate of iron is formed by the abforption of nitrous gas, by means of fulphate of iron.

The other, which had been obferved by Cavendifh, is the precipitation of nitrate of filver into muriate, by the nitrite of pot-afh obtained from nitrate of pot-afh in part de-compofed by fire. By adding to thefe obfervations the circumftance of the almoft in-variable prefence of muriatic acid in every cafe where nitric acid is formed, and by means of feveral other experiments carefully made, Citizen Berthollet was led to the difcovery of the nature of muriatic acid.

He afcertained that nitrous gas does not precipitate the folution of filver. By repeating the experiment of Cavendifh, with nitrate of pot-afh, he found it accurate, and was more-over affured that the precipitation could not proceed from the nitrous gas of the nitrite ; for,

1ft, Nitrate of lime does not produce the fame refult.

2d, If a folution of iron be made in nitric acid, and it be fo managed as to load it with this metal, little ammoniac is formed ; the folution becomes turbid, and it does not preci-pitate the folution of filver. If a frefh quantity of iron be added to this folution, there is an effervefcence, with a precipitation of almoft all the oxide of iron, the liquid contains more ammoniac, together with muriatic acid, which is eafily demonftrable by the folution of filver. If this liquid be diftilled, that which comes over contains nothing but ammoniac ; the muriatic acid remaining in the retort with part of the ammoniac.

3d. Nitric folutions of tin, of zinc, of copper, made by heat, have fometimes yielded muriatic acid. But it is to be remarked that this acid was more generally found in pro-portion as the production of ammoniac was confiderable.

In thefe experiments there are anomalies, the caufe of which citizen Berthollet has not yet been able to affign. But they already prove that muriatic acid is formed under thefe circumftances, and that its formation cannot be attributed to the prefence of pot-afh. The principles of this acid muft therefore be fought for in water and nitric acid.

Its incombuftibility, its refiftance to decompofition, prove, that if it contains hydrogen and oxigen, thefe two bodies are not predominant, for it is a principle of the theory of affinities, that the difficulty of deftroying a compound is in proportion to the fmallnefs of the quantity of one of its component parts. Almoft all the proportions of the combinations of azote and oxigen are known. Citizen Berthollet, therefore, fuppofes himfelf to be juftified in thinking that muriatic acid is a triple compofition of oxygen, of hydrogen in a fmall quantity, and azote in a greater proportion. By adopting this opinion, the prefence of muriatic acid, in a great number of chemical phenomena, is eafily explained.

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Thus

Thus it appears (experiment 2.) that it is at the moment when the iron has decompofed almoft all the nitric acid, and it becomes neceffary alfo to decompofe the water, in order to oxidate more metal; that the muriatic acid, and the greateft part of the ammoniac, is formed by the decompofition of the water.

It is to the prefence of oxygen, azote, and hydrogen, that citizen Berthollet attributes the formation of muriatic acid in artificial nitre beds, when the materials do not previoufly contain any muriate.

Although muriatic acid, from the proportions of its conftituent principles, ought ftrongly to refift decompofition, citizen Berthollet thinks that he has obferved that this decompofition takes place under fome circumftances.

He thinks that the refidue which is left by oxygen gas difengaged by heat, from oxigenated muriate of pot-afh, is owing to the decompofition of a fmall portion of this acid. He had, at firft, attributed this refidue to a foreign caufe; but having remarked that it was more abundant at the end than at the beginning of the operation, he conceived it could not refult from fuch a caufe.

Citizen Berthollet clofes his Memoir, by fhewing, from more accurate experiments, that the black colour which muriate of filver affumes from light, heat, and even from a fimple current of air, ought not to be attributed, as he himfelf had fuppofed, to a gafeous difengagement of oxygen, but to the feparation of a part of the muriatic acid, without decompofition.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Philofophical Tranfactions of the Royal Society of London, for the Year 1800. *Part the Second. Quarto,* 46 *Pages, with Eleven Plates.* London, fold by Elmfley.

THIS part contains.—12. On double images, caufed by atmofpherical refraction. By William Hyde, Wollafton, M. D. F. R. S. (fee our prefent, vol. p. 298.) 13. Inveftigation of the powers of the prifmatic colours to heat and illuminate objects; with remarks, that prove the different refrangibility of radiant heat. To which is added, an enquiry into the method of viewing the fun advantageoufly, with telefcopes of large apertures and high magnifying powers. By William Herfchel L. L. D. F. R. S. 14. Experiments on the refrangibility of the invifible rays of the fun. By William Herfchel, L. L. D. F. R. S. (abridgment of thefe two papers is given at p. 320) of our prefent volume. 15. Experiments on the folar, and on the terreftrial rays that occafion heat; with a comparative view of the laws to which light and heat, or rather the rays which occafion them, are fubject, in order to determine whether they are the fame, or different. By William

Herfchel,

Herfchel, L. L. D. F. R. S. (fee p. 360 of this number). 16. Chemical experiments on zoophytes; with fome obfervations on the component parts of membrane, by Charles Hatchett, Efq. F. R. S. 17. On the electricity excited by the mere contract of conducting fubftances of different kinds. In a letter from Mr. Alexandar Volta, F. R. S. profeffor of natural philofophy in the univerfity of Pavia, to the Right Hon. Sir Jofeph Banks, Bart. K. B. F. R. S. (abridged in this Journal, IV. p. 179.)

Foffil Wood, found at a very great Elevation.

Citizen Villars has communicated a Memoir to the national inftitute of France, in which he relates his having feen, in the department of *Ifere*, foffil wood, bedded in turf, at the height of 2320 metres   above the (actual level of the fea, and 850 metres   above the moft elevated fituation, where wood grows, at prefent.   The mountain where he has made this interefting obfervation, is that of Lans, in the canton of Aifans.   The trees which are found are   The roots, and parts of the trunk are obfervable.   The latter of thefe trees no longer grows in the vicinity.

The author attributes the refrigeration which thefe mountains have experienced to two leading caufes ; the firft is the excavation of the valleys, which has changed the elevation of the fummits relative to their bafe, and the furrounding country; and the fecond is the deftruction of the ancient forefts, which extended contiguoufly upwards to great heights, but cannot again grow at fimilar elevations, when once deftroyed, and the trees thus deprived of their mutual fhelter and defence.   Soc. Philom. No. 33.

Extract of a Letter from Mr. Humphry Davy, dated October 23. fupplementary to his Paper on Galvanifm, in the prefent Number.

When I exhibited to Dr. Beddoes the phænomenon of the renovation of the powers of the galvanic pile, after it had ceafed to act in hydrogen, nitrogen, &c. by momentary immerfion in water, holding in folution atmofpheric air, he requefted me to try whether oxigenated muriatic acid gas, folution of nitre, &c. would not increafe the effects. The fear of deftroying the filver prevented me at the time from trying the experiment on oxigenated muriatic acid gas.   I have fince, however, in endeavouring to afcertain whether the electrical phænomena of galvanifm were producible when the plates of the pile were oxidating, (feparated from each other by a non-conductor, and in contact with an aeriform fluid only) had an opportunity of obferving the very great power of this fubftance in producing the galvanic decompofition of water.

Ten pairs of plates of filver and zinc were connected by cement, fo as to prevent the alternate contact of the metal. The fpaces filled by cloths in the common pile, were

4   uffered .

fuffered to remain open, fo as to admit of a free circulation of air. A tube, with water, and filver wires, was adapted to the extreme plates.

This pile was introduced, without being moiftened, into a veffel, provided with a ftopper, filled with oxigenated marine acid gas; but no perceptible galvanic action took place. After two hours, no gas had formed in the tube, nor had any oxidation of the zinc wire been produced [*]. The fame pile was now moiftened by immerfion in water. Before and after it had been wiped, it fhewed no figns of action in the atmofphere. It was introduced into a veffel of oxigenated marine acid gas, opened as before in the atmofphere. In a moment, the zinc wire, in the tube, began to oxidate with the greateft rapidity, whilft gas was given plentifully from the filver wire. The procefs continued to go on till the green colour had difappeared in the cylinder.

This experiment not only arranges with the facts of Fabroni and Colonel Haldane, and thofe I have before ftated; but likewife feems to prove that the chief ufe of the large furface of water required in the pile of Volta is to oxidate a larger quantity of zinc: for in this inftance very minute quantities of water connected the plates, and confequently very minute quantities were fufficient to enable the electrical currents to form the circuit.

I am at prefent engaged in endeavouring to afcertain by experiments whether any differences exift in the gafes evolved from water by the galvanic current, when different oxidating fubftances form the medium of communication between the plates. When thefe experiments are compleated, or at fome future time I fhall probably offer fome obfervations on the peculiar affinities which enable iron, zinc, &c. to decompofe water only when it holds in folution atmofpheric air, acids, or other bodies containing oxygen. On the principles before laid down, nothing is more eafy than to explain the ufe of muriate of foda, muriate of ammoniac, &c. in increafing the powers of the common pile.

As the quantity of power in a pile is probably in proportion to the quantity of the oxidation of the zinc, and the number of the feries of plates, the pile of Mr. Cruickfhank, or that I have defcribed, will probably be moft ufeful for procefies, in which much galvanic power is required. In thofe procefies, muriatic acid, or very diluted nitrous acid, may be ufed as the oxidating fubftance: for they will enable the plates to act till all the oxidable metal is deftroyed, without the common trouble of cleaning and rebuilding the pile.

Analyfis of various Foffils.

Trommsdorff, who has lately been engaged in mineral analyfes, has found, that the lapis obfidianus of mount Heckla in Iceland is compofed of 63 parts of filiceous earth, 20.5 parts of alumine, and 13.5 parts of oxide of iron.

That the heliotrope of Bohemia contains 68 parts of filiceous earth, 15 of alumine, 10 of oxide of iron.

[*] The metals of the pile had been acted upon in this experiment, and were warm at the conclufion of it.

That

That the black felfpar which is found at Unkel in the Bafaltes, is compofed of 66.5 parts of alumine, 15.0 of filiceous earth, 6.5 of oxide of iron, and 4.0 of oxide of manganefe.

That the blue calcedony of Siberia contains only filiceous earth.

That rock cryftal is the fame as pure filiceous earth.

That the Holzftein of Bareuth contains 80.0 parts of filiceous earth, 11.0 of alumine, 3.0 of oxide of iron, 1.0 of oxide of chrome; which proves that chrome is alfo found in Germany. He fufpects that it is likewife to be found in feveral other of the foffils of this country.

Lime in the Nut-gall.

Prouft did not obtain lime from the nut gall; Trommsdorff afcertained the prefence of this earth with precifion.

The method which Prouft propofes to procure pure gallic acid is not advantageous; at beft it is good for feparating this acid from the tanning principle, but it does not difengage it from the extractive matter, and when it is propofed to feparate the muriatic acid by evaporation, the elevated temperature which is neceffary to effect it, refolves the gallic acid into its principles, or at leaft carbonifes it.

Extraction of the Alkali of Sea Salt by Lime.

The decompofition of fulphate of foda by lime in order to obtain that alcali cannot be advantageous, becaufe this earth attacks the falt only in proportion as it is itfelf diffolved in water.

Method of obtaining Phofphorus by Phofphate of Lead.

Trommsdorff has made new attempts to render Giobert's procefs ufeful, by precipitating urine by nitrate and acetite of lead; but the phofphorus which he obtained was in fuch fmall quantities, that it did not pay the expence of the fire. The chemift of Turin would be juftified in affirming that his method is the beft, if his precipitate had been the pure phofphate of lead. But the reduced lead which remains in the retort is the phofphuret of that metal.

On the Mineral Kermes, and the golden Sulphur of Antimony.

Several new experiments have alfo been made by the fame chemift, on the red and orange coloured hydro-fulphurets of antimony, and he is perfectly fatisfied that the former confifts of oxide of antimony and fulphurated hydrogen; and that the latter is compofed of the fame principles, and a portion of fulphur not hydrogenated. By directing a cur-

rent

rent of fulphurated hydrogen gas into nitro-muriate of antimony, kermes immediately falls down, and afterwards when there is a fufficient quantity of acid at liberty to difhydroge-nate the fulphur, golden fulphur precipitates. The fame gas converts all the antimony contained in the acetite, and in the tartrite of this metal into kermes, which procefs affords the means of procuring this preparation of a nature perfectly uniform. *

Elaſtic Reſin in Opium.

M. Bucholz, a friend of Trommsdorf, has lately addreffed a very curious memoir to him upon opium, which he means to infert in his next Journal. He found, in particular, that opium contains a great quantity of elaſtic gum.

Diſcovery of a New Earth in the Beryl of Georgen-Stadt.

During the laſt ten years a mineral has been found in the mines near Georgen-Stadt, to which, on account of its refemblance to the beryl, the name of that foffil had been given. Trommsdorff undertook to analyze this fubſtance in the hopes of finding the glucine, but inſtead of that earth he found a new one, or one which was different from thofe hitherto known.

1ſt. In its pure ſtate it refembles alumine. 2. It is not more foluble either in the dry or humid way in the cauſtic alcalis, than in their carbonates. 3. Ammoniac, whether cauſtic or carbonated, exercifes no action upon it. 4. It retains the carbonic acid but weakly. 5. It acquires hardnefs, but not fapidity by fire. 6. It is not foluble in water. 7. It readily unites to acids with which it forms falts, which have little or no taſte. 8. The earth, hardened by fire, diffolves in the acids with the fame eafe as that which has not been fo treated. 9. It forms with fulphuric acid a falt little foluble, perfectly infipid, which when it is acidulated diffolves without difficulty, and becomes cryſtallized in ſtars. 10. Superfaturated with phofphoric acid it affords a very foluble falt. 11. Its acetite is of very fparing folubility. Its other characteriſtics, as well as his procefs of analyfis, will be detailed in the 1ſt part of the 8th Volume of his Journal. He has given the name of *Aguſtine* to this new earth, from its property of forming with acids falts without taſte *†.

On the Method of hearing by the Teeth.

Citizen Vidron, muſic maſter at Paris, has announced the difcovery of a method of rendering muſic audible by perfons deaf and dumb from their birth.

Citizens Hauy, Lacépède, and Cuvier, who were appointed by the National Inſtitute to examine this difcovery, made their report (the 21 Meffidor) in the year 8 (July 21, 1800.)

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* See Annales de Chimie XXXII. 257, for an account of all the circumſtances which determine the formation of the different fulphurets of antimony. *French editor.*

† The laſt feven articles are tranſlated from a letter of Trommsdorff in the Annales de Chimie XXXIV. 130.

Citizen

Citizen Vidron ufes a rod of fteel, one end of which he places upon the found-board of the mufical inftrument, and the other between the teeth of the deaf perfon. He adds to it a branch terminated by a brafs button which refts upon the cavity of the ftomach, and fometimes a third which he places upon the head.

The commiffioners have found that many authors have announced the fact of the deaf being made to hear by putting their teeth in communication with the inftrument by means of a ftick, a goblet, or fome other body. They have quoted, amongft others, Fabricius D'Aquapendente, Schelhammer, Boerhaave, Winkla, and Joriffen.

They have alfo found that in like circumftances fteel is better for the purpofe than wood, which had been almoft generally ufed before the time of citizen Vidron; but that his two branches are of no ufe with refpect to what may properly be called hearing.

They particularly endeavoured to determine to what extent this method might be ufeful; as well with refpect to the different kinds of deafnefs, as to the different kinds of founds which are propofed to be rendered audible.

They produced an artificial deafnefs in themfelves by ftopping their ears, and retiring to a diftance. In both cafes they heard diftinctly by means of the fteel rod, and the founds appeared to them to proceed from within this rod, and not from their true place.

But the perfons, really deaf, whom they have examined prefented very different refults. Some of them manifeftly heard; but the greater number declared they only experienced a tremulous action more or lefs general.

The commiffioners conclude, that this method may be ferviceable in deafnefs which only proceeds from obftructions in the external paffage, but that it is ufelefs in that which is caufed by a paralyfis of the nerve, or an effential derangement in the interior part; which is the moft common kind of deafnefs, efpecially in thofe who are deaf from their birth. They think it proper, neverthelefs, to make the experiment on all young perfons who are deaf, fince if only one out of an hundred fhould derive benefit from it, to that one at leaft it would be a fource of enjoyment.

As to articulated founds or fpeech the commiffioners found that it was almoft impoffible to hope for an exact tranfmiffion of them by this method, at leaft in its prefent ftate.

Societé Philomath. No. 41.

*** Mr. Leflie has favoured me with a correction of his paper, where page 346, line 19, for *public opinion* the words *popular opinion* are to be fubftituted. And he fears that other verbal inaccuracies may have occurred, as the copy was written in hafte, and the unexpected abfence of the author, during the impreffion, prevented any fubfequent revifion.

Fig. 1.

Fig. 2.

Intensity.

Red

Yellow

Green

Blue

Violet

Heat from colored Rays.
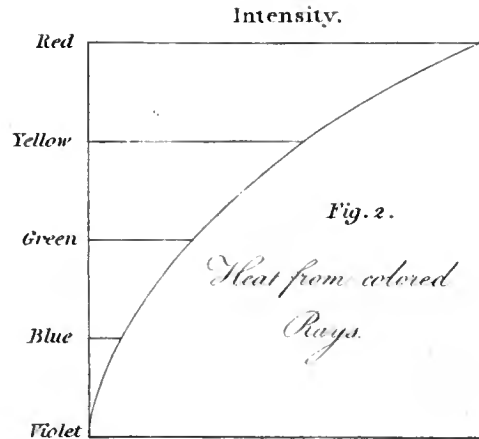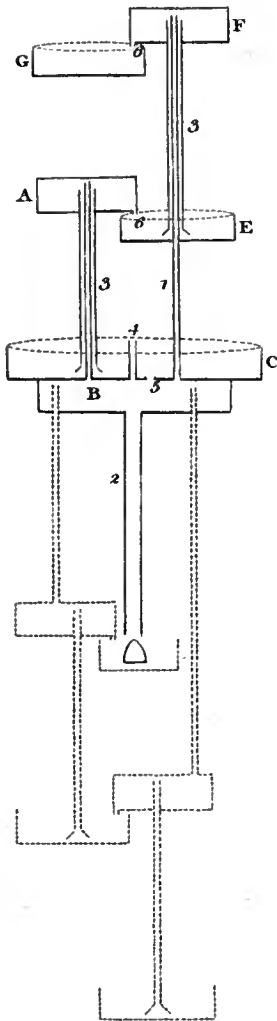
Fig. 3.
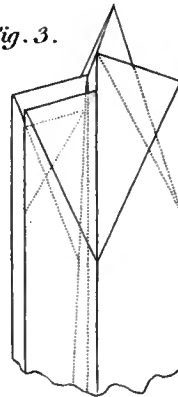
Arragonite

Fig. 4.
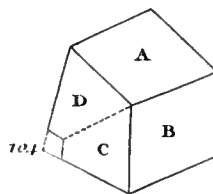
Fig. 5.
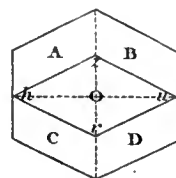
Radiation of heat.

Fig. 2.
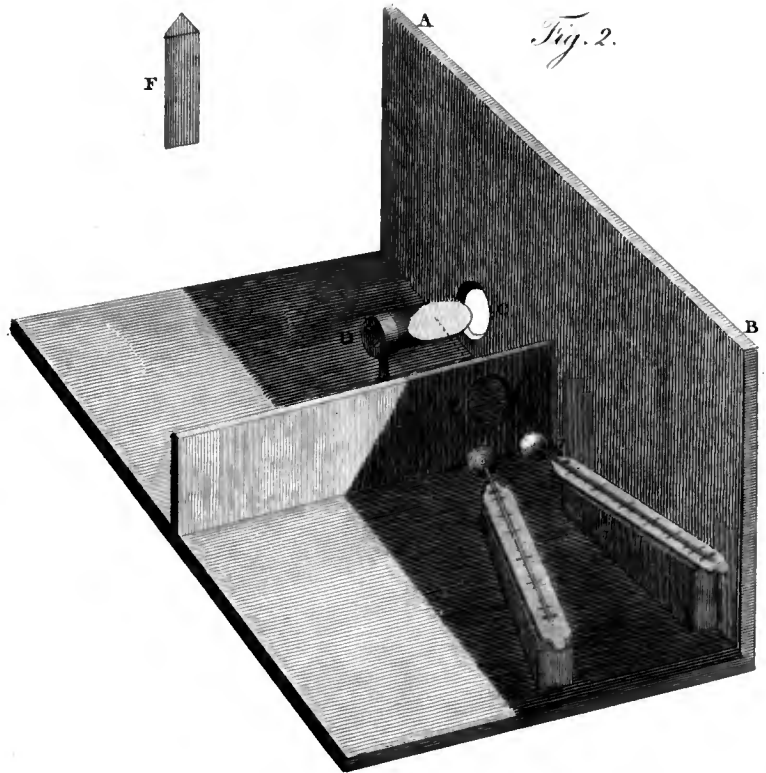
Fig. 1.

A JOURNAL

OF
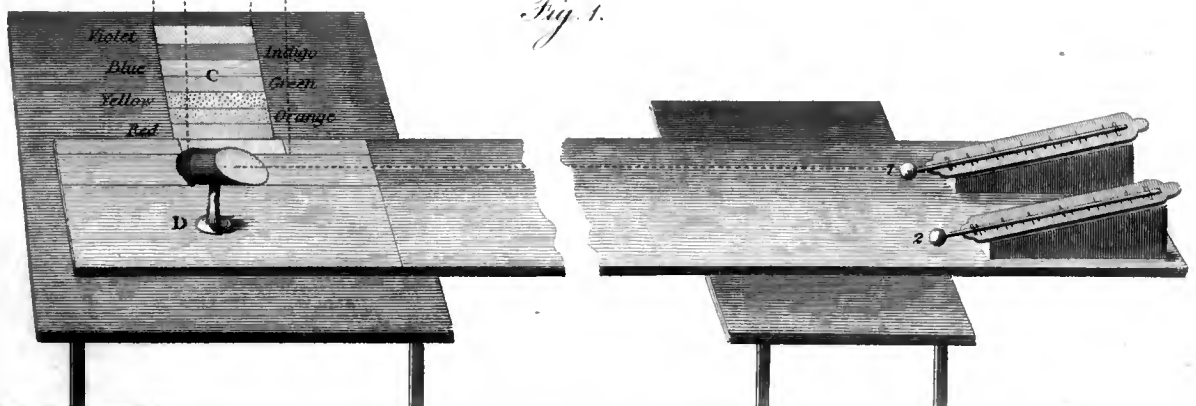
NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER 1800.

ARTICLE I.

On the Number of the Primitive Colorific Rays in Solar Light. By the Rev. MATTHEW YOUNG, *D. D. S. F. T. C. D. & M. R. I. A.*

THE opinion that there are but three primitive colours has been maintained by M. du Fay; and after him by Father Caftelli. See Montucla, Vol. I. p. 630.; but they and all others who hold the fame doctrine, defend it merely on the principles of a painter, who fhews how with thefe three colours on his pallet, he can compound all others; for with red and yellow he can form an orange colour; with blue and yellow he forms green; and with blue and red he forms indigo and violet; and thus having compounded the feven prifmatic colours, it is manifeft that all other colours, with their different gradations, can be formed from them likewife. But this *pharmaceutical* argument is by no means fufficient to fatisfy us as to the real compofition of folar light.

"Light, in refracting, is decompofed into feven rays, red, orange, yellow, green, "blue, indigo and violet. It has been fuppofed," fays Fourcroy, "that three of thefe "colours, the red, yellow, and blue, were fimple; and that the other four were formed "each of its two neighbours; that is, the orange from the red and yellow, the green from "the yellow and blue, the indigo from the blue and violet, and the violet from the red "and indigo. But this fuppofition has never been proved." See his Philofophy of Chem.

chap. 1. § 3. Befides that this is a mere hypothefis, unfupported by any fact, as Fourcroy obferves, we remark, that it is in itfelf inadequate; 1ft, becaufe in the folar fpectrum, the red and indigo are not *neighbouring* colours, but are almoft at the greateft poffible diftance from each other. 2dly, According to this hypothefis, indigo is compofed of blue and violet; but violet is compofed of red and indigo; indigo therefore is compofed of red, blue and indigo, that is, indigo itfelf is one of its own effential ingredients, which is abfurd.

The experiments of the prifm feem to eftablifh, in a very clear manner, the exiftence of feven original and uncompounded colours; and though green, for inftance, may be compounded of blue and yellow, yet it does not directly follow from thence, that it always is fo actually compounded. Accordingly Newton tells us, that green may be exhibited in two different ways, either by primitive, green-making rays, which are fimple and not refolvable by any reflection or refraction into different rays; or by a compofition of blue and yellow rays, which are differently refrangible, and which therefore after their union, may again be feparated by refraction, and exhibit their proper colours of blue and yellow.

On this doctrine of the two-fold generation of green, we may in the firft place remark, that the ancient, received axiom " *Deus nil agit fuftra*" ought not to be too haftily abandoned, as it muft appear to be, if this doctrine be maintained: for if green may be produced by blue and yellow, then blue and yellow being already exiftent, green is a confequence; and therefore peculiar rays formed for the production of green are fuperfluous. Though I acknowledge, that this maxim is not fo cogent or felf-evident, as to preclude all objection, yet fince the general obfervation of nature feems to fhew, that this wafte of power or multiplicity of means is not adopted by the Supreme Artift, it certainly feems juftly entitled to our attention, at leaft fo far as this, that we fhould be careful in fhewing, that we are led to thefe different caufes of the fame effect, by a legitimate and cautious analyfis.

In defence of the doctrine of three primitive colours only, F. Caftelli contents himfelf with faying, that the colours of the prifm are immaterial, accidental, artificial, and therefore unworthy the regard of a philofopher; whereas the colours of painters are fubftantial, natural, palpable. From them, of confequence, the theory of chromatics fhould be deduced; but they tell us, that there are but three parent colours, which give birth to all others.

In reply to this we need only obferve, that Sir I. Newton has proved, that the colours of natural bodies depend on the colorific qualities of the rays of light; and therefore that our theory of colours muft be derived from an enquiry into the conftitution of folar light, for according to that conftitution the colours of bodies will vary: and he farther fhews, that if folar light confifted of but one fort of rays, all bodies in the world would be of the fame colour. However true therefore F. Caftelli's theory may be, the manner in which he deduces it from phænomena is unqueftionably falfe.

I fhall therefore proceed to enquire fcrupuloufly into the compofition of the folar fpectrum, from which, without doubt, the true doctrine of the origin of colours is to be derived.

If

If the folar light confifted of feven primitive, homogeneal coloured rays, and that thefe homogeneal rays were equally refrangible, the fpectrum would confift of feven circles of different colours, fince the homogeneal rays of each colour would paint a circular image of the fun. But it is manifeft, that feven circles could not compofe an oblong fpectrum, with rectilineal fides. Therefore the rays of the fame denomination of colour muft be differently refrangible. Which is alfo made ftill farther evident by obfervation of the fpectrum, fince in it we perceive, that the prifmatic colours are diffufed over fpaces, which are, on the fides, terminated by right lines, and therefore the centers of the circles of the fame denomination of colour are diffufed over lines equal to thefe fegments of the rectilineal fides of the fpectrum. Newton has fhewn, prop. 4. B. 1. Optics, how to feparate from one another the heterogeneous rays of compound light, by diminifhing the breadth of the fpectrum, its length remaining unchanged; and when the length of the fpectrum is to its breadth, as 72 to 1, the light of the image is feventy-one times lefs compound than the fun's direct light. In the middle of a black paper he made a round hole, about a fifth or a fixth part of an inch in diameter, upon which he caufed this fpectrum fo to fall, that fome part of the light might pafs through the whole of the paper; this tranfmitted part of the light he refracted with a prifm placed behind the paper, and letting the light fall perpendicularly upon a white paper, he found that the fpectrum formed by it was perfectly circular. Hence, therefore, it follows, that the equally refrangible rays occupy a fpace on the rectilineal fides of the fpectrum equal at leaft to the fifth or fixth part of an inch; that is, the rays of the fame colour are differently refrangible.

The different quantity of the homogeneous rays of different colours will not account for the different fpaces they occupy in the fpectrum; for this difference in quantity would affect only the intenfity of the colour, not the magnitude of the fpace which it would occupy. All the red light therefore is not homogeneous; but confifts of rays of innumerable, different degrees of refrangibility; and fo of the other colours.

Now fince the rays which are of the fame denomination of colour are differently refrangible, they will either form oblong fpectrums detached from each other; or they will in part lap over, and fall on each other. The former pofition is manifeftly falfe: therefore the original prifmatic colours will partly lap over and fall on each other, and therefore neceffarily generate the intermediate colours. And fo Sir I. Newton obferves, where he fays, that the original, prifmatic colours will not be difturbed by the intermixture of the conterminous rays, which are intermixed together. This overlapping however, which Newton fpeaks of, arifes only from the fun's having a fenfible diameter, and does not neceffarily imply an equal refrangibility in any differently coloured rays. If there be but three original prifmatic colours, red, yellow, and blue, and that the red and yellow lap over, fo as that there fhall be a certain fpace in the fides of the fpectrum equally occupied by yellow and red circles, then will thefe circles by their intermixture compound an orange colour; and this colour as to refrangibility will be homogeneous, becaufe the coincident rays of different colours are equally refrangible. In like manner green may be compounded by the

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mixture

mixture of blue and yellow circles, equally refrangible. Now this is fimple, and conformable to the other phænomena of the fpectrum; for if rays of the fame denomination of colour be differently refrangible, it is not unreafonable to fuppofe, that rays of a different denomination of colour may be equally refrangible; and therefore fince the red rays are unequally refrangible, and likewife the yellow, there is nothing incongruous in fuppofing that fome of the lefs refrangible of the yellow may be equally refrangible with fome of the more refrangible of the red; and if fo, they will confequently be intermixed with them : and the fame may be faid of the green. This hypothefis likewife receives confiderable ftrength from this confideration, that the orange, green, indigo, and violet occupy thofe places which they ought to do, in cafe there were but three primitive colours, red, yellow, and blue : thus the orange lies between the red and yellow, becaufe it is formed by fome of the extreme rays of red and yellow, which are equally refrangible; in like manner the green lies between the blue and yellow, becaufe it is formed by the mixture of blue and yellow. The indigo and violet muft alfo occupy the extreme part of the fpectrum, where the moft refrangible red and blue rays are united, and gradually becoming more and more dilute, fade away, and at length entirely vanifh. But if the orange, green, indigo and violet be primitive colours, there is no apparent reafon why they fhould have had fuch degrees of refrangibility affigned them, as that they fhould occupy the places they do, rather than any other.

Moreover, if thefe three colours red, yellow, and blue be the primitive colours, they cannot themfelves be generated; and accordingly we find, that yellow cannot be generated by the mixture of the adjacent prifmatic colours, orange and green; and the reafon of this is evident, becaufe orange is compounded of red and yellow; and green is compounded of yellow and blue; but red and blue compofe purple; which added to the yellow will generate a new compound colour, viz. a fickly green, differing manifeftly from yellow, the colour which ought to refult according to the analogy of the other primitive colours, in which the extremes, by their mixture, generate that which is intermediate. In the fame manner, blue cannot be generated by the mixture of green and indigo, becaufe green is compofed of yellow and blue, and indigo of blue and violet; therefore the refulting colour is compofed of blue, yellow, and violet; but yellow and violet do not compofe blue, therefore neither will blue, yellow, and violet compofe a blue colour. Now if orange and green be primitive colours, in the fame manner as red, yellow, and blue, we can affign no reafon why blue fhould not be generated by the mixture of the adjacent colours, as well as green and orange. But it is a received principle, that an hypothefis fhould folve all the phænomena; of the two hypothefes therefore, namely, that there are feven primitive colours, differently refrangible; or that there are but three, fome of which, of each fpecies, are equally refrangible; the latter alone folves all the phænomena of the folar fpectrum, and therefore is to be preferred.

If it be faid, that thofe rays which are equally refrangible muft excite the fame fenfation on the retina, becaufe they muft have the fame momentum; it is replied, 1ft, That it has

not yet been proved, that the senfation of different colours depends on the different momentum of the rays. 2dly, The rays may have different momentums, and yet be equally refrangible; for fince refraction is fuppofed to depend on the attractive force of the denfer medium, we muft fuppofe it analogous to the attractive force of gravity, which is proportional to the quantity of matter; and therefore the greater or lefs quantity of matter in a particle of light would produce no alteration in its refraction. Neither can the different refrangibility depend on the different velocity of the rays; becaufe the difference of refrangibility of the red and violet rays is much greater in flint glafs than in crown glafs; and this would require a proportionably greater difference in the original velocities, which cannot be. And this fame argument holds equally againft the former hypothefis, that the difference of refrangibility depends on the different magnitude or denfity of the particles of light. 3dly, Refraction feems to arife from a fpecies of elective attraction, fince different mediums which act on the mean rays equally, act on the extreme rays unequally: hence rays of the fame quantity of matter and velocity, and therefore of the fame momentum, may be diverfely refracted; and rays of different momentums equally refracted.

Nor is it to be wondered at that the rays of light fhould be differently refrangible, independent of any regard to their momentum, when we confider, that the different coloured rays appear to be combined with combuftible bodies, with different degrees of attractive force. For in combuftion we find, that different bodies are difpofed to part with different rays with greater facility; but when the combuftion is fufficiently rapid, they part with all the different coloured rays together, and the flame is therefore white; and this is what is called a white heat. Dr. Fordyce in the Phil. Tranf. for 1776, tells us, that when the heated fubftances are colourlefs, they firft emit a red light; then a red mixed with yellow, and laftly, with a great degree of heat, a pure white. All this is wonderfully conformable to the refraction of light by tranfparent fubftances, which refract, and therefore attract the red light lefs, and confequently in combuftion part with it more eafily. On the other hand I know it is generally believed, that the light in combuftion proceeds from the air, but this circumftance of the different colour of the light in different cafes, feems to overturn this opinion; for if vital air were oxygen diffolved in caloric and light, then the oxygen being abforbed by the burning body, the light extricated would in all cafes be of the fame nature; the greater or lefs rapidity of the combuftion would only produce an extrication of a greater or lefs quantity of light, but could not produce any variation in its nature, it being neceffarily the fame in all cafes, to wit, that in which vital air is diffolved. But the truth or falfhood of this reafoning will not affect the validity of the pofition, that the refrangibility of the rays of light cannot depend on the different magnitude, denfity, or velocity of the particles.

But though fpeculation feems thus to render it probable, that there are but three parent colours; our theory muft ever remain unfatisfactory, unlefs it receives the fanction of direct experiment. In this however there is no fmall difficulty; for fince the rays of light which compofe any given individual point of the colours of orange, green, violet, and indigo are

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equally refrangible, they will be also equally reflexible; and therefore cannot be separated either by refraction or reflection, so as to exhibit the different coloured rays of which they are composed. It seems therefore, that the only way remaining, by which we can experimentally ascertain the composition of these colours, if they be indeed compound, is transmission. For since transparent coloured bodies are such merely by their letting pass through them either solely, or more copiously, rays of a certain colour, and intercepting all others, such transparent bodies, applied to compound colours, will ascertain that composition, by extinguishing, in a great measure, all rays except such as are so adapted to its conformation, as to pass through it, and give it its peculiar denomination of colour.

In order to try the truth of the hypothesis of seven colours by this test, I looked through a blue glass at the red end of the spectrum : now we are to consider, that if that part of the spectrum was composed of red rays, and none other, the only effect of the blue glass would either be a total or partial suffocation of the red rays; and therefore that part of the spectrum, when looked at through the glass, would either totally disappear, or become a faint and diluted red. But, on experiment it appeared of a purple colour. The purple in this case could not be a primitive and original colour, as is manifest, because it did not proceed from the purple part of the spectrum ; we must therefore conclude, that it was a compound colour. But purple, when compound, is made up of blue and red, therefore it follows, that some blue rays did actually exist in the red part of the spectrum : which combined with the few, straggling red rays which penetrated the blue glass, composed that purple colour, which the red extremity of the spectrum assumed, when viewed by the light transmitted through the blue medium.

To try, on the other hand, whether any red rays lay hid amongst the blue, I proceeded in the same manner, and looking at the bluest part of the spectrum through a red glass, it appeared of a purple colour : some red rays therefore are equally refrangible with the blue ; and if the red extends as far as the blue, there is no reason why we may not suppose that it extends somewhat farther, so as to compound, with a diluted blue, the extreme colours of the spectrum, indigo and violet.

But it may be said, that if blue rays existed amongst the red, that part of the spectrum could not appear so extremely brilliant as it really does ; but would put on a purplish appearance in the spectrum itself, even to the naked eye. In answer to this objection we may observe, that the most intense and vivid, natural red bodies do, in fact, reflect a very great proportion of blue rays, because they appear of a strong blue colour when placed in the blue part of the spectrum ; and therefore they reflect just as many when the direct, white solar light falls on them, in which all that blue is involved ; though by the predominance of the red rays, they appear of that colour, without any visible tincture of blue.

In order to determine whether the purple appearance of the red extremity of the spectrum, when viewed through a blue glass, was caused by any of the white solar light, which might perhaps be reflected from the air, or surrounding objects to the spectrum, and thus throw on that part such a quantity of blue as might produce a sensible effect; I caused the

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middle

middle and moſt intenſe part of the red to paſs through a hole in a blackened paper, and then fall on an optical ſcreen; by which I was ſure that I had as pure and uncompounded a red as could be deſired; which alſo underwent the uſual teſt of purity by ſubſequent re-fraction, without any change in the form of the ſpectrum; I then looked at the body which was illuminated with this red, through the ſame blue glaſs, and the effect was the ſame as before.

To try this doctrine of three parent colours ſtill farther, I conſidered, that if the orange were really compounded of the red and yellow rays, then by looking at the orange through a red glaſs, the orange would in a great meaſure vaniſh, and the red would appear to ex-tend much farther than in the original ſpectrum; becauſe the yellow rays being conſider-ably obſtructed, the red would become more predominant; and that part of the ſpectrum, which before appeared orange, in conſequence of a certain mixture of yellow and red, would now, by the failure of ſo conſiderable a part of the yellow, loſe its orange appear-ance, and put on that of red: and, on experiment, I found the caſe to be ſo really in fact; for while an aſſiſtant looked at the ſpectrum through the red glaſs, I moved an obſtacle from the red towards the other end of the ſpectrum, deſiring him to ſtop me, when the obſtacle ſhould arrive at the confines of red and orange; but when he did ſo, the obſtacle had attained the middle of the orange, or rather had paſſed beyond it. Now if the orange were really a primitive colour, I ſhould ſuppoſe, that when looked at through the red glaſs, it would either appear diluted, without any change of dimenſions; or that if the weak part of the orange, next the red, ſhould vaniſh, by the obſtruction of the glaſs, a dark interval would appear between the orange and the red; in neither caſe can we account for the apparent extenſion of the red into the region of the orange; nor by any other hypotheſis, as appears to me, than that ſome of the red rays are equally refrangible with ſome of the orange.

There is another argument derived from the ocular ſpectra of Dr. Darwin, which ſtill further corroborates the doctrine of three primogenial colours. Place a piece of coloured ſilk, about an inch in diameter, on a ſheet of white paper, about half a yard from your eyes; look ſteadily upon it for a minute; then remove your eyes upon another part of the white paper, and a ſpectrum will be ſeen of the form of the ſilk thus inſpected, but of a different colour, thus

Red ſilk produced a green ſpectrum,
Green - red,
Orange - blue,
Blue - orange,
Yellow - violet,
Violet - yellow.

The reaſon of theſe phænomena is very ingeniouſly aſſigned by Dr. Darwin; he ſays, that the retina being excited into a violent and long continued. action by the red rays, in the firſt experiment, at length is ſo fatigued as to become inſenſible to them; but that it

ſtill

ftill remains fenfible, that is, liable to be excited into action by any other colours at the fame time; and therefore the fpectrum affumes a green appearance, becaufe if all the red rays be taken out of the folar light, the remaining rays will compofe green. See Phil. Tranf. Vol. LXXVI. Converfely, a green object produces a red ocular fpectrum. Now we may obferve, that if all the green rays be taken out of the folar fpectrum of feven colours, the remaining colours will not compound red. If indeed green be not a primitive colour, but a compofition of blue and yellow, then will the eye, in looking on a green object, be at once affected by blue and yellow rays; and therefore become infenfible to them both; and confequently the fpectrum will appear red. But if green be a primitive, original colour, generated by its own peculiar green-making rays, the eye in contemplating a green object, will become infenfible only to the green rays; and therefore the other fix prifmatic colours, which are fpecifically different from the green, ought to be fenfible, and produce their proper compound effect; but this would not be the fenfation of red. In like manner, if the object be yellow, the eye will at length become infenfible to the yellow-making rays, and the fpectrum will be violet. Now fince on the hypothefis of feven original colours, the orange and green are primitive, though the eye be rendered infenfible to the yellow rays, it will not be fo to the orange and green, which therefore, together with the red, blue, violet and indigo, will produce their compound effect; but the colour refulting from this joint action is not violet, which neverthelefs is the colour of the ocular fpectrum. On the other hand, if there be but three primitive colours, red, yellow, and blue, when the eye is infenfible to the yellow-making rays, the fpectrum muft neceffarily be violet, which is the colour that refults from the mixture of red and blue. If it be objected, that the eye is not only infenfible to the unmixed yellow rays, but likewife to the yellow of the orange and the green, then it is admitted that orange and green are compound colours. Befides, fince the colour which would refult from the mixture of red, orange, green, blue, indigo and violet is not yellow, the eye ought not to be infenfible to this colour; and confequently, fince by the exemption of the yellow rays from the white folar light, that colour does not refult, but a diftinct purple, it follows, that the orange and green are not primitive colours inherent in folar light.

It remains now only for us to fhew, that the three colours of red, yellow, and blue, are adequate to the folution of all the phænomena of chromatics. But in order to fhew this, few words will be fufficient, for having feen, that the feven prifmatic colours can be generated by thefe three, it follows that all others can be generated from them, as Sir I. Newton has proved at large. However, I think it will not be fuperfluous to obferve, that white may be directly produced by thefe three colours, without the previous generation of the other four prifmatic colours, in the fame manner as it is ufually generated with feven. " I could never yet," fays Newton, " by mixing only two primary colours, produce a per- " fect white. Whether it may be compofed of a mixture of three, taken at equal diftances " in the circumference, I do not know." Now to fhew that white may be thus generated, let an annulus of about four inches diameter be divided into three parts by lines tending

towards

towards the centre, and let thefe three divifions be refpectively painted red, yellow, and blue, in proportions to be afcertained by trial; then if the annulus be turned fwiftly round its centre, it will appear white. That white may be generated by the mixture of only the three colours red, yellow, and blue, might alfo appear from the rule which Newton him-felf has given us, for determining the colour of the compound which refults from the mixture of any primary colours, the quantity and quality of each being given.

The rule is this, the circumference of a circle is diftinguifhed into feven arches propor-tional to the feven mufical intervals in an octave, that is, proportional to the numbers 45, 27, 48, 60, 60, 40, 80: the firft part is to reprefent a red colour, the fecond orange, the third yellow, the fourth green, the fifth blue, the fixth indigo, and the feventh violet. Thefe are to be confidered to be all the colours of uncompounded light gradually paffing into one another, as they do when made by prifms, the circumference reprefenting the whole feries of colours from one end of the fun's coloured image to the other. Round the centers of gravity of thefe arches let circles proportional to the number of rays of each co-lour in the given mixture be defcribed. Find the common centre of gravity of all thefe circles, and if this common centre of gravity coincide with the centre of the circle, Newton fays that the compound will be white. Join therefore the centers of gravity of the blue and yellow circles, and from the centre of the red circle draw a right line through the centre of the principal circle; from the conftruction it will cut the line which joins the centers of the blue and yellow circles; if therefore the number of the blue and yellow rays be to each other inverfely as their diftances from the point where the line which joins their centers is cut by the line drawn from the centre of the red circle; and if the number of red rays be to the fum of the yellow and blue rays inverfely as the diftances of the centre of the red circle, and the common centre of the yellow and blue from the centre of the principal circle, the common centre of gravity of the red, blue and yellow circles will coincide with the centre of the principal circle, and therefore the refulting compound will be white.

But it is manifeft that this conftruction cannot be relied on, becaufe the quantities of the rays of any given colour in folar light, do not appear to be proportional to the fpaces which they occupy in the rectilineal fides of the fpectrum. Thus it is known that the yellow making rays are predominant in folar light, yet the fpace they occupy in the fpec-trum is to the fpace occupied either by green or blue as four to five, and to the fpace oc-cupied by the violet only as three to five.

II.

An Account of fome Additional Experiments and Obfervations on the Galvanic Phenomena. By Mr. Davy, Superintendant of the Pneumatic Inftitution. Communicated by the Author.

I.

Additional Experiments on the Caufes of the Galvanic Phenomena.

1. S ULPHURIC acid, when highly concentrated, is poffeffed of but little power of action upon zinc, though when diluted it diffolves it with the greateft rapidity. Affuming then the truth of the principles advanced in my laft paper, namely, that the powers of the pile of Volta are primarily excited by the oxidation of the zinc, it follows, that diluted fulphuric acid, when made the medium of connection between the pairs of plates, ought to produce much greater effects than concentrated fulphuric acid.

This I have found is actually the cafe. When the cells of a feries of twenty pairs of plates of filver and zinc, conftructed with waxen cement, in the mode defcribed in 8 of my laft paper, were filled with fulphuric acid, nearly of fpecific gravity 1.9, no galvanic action, except the production of a flight cauftic tafte, was perceptible by the ufual methods of trial; though when diluted fulphuric acid was ufed the ends of the feries gave fhocks to the moiftened fingers, and wires connected with them effected the ufual changes in water.

That concentrated fulphuric acid is not of that order of more perfect galvanic conductors which, when interpofed between the plates, deftroy their electrical effects, is evident from the following experiment: the cells of ten pairs of plates of copper and zinc, conftructed with waxen cement, were filled with concentrated fulphuric acid; but not the flighteft galvanic power was produced. A fmall drop of water was then poured upon the acid in each of the cells. The action of the feries was immediately fhewn by its producing the ufual appearances on wires in water.

2. The galvanic conducting powers of liquid fulphurets are at leaft equal to thofe of water. I found that when the fingers were plunged into glaffes, containing folution of fulphuret of ftrontian connected with the ends of a pile, the fhock was full as fenfible as if the communication had been made through water. When the galvanic current was made to act on folution of fulphuret of ftrontian by means of filver wires, the zinc wire became blackened, and gas was given out round the filver wire. But folutions of fulphurets are incapable of giving oxygen to zinc; they, confequently, ought to produce no galvanic effects when made the media of connexion of the double plates in the pile of Volta. Twenty-five pairs of filver and zinc, erected with cloths moiftened in folution of ful-

phuret

phuret of ftrontian, produced no fenfible galvanic action, though the moment the fides of the pile were moiftened with a little nitrous acid, the ends gave fhocks as powerful as thofe of a fimilar common pile.

3. No phenomenon is more conftant than the ceffation of the action of the common galvanic pile in a vacuum * when the gage is below one-tenth. Suppofing the expulfion of atmofpheric air from the water preventing it from oxidating the zinc, the fole caufe of this ceffation, it follows, that a pile ought to act in vacuo when nitrous acid, or diluted fulphuric acid, is the medium of connexion between the plates. Into each of the cells of a feries of twelve pairs of filver and zinc plates, which had juft been moiftened with a little water, a large drop of nitric acid was introduced: when the wires connected with the ends immediately began to produce the ufual appearances in water. The feries was introduced under the receiver of an air pump, and the filver wire from its zinc end connected with a veffel of water that had been long boiled. The wire from its filver end was fo faftened by refinous cement, to a fliding brafs wire paffing through the top of the receiver, that it could be plunged at pleafure into the water when the vacuum was made. The receiver was exhaufted till the gage ftood at $\frac{6}{10}$ of an inch, when the communication was effected. The zinc wire immediately began to oxidate, and gas was given out round the filver wire. The procefs went on for many minutes, and when it had ceafed, was not fenfibly revived by the admiffion of the atmofphere. In another experiment the fame phenomena were obferved. Gas appeared to be given out more rapidly from the filver wire than in the atmofphere; but this was from the diminution of preffure. The oxidation was certainly lefs: which may be eafily accounted for, when we confider, that no nitrous acid could be recompofed in vacuo, as in the atmofphere from the nitrous gas difengaged between the plates, and that, in confequence of the diminifhed preffure, fome of the acid muft probably have affumed the aëriform ftate.

A drop of fulphuric acid, poured into each of the moiftened cells of twelve pairs of plates, enabled the wires from the ends to effect the ufual changes in pure water for rather more than half an hour in vacuo, the gage being at $\frac{6}{10}$. The oxidation went on nearly as vividly as in the atmofphere, and what is rather remarkable, fome gas was given out from the oxidating wire, though very little was produced from the filver wire.

4. The refults of the laft experiment are interefting not only from their coincidence with the other facts, but likewife becaufe they afford proofs that the prefence of oxygen in that loofely combined or peculiar ftate in which, when abforbed by combuftible bodies, it produces inflammation, and in which, in my infant chemical fpeculations, I fuppofed it to be combined with the matter of light, is not effential to the galvanic effects. Whether water is abfolutely effential, we fhall find fome difficulty in determining, as it exifts in larger or fmaller quantities in all the non-metallic fluid galvanic conductors that have been yet experimented upon. The following fact is in favour of

* See Colonel Haldane, Phil. Journal, No. 43, Vol. IV. and my laft paper, No. 44.

its

its eſſentiality. The compound of concentrated ſulphuric acid and oxigenated muriatic acid * (which may be formed by introducing oxigenated muriate of pot-aſh into ſulphuric acid, or by paſſing oxigenated muriatic acid gas through it), ſlowly oxidates both zinc and ſilver, the oxigenated marine acid being decompoſed. I expected that it would produce ſtrong galvanic effects when made the communicating medium of the cells of a pile; but in this I was diſappointed; a ſeries of twenty pairs connected by it produced hardly any ſenſible action.

5. If any perſon wiſhes to repeat the experiments in vacuo juſt detailed, great caution muſt be obſerved with regard to the quantities of acids introduced into the cells. Two or three drops in each will be ſufficient, particularly if it has been previouſly a little moiſtened. When larger portions are employed, the efferveſcence highly increaſed by the removal of atmoſpheric preſſure will be often ſufficient to moiſten the edges of the cells, and to make a communication betweem them. In conſequence of the uſe of too much acid, I have made many unſucceſsful experiments.

The water uſed in vacuo for connecting the wires ſhould be always previouſly deprived of looſe air by long ebullition, or otherwiſe the diſengagement of that ſubſtance from it will much diſturb the reſults.

II.

Obſervations gained from minute Inſpection of Galvanic Proceſſes.

1. Whenever the galvanic circuit, paſſing through the pile with wires, is broken by means of water, oxygen is uniformly produced at the zinc metallic point, and hydrogen at the ſilver metallic point. This is ſhewn from many experiments in Mr. Nicholſon's Philoſophical Journal. Conſidering analogies, an intereſting queſtion occurs. Do not the ſame phenomena take place in every part of the ſeries? *i. e.* is not oxygen fixed on every plate of zinc, and hydrogen produced on every plate of ſilver, at the points of their contact with the water of the cloths? With the hopes of gaining a ſolution of this queſtion, I conſtructed a ſeries of twenty glaſſes with ſpring water, containing plates of ſilver and zinc connected by braſs wire, in the mode pointed out by Volta. This ſeries gave feeble ſhocks, and a ſilver wire connected with it produced the uſual appearances in water. Oxygen was fixed upon that part of the wire in the glaſs containing the laſt ſilver plate, and hydrogen was liberated from that part of it in the glaſs containing the laſt zinc plate.

The ſeries was made analogous in all its parts, the end glaſſes being connected by a pair of plates, ſo that every glaſs contained a ſilver plate and a zinc plate. On minutely inſpecting the glaſſes immediately after, no particular phenomena could be perceived. But after occaſional attention to the proceſs for many hours, I obſerved the zinc plates

* I accidentally diſcovered this combination in July, 1799. Some of its properties are very peculiar. More intereſting enquiries have hitherto prevented me from minutely examining them.

beginning

beginning to oxidate in many of their points, though no gas was produced upon them. No gas had formed upon the silver plates, but the surface of the water in contact with them became covered with an opaque white pellicle.

2. The silver plates used in this experiment were not perfectly polished. This might have influenced the results: and some gas might have escaped my observation. That the operation might go on in closed vessels, I cut off the bottoms of some bottles with a file, so that they could be easily joined again by cement. Into one of these bottles I introduced a plate of polished zinc *, and into another a plate of polished silver. The plates were connected by a wire attached to their upper angles, which protruded into the atmosphere through orifices made at the places of junction of the bottoms of the phials with their sides; these places of junction being rendered perfectly water-tight by resinous cement. Four apparatusses of this kind were constructed. They were filled with pump water, inverted in the galvanic order in glasses containing that fluid, and made part of a connected series of twenty glasses.

After more than twelve hours, the zinc plates had become tarnished, but had given out no gas. In two of the bottles with the silver plates, globules of gas, too small to be analysed, had collected: these plates examined in the atmosphere, as well as all the other silver plates, were covered in some points with a film of white substance, which was soluble with slight effervescence, and without producing cloudiness in muriatic acid.

3. Unable to account for the non-appearance of hydrogen during the oxidation of the zinc, I could not but conclude that it was condensed or absorbed in some new compound on the surface of the silver or the zinc. Guessing that the quantity of surface might be connected with the phenomenon of its non-appearance, I substituted in three of the phials for the square silver plates oblong ones of the same length, and about ,3 inches wide. These had not been long connected with the series before gas began to form upon them; and in five hours sufficient was collected to be examined: from the coarse test of inflammability it appeared to be hydrogen.

Thirteen pairs of a connected series of twenty-five glasses were now composed of square zinc plates, and oblong silver plates of different sizes; some of them being about ,3, and others not more than ,1 inches wide. Gas was almost immediately given out from the greater number of the oblong plates, and in largest quantities from the smallest: from the slips of ,1 wide indeed a constant stream of globules ascended through the water.

Small oval, circular, and square plates of nearly equal surfaces, with the slips connected in the series in the places of some of them, produced precisely the same effects. In short, whenever the surfaces of the silver plates did not exceed one-fourth of the quan-

* Of 1.2 inches square, the size used in all the former experiments.

tity of the furfaces of the zinc plates, whatever were their forms, gas was always produced upon them: and both large and fmall furfaces in common water in a great length of time became covered at fome of their points of contact with that fluid with a whitifh film.

When fmall oblong zinc plates were introduced into any part of the feries inftead of the larger plates, they appeared to oxidate rapidly, without giving out any gas.

4. The fubftitution of oblong filver flips for many of the plates, did not apparently much diminifh the power of the feries: I therefore conftructed a feries of twenty-feven glaffes, wholly compofed of zinc plates attached to thin filver wires. This combination with pump water gave feeble fhocks, which were lefs vivid than thofe of the common feries of eighteen. When, however, it was made analogous in all its parts, all the wires not deeply inferted in the water gave out gas, and the zinc plates flowly oxidated. In another experiment, in which a feries of thirty glaffes, containing wires and zinc plates, were ufed, moft of the wires not only gave out gas, but after fome time became covered at their points of contact with the furface of the water with a white film; a few of them, not deeply inferted, produced a flight white precipitation.

5. Thefe facts feemed to fhew that the quantity of hydrogen produced in a feries was in fome meafure, and to a certain point, in the inverfe ratio of the quantity of the furface of the filver plates. Speculating upon them, and comparing them with the experiments of Mr. Cruickfhank, and thofe which I noticed in my laft paper on the figns of ammoniac perceived during the action of a pile in common air, I could not but conjecture that whilft oxygen was condenfed on all the zinc excitors in the feries, hydrogen was produced on all the filver ones; and in fmall furfaces chiefly liberated, whilft on larger ones it was almoft wholly condenfed by the nitrogen of atmofpheric air diffolved in the water, and this conjecture was rendered more probable, when I confidered the white matter chiefly formed round the filver at the furface of the water, and its folubility, without cloudinefs in acids, as it might eafily have been produced by the decompofition of magnefian falts exifting in the pump water.

To determine whether ammoniac was produced, I made many experiments on different feries, confifting of from feventeen to thirty glaffes. In fome of thefe glaffes wires were ufed, and in fome of them plates. Sometimes diftilled water was employed, and fometimes pump water, both of which were occafionally tinged with red cabbage juice.

Without being minute in the detail of thefe experiments, I fhall give their general refults. In the veffels containing red cabbage juice, that fluid, after many hours, became tinged with green where it was in contact with the filver, though at its points of contact with the zinc no change of colour could be obferved in it. In the pump water a white film always formed on the furface of the water near its points of contact with the filver: whilft in diftilled water fuch an appearance was hardly perceptible. The anomaly of its being now and then perceptible I am inclined to refer to accidental impurities in the veffels.

The

The filver flips in pump water almoft always became incrufted with a white matter, which was never notable in diftilled water, and which was foluble, without cloudinefs, in nitrous acid.

In one experiment, when a filver flip, forming part of a powerful feries, was introduced into a fmall veffel, containing folution of muriate of magnefia * (connected with the next zinc glafs by means of mufcular fibre, to prevent the interference of the oxydating metal with the refults), in the courfe of a night much gas was given out from it, and it became incrufted with a white matter, which diffolved with flight effervefcence in marine acid. A precipitation had taken place in the fluid.

Thefe refults afford ftrong probabilities in favour of the production of ammoniac on all the filver excitors of the feries formed with common water : and compared with the facts before mentioned, they amount almoft to demonftrations. Whether the nitrogen of atmofperic air diffolved in water is the agent which forms with the hydrogen ammoniac, future experiments made in vacuo muft determine †.

6. The power of the feries with filver wires, was much lefs than that of the feries with plates. Suppofing the formation of ammoniac, it was probable, that the larger quantity produced upon the plates, might be in fome meafure the caufe of their greater powers; and if fo, it was likely that the condenfation of nafcent hydrogen upon the wires would be connected with increafe of power. From the following facts it appears that this is the cafe. A feries of thirteen plates of zinc, with their filver wires conftructed in glaffes, containing weak folutions of red fulphate of iron, mingled with a little folution of common fulphate of iron and nitrous gas, acted full as powerfully as the common feries of twenty plates. The wires gave out no hydrogen, but occafioned a brown precipitation in the fluid, and the zinc plates foon became covered with green oxide of iron. The phænomenon was the fame with other mixed metallic folutions, capable at the fame time of abforbing hydrogen, and oxydating zinc.

7. The ftrongeft analogies would induce us to believe, that all the galvanic feries compofed of eafily oxydable metals and difficultly oxydable metals muft follow the fame laws in producing changes in their connecting fluid as zinc and filver. But as from the interefting facts of Colonel Haldane, it appeared that iron and zinc as a combination were poffeffed of very confiderable powers, and as iron is but little inferior to zinc in its affinity for oxygen,

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* Muriate of magnefia was ufed in preference to nitrate or fulphate, becaufe there was a poffibility of the decompofition of the acids in thofe falts by the nafcent hydrogen, which alone would occafion a precipitation of earth.

† Mr. Cruickfhank, who firft noticed the probable formation of ammoniac on the filver wire of the pile, has offered fome ingenious arguments to prove that nitrous acid is formed at the zinc wire. Analogy would induce us to conjecture, that if it was formed on one of the oxydating furfaces, it ought to be formed on all. That no change of colour takes place in cabbage juice, in contact with the oxydating zinc plates, may be owing to the great extenfion of their furfaces. It is worth obferving, that the filver oxydates as rapidly in water in vacuo as in the atmofphere, when the pile is in the atmofphere ; or when nitrous or fulphuric acid is ufed in the cells in vacuo.

3

it became a curious queſtion what would be the habitudes of thoſe metals in effecting changes in the water conſtituting them a ſeries? Sixteen zinc plates, and ſixteen pieces of thin poliſhed iron wire, were connected in galvanic order. One half of the ſeries of glaſſes being filled with ſolution of red ſulphate of iron, and the other half with pump water, the end glaſſes gave faint ſhocks when the tongue was introduced into one, and the fingers into the other. When they was connected ſo as to make the ſeries analogous in all its parts, all the iron wires in the common water gave out gas without oxydating, and theſe in the ſolution exhibited the ſame appearances as the ſilver wires in II. 6.

8. A ſeries of glaſſes compoſed of zinc plates and ſilver wires in galvanic order with pump water, was ſuffered to remain for ſome hours without being connected at the extremities, ſo as to compleat the circuit. At the end of this time ſome globules of gas appeared on ſome of the zinc plates, which were a little tarniſhed; no gas was produced on the ſilver wires, and they had undergone no apparent change.

Having ſet up a powerful ſeries of 27 glaſſes, (ſome with red ſulphate of iron, and ſome water) in which zinc and ſilver wire were the excitors, I found that whenever I ſupplied the place of a pair of plates, either by a ſingle metallic wire, or a chain compoſed of different metals, whatever were their habitudes of oxydation, hydrogen was always produced at the place of the ſilver, and oxygen always fixed or extricated at the place of the zinc. When many ſilver wires were introduced into the ſeries in new glaſſes, ſo as to preſerve the original number of exciting plates, the powers of it ſeemed to be very little diminiſhed, and gas was given out, and oxygen fixed in every new glaſs. When the points of contact of ſome of the plates and wires above the water were covered with cement, the phænomena were the ſame as if they had been expoſed to the atmoſphere. When one pair of a ſeries was in vacuo, the gage being at $\frac{6}{10}$, the powers of the whole were not ſenſibly diminiſhed, and gas was extricated from the ſilver wire.

9. On theſe facts I ſhall not preſume to ſpeculate. There is every reaſon to believe that a number of new experiments muſt be made, before we ſhall be able to diſcover the laws in conſequence of which one quantity of chemical action generates in the galvanic ſeries of Volta, an influence capable of increaſing all analogous actions, and of generating new ſimilar actions. Many new obſervations muſt be collected, probably before we ſhall be able to aſcertain whether water is decompoſed in galvanic proceſſes. Suppoſing its decompoſition, we muſt aſſume, that at leaſt one of its elements is capable of rapidly paſſing in an inviſible form through metallic ſubſtances, or through water and many connected organic bodies; and ſuch an aſſumption is incommenſurable with all known facts. But a ſhort period is elapſed ſince philoſophers beheld with wonder, ſolid and fluid ſubſtances aſſuming new modes of exiſtence in different gaſes. Do not the new phænomena of galvaniſm authoriſe us to hope that at no very diſtant time they will behold even thoſe gaſes undergoing novel changes, and exiſting in new and now unknown forms?

III. *Remarks*

III.

Remarks on the Powers of different Galvanic Combinations.

1. I have found by many experiments, that when muriatic acid is introduced (in quantities fufficient to produce vifible changes) into water contained in the glaffes of an effective galvanic feries compofed of zinc plates and filver wires, the zinc plates are acted upon, and gas is given out from every part of their furfaces; whilft the quantities of gas produced on the filver wires are increafed only in the apparent ratio of the increafed power of the feries to give the fhock. When muriatic acid is introduced into a certain number only of the glaffes of a feries, fimilar changes are produced in thofe glaffes; but there is no apparent alteration in the natvre of the phænomena taking place in the other glaffes; thefe phænomena are only rendered more vivid. Effects analogous in *appearance* take place when fulphuric acid, and even when nitric acid is employed. So that it appears that the power of a feries to fix oxygen upon its zinc plates, and to evolve hydrogen from its filver plates, is limited, and cannot be increafed by oxydating bodies beyond a certain extent. We muft confequently conclude, that only a certain quantity of galvanic influence can be made to circulate through a feries in a given time, and that the increafe of oxidation beyond a certain term is connected with no new increafe of power.

2. The fubftances which are capable of rapidly oxydating the imperfect metals, and of condenfing nafcent hydrogen at the fame time, are thofe which produce the moft powerful effects, when made the media of connection between the metals in the galvanic feries. The nitric and oxygenated marine acids appear to be the moft powerful of the known fluid excitors of the pile. The folutions of metallic falts, compofed of acids and oxydes at their maximum of oxidation, ftand next in order: then follow the muriatic and fulphuric acids, and the neutral falts containing thofe acids, or nitric acid.

I have lately endeavoured to make fome comparifons between the powers of piles conftructed with nitric acid, and thofe of common piles; but without much fuccefs. The rapid action of the acid, the evolution of gafes connecting the plates by moifture, and the production of heat, all tended to difturb the refults. The fmalleft feries of plates from which I was able to obtain fhocks by nitric acid, was compofed of three pairs. Six pairs, with moderately ftrong nitric acid, gave a fhock more acute than that produced by a common pile of twenty-feven pairs, but apparently much more limited in extent: it was felt no further than the upper joints of the fingers. With twelve pairs, or ftill more numerous combinations, the fhock was always more acute and painful, than from a common pile compofed of four or five times the number of plates; but apparently more local, and felt over a fmaller furface. As fufion of the cement always took place when the trough was ufed in thefe experiments, cloths wetted with the acid were generally employed; but in no cafe was it poffible to prevent the edges of the plates from being moiftened, fo that a certain degree of communication between the ends always exifted, and from this communication the effects muft have been much diminifhed. A pile with nitric acid had its power

very little leffened by momentary immerfion in water. Hence the increafe of effect cannot much depend on the increafed temperature of the plates.

3. Copper and zinc act very powerfully with nitric acid. Iron and zinc feem to act nearly as intenfely with muriatic acid as with nitric acid.

The galvanic combinations that I have been lately moft in the habits of ufing, are feries of glaffes conftructed with zinc plates, filver or iron wire, and folutions of red fulphate or muriate of iron. Thefe combinations act for a long time intenfely ; fix or eight glaffes being capable of flowly decompofing water ; and if their action is at any time diminifhed from the depofition of oxyde of iron on the zinc plates, it may be eafily reftored by the addition of a little acid to the folution.

4. I noticed in a former paper the conducting powers of charcoal, when made part of the galvanic circuit *, I lately fet up a feries of eight glaffes, with fmall pieces of well burnt charcoal, zinc, and folution of red fulphate of iron ; the charcoal and zinc being connected by filver wire. This feries gave fenfible fhocks, and rapidly evolved gafes from water ; whilft an equal feries with filver and zinc produced much weaker effects. Hence it would appear that charcoal and zinc are equal, if not fuperior, to any metallic combinations.

*** In my laft paper two errors occur, which require to be corrected. Page 340, line 18, for $\frac{6}{40}$ read $\frac{6}{10}$; and page 341, line 11, for *proportional* read *in great meafure proportional.*

III.

On the Quotients arifing from the Divifion of an Unit by prime Numbers. By H. GOODWYN, *Efq.*

TO MR. NICHOLSON.

S I R,

THE following account of the quotients arifing from the divifion of an unit, &c. by prime numbers, being, I believe, perfectly new, and promifing to be very ufeful, is very much at your fervice ; and if you think it worthy a place in your Journal, it may induce the publication of a fmall table prepared for a farther elucidation of the fubject.

I am, SIR,

Refpectfully your's,

Eaft Smithfield, Oct. 1800.   H. GOODWYN.

* Dr. Wells, in an excellent paper on galvanifm, in the Phil. Tranf. has mentioned the great powers of an arc compofed of charcoal and zinc, in exciting the limbs of frogs.

The

The quotient of an unit, divided by the prime number 17·, will confift of 16 places of figures, forming a complete circulating decimal. If the numbers, 2, 3, 4, &c. to 16, be divided by the fame prime number, each refpective quotient will ftill confift of 16 places of circulating decimals. Thus far the property of like divifions has been afcertained by various writers on decimal arithmetic, &c.

But at leaft one very curious, concife, and ufeful property attached to fimilar divifions in general, yet remains to be unfolded. It is this—that the quotient arifing from the firft divifion, virtually reprefents the quotient of every other divifion above-mentioned.

FIRST QUOTIENT.

	Dividends.	Divifors.																
For	1·	÷	17·	=	·0	5	8	8	2	3	5	2	9	4	1	1	7	6	4	7·
	2·	÷	17	=	{ 0	5	8	8	2	3	5	2	9	4	1	1	7	6	4	7
	3	÷	17	=	{ 0	5	8	8	2	3	5	2	9	4	1	1	7	6	4	7
	4	÷	17	=	{ 0	5	8	8	2	3	5	2	9	4	1	1	7	6	4	7

And in like manner the quotient arifing from the divifion of an unit by every other whole-number, lefs than the divifor, will commence with a different figure in the firft quotient, and will circulate to that figure again.

And thus the complete quotient arifing from the divifion of each whole number, lefs than the divifor, in that divifion may be expreffed on the firft quotient, by placing the refpective dividends over their firft quotient figure.

Thus	1	10	15	14	4	6	9	5	16	7	2	3	13	11	8	12	dividends.
	·0	5	8	8	2	3	5	2	9	4	1	1	7	6	4	7	quotients.

This difpofition of the dividend and firft quotient enables us to find, by infpection, the complete decimal quotient or expreffion for a vulgar fraction, whofe numerator or dividend is any given whole number, between 1 and 17, and whofe denominator or divifor is 17·. The quotient of $\frac{2}{17}$, $\frac{3}{17}$, and $\frac{4}{17}$, is feen in the firft elucidating arrangement above, and perfectly coincides with this laft. By this, if I want the complete decimal quotient of $\frac{16}{17}$, I have only to fearch for the number 16 in the line of dividends, and under it is the firft figure of the circulating decimal that will comprize complete quotient of $\frac{16}{17}$, viz. ·9411764705882352, and the fame of the other dividends.

But what authorifes the above property to be termed curious, concife, and ufeful, is, that it does not attach to the prime number 17· only, but under certain laws is equally applicable to all prime and multiples of prime numbers whatever.

3 F 2

Conftruction

IV.

Construction of a Wheel adapted to express by its Rotation the unequal angular Motion of the Planets. By M. ROEMER.

To Mr. NICHOLSON.

SIR,

As you have profeffed it to be part of your plan to infert all difcoveries in your Journal without regard to date, provided they poffefs fufficient merit, and are not fufficiently known in this country, I take the liberty of recommending to your notice an invention of fingular ingenuity, upwards of a century old, but fo little known, that the (in my opinion inferior, though happy) contrivance of Defaguliers was offered to the public forty years afterwards, and ftill continues in ufe.

I am, SIR,

Your's, &c.

R. B.

If it be defired * to move a wheel of 24 teeth by a pinion of fix, in fuch a manner that in certain parts of its revolutions it fhall move fo fwiftly as if it had but 12 teeth, and in other parts as flowly as if it had 48 teeth, the method of accomplifhing this is as follows :

1. Defcribe the right angled parallelogram L M N O, Fig. 1, Plate XVII. having its fide N O equal to the diameters of the great wheel and the pinion taken together, and its breadth L N equal to their thicknefs, which laft muft be greater, the more confiderable the inequality of the intended movement.

Divide N O in Q, in fuch a manner that Q O may be to Q N as 6 to 48, that is to fay, reciprocally as the velocity of the pinion to the greateft velocity of the wheel.

Divide alfo L M in P, in the proportion of 6 to 12, or reciprocally as the velocity of the pinion to the fmalleft velocity of the wheel. Then draw P Q, and as many lines S R, parallel to L M, as there are teeth intended in the great wheel, upon which write the degree of velocity they exprefs, which are in the inverted ratio of their lengths.

2. Let two truncated cones be made in the lathe; one equal to what might be formed by the revolution of the trapezium L P Q M round L N as an axis, and the other equal to what might be formed by the revolution of the trapezium P Q M O round the axis M O.

* In what follows I tranflate and take the figure from the *Machines et Inventions approuvées par l'Academie Royale des Sciences* for 1699, page 89.---B.

On the largeft of thefe two cones let the circles generated by the revolution of the points P, T, Q, be marked and diftinguifhed by the fame numerical figures as the correfponding parallels of the parallelogram L O.

Defcribe upon the two bafes of the conical fruftum radial lines, which fhall make angles at the centre C, Fig. 3, in the fame proportion to each other as the intended velocities of the wheel, as exprefled in Fig. 2, and let teeth be cut in the furface of the cone according with thefe lines; after which look on the circles which exprefs the different velocities, and have been traced on the fame furface, to find what part of each tooth ought to remain oppofite its correfponding radius, and cut or file away the reft. The teeth will thus lie in an elliptical or inclined curve on the conical face, which in the figure is marked by a darker fhade.

The pinion muft be made of a regular conical form, as is fhewn at M O, in Fig. 3.

By this contrivance the largeft or wideft teeth will always meet the largeft part of the pinion, and the narroweft will correfpond with the fmalleft part; by which means, though the pinion has an uniform motion, the wheel will be carried unequably, according to the required law [*].

V.

On the Solutions and Precipitates of Mercury. By Cit. BERTHOLLET [†].

WHITE fulphate of mercury, flightly oxided, as defcribed by Citizen Fourcroy in the laft volume of the Academy of Sciences, is obtained with more eafe, and in a purer ftate, by boiling very diluted fulphuric acid upon mercury. When the liquid undergoes a ftrong heat, it becomes oxigenated fulphate of mercury. When there is not too great an excefs of acid, it is in part decompofed by the water, which beomes acid by feizing a portion of the acid. It, however, contains lefs acid than the mild fulphate. The proportions between the precipitated oxided fulphate, and the acid fulphate fufpended in the water, are different, according to the temperatures, the quantity of water, &c.

When the fulphate is decompofed by an alkali, the precipitated oxide always retains a fmall quantity of acid.

Nitric acid has the fame habitudes with the oxides of mercury. Citizen Gay has remarked, that when mercury is diffolved by heat in nitric acid, there is firft a difengage-

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[*] The contrivance of *Defaguliers*, defcribed in his *Courfe of Experimental Philofophy*, I. 464, confifts of two elliptical wheels, Fig. 4 and 5, connected by tooth or by catgut (which laft is a very bad way.) They revolve on their foci, and while the driving ellipfis moves uniformly, the radius vector of the other has the required motion.---R. B.

[†] Societé Philomath. No. 41.

ment

ment of nitrous gas; after which solution then takes place quietly, the oxigenated nitrate, which was first formed, being decomposed by the portion of mercury which remains. The nitric solution of mercury made by heat, cannot keep all the oxide of mercury in solution, except by means of an excefs of acid.

Muriate of foda precipitates the nitric folutions of mercury, and affords muriates which differ according to the degrees of oxigenation of the mercury in its folutions.

Though the oxides of mercury, when too much oxigenated, cannot remain combined with the fulphuric and nitric acids, it is not the cafe with refpect to the muriatic acid which, not being faturated with oxygen like the two former diffolves mercury, and the other metals at every degree of oxidation. Thus mercury combined with that acid in the oxigenated muriate, is much more oxided than in the moft highly oxided of its nitric combinations.

Bayen has proved, that moft of the precipitates of mercury contain a fmall quantity of acid. Citizen Berthollet has added new experiments to thofe already made by this chemift.

When a muriatic folution of mercury is precipitated by carbonate of foda, an examination of the precipitate, and of the fupernatant fluid, proves that the latter contains the alkali with a great part of the carbonic acid, muriatic acid, and a fmall quantity of oxide of mercury. The precipitate is formed of oxide of mercury, muriate of mercury, and carbonate of mercury. When carbonate of pot-afh is ufed, the precipitate feizes all the carbonic acid, and a greater portion of the muriatic acid; fo that it fublimes almoft totally in mercurial muriate. The precipitate contains lefs muriatic acid, if the carbonate of pot-afh which is ufed contains pure pot-afh.

Bayen had alfo remarked, that certain precipitates of mercury poffeffed the property of detonating when mixed with fulphur, but he has not explained this phænomenon, and the circumftances under which it prefented itfelf. Citizen Berthollet has fhewn, that fulphur in contact with the oxides of mercury, fuddenly takes from them the flightly adhering oxygen, when thefe precipitates contain only a fmall quantity of muriatic acid; but this effect cannot take place when the oxide of mercury is defended from the action of the fulphur by too large a quantity of muriatic acid.

VI.

A Chemical Analysis of Three Species of Whinstone, and Two of Lava. By Robert Kennedy, *M. D. F. R. S. & F. A. S. Edin.* *

ON the 5th of August (1798) I announced to the Society that I had discovered soda in several varieties of the whinstone † of Scotland, and also in lava from Mount Ætna; but did not describe the various experiments to which these substances had been subjected in my examination of them. In the following paper, therefore, I have the honour of laying an account of these experiments before the Society.

ANALYSIS I.

Basalt of Staffa.

The specimen of this basalt, submitted to the following analysis, was given me by a gentleman, who brought it himself from the celebrated basaltic columns in Staffa. A description of its external mineralogical characters may be found in Sir James Hall's paper, (p. 8. and 56 of our present Volume) to which I beg leave to refer.

This basalt, though reduced to fine powder, does not effervesce with acids. The colour of the powder is greyish, and when wet greenish. By being exposed to a low red heat, the colour of the stone is changed to brown. It is not attracted by the magnet, either in its natural state, or after being heated red hot.

Its specific gravity, taken in distilled water at the temperature of 60° of Fahrenheit, I found to be 2.872.

Some small pieces being exposed to a low red heat for half an hour, lost 5 *per cent.* in weight; and when the stone was reduced to powder, and heated red hot, the loss was the same. I also examined the effects of high heat on it, in the following manner: having made some small crucibles of the porcelain clay of Cornwall, which I used on account of its great purity and infusibility, I baked them in pretty strong fires, generally above 100 of Wedgwood. As soon as they were cold, they were each exactly weighed. A portion of the basalt in fragments, also weighed, being put into one of these small crucibles, and a pyrometer into another of the same size, both were placed in a Hessian crucible. A small flat cover, also made of the porcelain clay, was laid upon each; and then a lid was carefully luted on the Hessian crucible with clay and sand. The apparatus thus prepared was next set into a furnace; and the fire being raised gradually till it appeared to have attained the pitch desired, it was kept as equal as possible for about an hour. The small crucible,

* From the Edinburgh Transactions, 1799.

† The name whinstone is used throughout this paper in a generic sense, comprehending basalt, trap, certain kinds of porphyry, wacken, and some other stones of the argillaceous class.

and

and the melted basalt it contained, being weighed as soon as cold, it was easy to determine how much weight was lost.

In this manner some of the basalt was exposed to a heat of 72 of Wedgwood, at which it was vitrified, and lost exactly the same weight as in a low red heat. At 160 the effects were in every respect the same; the loss not being greater in that intense fire. The small crucibles, in which the pyrometers had been placed, did not in these experiments lose the smallest weight.

The volatile matter thus driven off by heat is partly water, as the following experiment shows : I put half a pound troy of the basalt in fragments, into a small Wedgwood retort, and luted to it a receiver, into an aperture of which was fitted one end of a glass tube, the other end being adapted to a pneumatic apparatus. The retort was then heated slowly to redness, and kept moderately red hot for two hours. In the receiver some water was condensed. Some gas also passed over; but I could not ascertain with precision either its quantity or its nature, as it was mixed with the air of the receiver. I have not made farther experiments on the volatile matter contained in whins ; but it deserves to be examined with attention.

This basalt being exposed to heat in a muffle, was found to soften at 38 of Wedgwood*.

Some of it being reduced to fine powder, was boiled in thirty times its weight of water for half an hour. After filtration the water was examined with different chemical tests, but gave no precipitate with any, except a slight cloud with nitrate of silver ; and a portion being evaporated to dryness, left only some thin streaks on the bottom of the glass.

Having premised these particulars, I proceed to describe the analysis :

1. One hundred grains of the basalt, reduced to fine powder in a Wedgwood mortar, were mixed, in a small retort, with about 1200 grains of muriatic acid; and a receiver being adapted, the mixture was gradually heated till it boiled. It was at first of a brownish-yellow colour, but afterwards became brownish. Part of the powder was dissolved. To distill off the uncombined acid, the heat was continued till the mixture began to grow thick. It was then diluted with boiling distilled water, and poured on a filter; and the undissolved part, after proper edulcoration, being dried and heated red hot a few minutes, weighed 67 grains, and was greyish-white.

2. The filtered solution was of a faint yellowish-brown colour. Being saturated with caustic ammonia, a bulky precipitate was thrown down, which was carefully separated by filtration. It had at first a dirty greenish-colour, which was afterwards changed to brown by the action of the air.

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* The fusibility of this, and the other substances to be afterwards mentioned, I examined with Sir James Hall. For this purpose, a small piece of each was placed, with a pyrometer as near to it as possible, in an open muffle previously heated to redness. It could thus be seen perfectly during the operation, and the fire being raised, as soon as it was found to be soft, when pressed slightly by an iron rod, the degree of heat was ascertained by measuring the pyrometer.

† The mortar I used was not scratched by any of the whins or lavas mentioned in this paper.

3. The

3. The solution, after being freed from this precipitate, was perfectly colourless and transparent. I dropt into it a small quantity of sulphuric acid, which produced no cloud; consequently the solution contained neither barytes nor strontian. It was then evaporated to a small quantity, and treated with carbonate of ammonia. Some white earth was thrown down, apparently carbonate of lime, which after being washed, dried, and heated red hot a few minutes, weighed $6\frac{1}{2}$ grains.

4. The insoluble residuum, No. 1. which weighed $67\frac{1}{2}$ grains, I mixed in a silver crucible with a solution of caustic pot-ash, containing as much alkali as was equal to twice the weight of the residuum. This mixture, being evaporated to dryness, was exposed for one hour to a red heat, in which it melted. When cold, the mass was green. After being softened, and washed out of the crucible with boiling distilled water, it was supersaturated with muriatic acid, by which the greater part was dissolved. This mixture, being then evaporated to a small quantity, became gelatinous. It was next diluted with water, digested, and filtered. Some silex remained on the filter, which, after proper washing being dried, and heated red hot a quarter of an hour, weighed 43 grains, and was perfectly white. To learn whether this silex was free from every other earth, I mixed a part of it with four parts of carbonate of soda, and melted the mixture in a silver crucible. Water, being poured on the melted mass, dissolved it entirely into a liquor silicum, which was diluted largely, and saturated exactly with an acid. No precipitate appeared, even after six or eight days; therefore these 43 grains were pure silex.

5. The solution, No. 4. (from which the silex had been separated), was of a light greenish colour. Caustic ammonia, when poured into it, threw down a brownish precipitate. Having carefully separated this precipitate, and washed it on a filter, I dropt into the remaining solution, which was now colourless and transparent, a small quantity of sulphuric acid, in order to detect barytes or strontian. No precipitate was formed. The solution was then evaporated to a small quantity, and treated with carbonate of ammonia, by which a second portion of carbonate of lime was obtained. Its weight, after being heated red hot, was $9\frac{1}{2}$ grains.

6. The brownish precipitates, thrown down from the solutions No. 2. and 5. by caustic ammonia, had the appearance of argil mixed with iron. To separate the argil, these precipitates were mixed together, and boiled, while still moist, in a solution of caustic pot-ash, in a silver crucible. A part was dissolved; but a spongy matter remained, of a darker brown colour than at first, which was collected on a filter.

7. Into the caustic-alkaline solution I poured sulphuric acid, till slightly in excess, and neutralised it again by carbonate of soda. The argil was precipitated; which being sufficiently washed, was re-dissolved in diluted sulphuric acid. This solution was then mixed with some acetite of pot-ash, and gave, by successive evaporations, small regular crystals of alum. At last it became gelatinous; and being evaporated to dryness, and diluted again with water, one grain of silex was left. The remaining solution produced, to the last drop,

cryftals of alum *. I diffolved thefe cryftals in water, and precipitated the argil by carbo nate of ammonia. After being carefully wafhed, dried, and heated red hot a quarter of an hour, it weighed 12 grains.

8. The brownifh matter, No. 6. infoluble in cauftic pot-afh, feemed to be oxyde of iron; and after having been heated red hot, weighed $24\frac{1}{4}$ grains. I powdered this mafs, and poured on it fome acetous acid, in order to detect magnefia; but nothing was diffolved. It was next treated with nitric acid, which diffolved the iron, but left 4 grains of filex. The iron being precipitated, dried, and heated red hot, weighed 20 grains, and was magnetic. Sufpecting that fome argil might ftill be mixed with it, from having efcaped the action of the cauftic pot-afh, I diffolved 5 grains in muriatic acid, and precipitated the iron by Pruffian alkali. Having feparated the blue precipitate, I boiled the folution with carbonate of foda, and obtained 1 grain of argil. Thefe 20 grains confifted, therefore, of 16 grains of iron, and 4 grains of argil.

The remaining part of the iron was melted for an hour, with ten times its weight of nitre, in order to detect manganefe. The mixture, however, when cold, was not greenifh; and water made a colourlefs folution of the faline matter, which did not become turbid, when expofed fome days to the action of the air †.

9. The two portions of white earth above mentioned, which feemed to be carbonate of lime, weighed together 16 grains. To feparate magnefia, if any were mixed with this earth, I put it into a little water, and added fulphuric acid till flightly in excefs. Sulphate of lime was produced. Having poured fome alcohol into this mixture, I filtered it, and wafhed the fulphate of lime with more alcohol diluted with water. The filtered liquor was then boiled with carbonate of foda, but no magnefia was precipitated.

Thefe 16 grains were therefore carbonate of lime, of which, according to Mr. Klaproth's calculation, about 9 grains were pure lime.

One hundred parts of the bafalt of Staffa contain, according to the above analyfis:

Silex,	-	(No. 4. 7. and 8.).	48
Argil,	-	(No. 7. and 8.).	16
Oxyde of iron,	-	(No. 8.).	16
Lime,	-	(No. 9.).	9
Moifture, and other vol. matter,			5
			——
			94

The fum is 94 parts; confequently there is a lofs of *6 per cent.*

* It therefore contained none of the earth which Vauquelin lately difcovered, and to which he has given the name of glucine.

† I think, however, it is probable, that this bafalt contained a fmall quantity of manganefe, both from the brownifh colour of the folution, No. 1. and from the green colour which the undiffolved refiduum gave, by fufion, with cauftic pot-afh.

About

About a year ago I analyzed specimens of some of the whins in the neighbourhood of Edinburgh, and found, that the sum of the earths and iron, separated by the analyses, never amounted to more than 93 or 94 *per cent.;* so that the loss was always equal to that just mentioned. It was this circumstance which first led me to suspect that some saline substance existed in these stones; and their considerable fusibility favoured the suspicion.

Soon after these analyses were made, I observed another circumstance, which amounted to an absolute proof of the whins containing something of a saline nature, in combination with their earthy bases. Most of the artificial crystallites, made by Sir James Hall, which I had always an opportunity of examining, threw out on their surfaces, two or three weeks after their formation, a white efflorescence, which had a very salt taste. It was in too small a quantity to be collected and examined; but when washed off, it was often formed a second time.

I was thus convinced of the existence of some saline substance in these bodies, and made different experiments with several of them, in order to separate it, and ascertain its nature; and soon found that it was soda.

I shall next describe some of the methods by which this alkali was most easily separated from the earthy parts of the whins.

Experiments to obtain the Soda, and determine its Quantity.

Having broken some of the basalt of Staffa to small fragments, I weighed 400 grains, and ground the whole with water to an extremely fine powder, in a Wedgwood mortar. The powder, and the water with which it had been ground, were then put into a small retort, and mixed with about 1200 grains of sulphuric acid, which I had carefully distilled for this operation. I placed the retort in the sand bath of a small furnace which I use for analyses, adapted a receiver, raised the fire till the acid began to distil slowly, and carried on the distillation to dryness. Water was then poured into the retort, and boiled, the mixture thrown on a filter, and the undissolved residuum sufficiently washed. This residuum was next treated a second time with a fresh portion of sulphuric acid; and afterwards boiled with water, filtered and washed, exactly in the same manner as before. The undissolved part of the stone was now almost white.

The filtered solutions being mixed together, were evaporated to dryness; and the saline mass which remained was heated red hot for one hour in a clean and new Hessian crucible. When cold the mass was of a brick red colour. Having powdered it well, I boiled it in water, poured the whole on a filter, and washed the reddish matter carefully. This filtered liquor, in which all the soda, separated from the basalt by the sulphuric acid, was dissolved in the state of sulphate of soda, could contain only a small quantity of earthy matter; for the greater part of the sulphate of argil, and of iron, formed by the first part of the process, must have been decomposed by the red heat, to which the mass was afterwards exposed. Accordingly, the solution being treated with carbonate of ammonia, only a small quantity of a precipitate was thrown down, which was carefully separated. The

3 G 2

solution

folution was then evaporated to drynefs. A faline mafs remained, confifting, in part, of fulphate of ammonia; to feparate which the whole was expofed to heat in a fmall crucible, and when it ceafed to emit fumes, the heat was increafed to rednefs. A fixed white falt was left, which weighed 25 grains.

This falt I re-diffolved in water, added fome carbonate of ammonia, and heated the mixture till it boiled. A fmall quantity of an earthy precipitate was again thrown down, which being feparated by filtration, the folution was evaporated to drynefs, and the falt which remained heated red hot a fecond time. By thefe fucceffive operations, all the earthy matter, at firft diffolved, was feparated. The falt now weighed 23 grains, and had all the properties of fulphate of foda. Thefe properties were the following:

1. It was not volatile in a moderate red heat.

2. After being thus dried, it diffolved readily in about fix times its weight of water, at the temperature of 60 of Fahrenheit.

3. This folution gave, by evaporation, cryftals exactly the fame in form as artificial ful-phate of foda; and thefe cryftals efflorefced in dry air.

4. A part of the folution of this falt being boiled with carbonate of foda, gave no preci-pitate; a proof that it contained no earthy matter.

5. Some of the folution being mixed with a very ftrong folution of acid of tartar, re-mained unaffected; the falt therefore contained no pot-afh.

6. Some of the falt being diffolved in water, was decompofed by nitrate of barytes; and the fulphate of barytes produced was feparated by filtration. The nitric acid, thus united to the alkaline bafis, formed a faline compound, which, in the next place, was mixed and deflagrated with charcoal. By wafhing the coaly refiduum, and evaporating the water, I obtained pure carbonate of foda, which efflorefced readily in the air.

There can be no fufpicion of the retort which was ufed furnifhing any part of the alkali; for I weighed it previoufly in a balance of great accuracy; and after the operation was finifhed, found its weight exactly the fame as at firft, and the luftre of the glafs altogether unimpaired.

The whin which I next fubmitted to examination, for the purpofe of feparating the foda, was taken from a quarry near the Water of Leith *. I ufed a confiderable quantity, 800 grains; which were diftilled twice with fulphuric acid, and then treated in every re-fpect exactly as the preceding. The fulphate of foda obtained, amounted to 43 grains.

I afterwards fubjected fome other whinftones to the fame kind of proceffes, and in each fpecies found foda. The nitric and muriatic acids alfo diffolve a certain quantity of the alkali contained in thefe fubftances; but their action is weaker than that of the fulphuric acid.

* This fpecies is the firft mentioned in Sir James Hall's paper. When powdered, it effervefced flightly with acids. I did not analyze it; but, in the courfe of the procefs for detecting foda, one of the earthy precipitates proved, upon examination, to be magnefia. It is the only whin in which I have found this earth.

4   By

By the experiments now described, there were separated from 100 parts of each of the whins, between five and six parts of sulphate of soda, which may be equal to two or three parts of pure soda. But as these two or three parts, when added to the sum of the earths and iron, did not account for the loss of 6 or 7 *per cent.* always observed in my analyses, I was satisfied that the whole of the alkali was not obtained by the processes which were followed; even although, in that with the whin from the Water of Leith, it had been exposed, in very fine powder, to the action of the sulphuric acid, at a boiling heat, for more than eighteen hours. It appeared necessary, therefore, to try other methods; and after some consideration it occurred to me, that if the powdered whins could be exposed, while red hot, to the vapours of the sulphuric acid, also in a red hot state, its power in separating the whole of the alkali from the earthy bases of these substances, would probably be greatly increased in so high a a temperature. I succeeded in applying a red heat both to the powdered stone, and to the acid at the same time, by the following means.

Some of the basalt of Staffa being mixed, in very fine powder, with three parts of sulphuric acid, the mixture was evaporated slowly to dryness in a sand bath. The dry mass was then heated gradually to redness, and kept in the fire for one hour. It was next powdered, and boiled in water; and the water being filtered, was treated with carbonate of ammonia, which threw down a small quantity of a brownish precipitate. After separating this precipitate by filtration, the liquor was evaporated to dryness, and the sulphate of soda, which was left, was purified in the manner already described, and heated red hot. It amounted to 9 parts for every 100 parts of the basalt employed.

In this experiment, the sulphuric acid was first united to a part of the lime, of the argil, and of the iron, contained in the stone; and afterwards, when the mass was exposed to a red heat, the acid was driven off partly or wholly from these, and applied in red hot vapours to every part of the powder; by which its action appears to have been rendered much more powerful, as 9 *per cent.* of sulphate of soda was produced: and by the same process, so simple and easy to execute, I got from the rest of the substances, to be mentioned in this paper, from 8 to 11 *per cent.* of sulphate of soda, although, when they were merely boiled in the acid, the quantity of this salt never exceeded 5 or 6 *per cent.*

As the proportion of acid and alkali in neutral salts has not been hitherto determined with certainty, the quantity of soda in these whins cannot be exactly known. But it is probable that 9 parts of sulphate of soda, dried by a red heat, do not contain less than $3\frac{1}{2}$ or 4 parts of pure alkali*; which must therefore be considered as the weight in 100 parts of the basalt of Staffa; and as $3\frac{1}{2}$ or 4 parts of soda, when added to the sum of the earths and iron, amount nearly to the 100 parts of the stone employed in the analysis, this calculation may be reckoned very near the truth. For the same reason I think it likely, that the greater part, or the whole of the soda, was obtained from the basalt by the process which has been last described.

* This is nearly the proportion given by Mr. Kirwan.

It

It is well known among the friends of the late Dr. Hutton, that he made some experiments on zeolite; by which he concluded, that soda entered into the composition of that substance *. He has not mentioned the circumstance in any of his works; but Dr. Black has been accustomed, as he informed me himself, to take notice of it in his lectures on chemistry, for many years. It is my intention to analyze some species of zeolite; and if the results seem of any importance, they shall be laid before the Society.

Among the experiments on the basalt of Staffa, already described, it has been observed, that, when the powder was boiled in water, a slight precipitate was produced in the water by nitrate of silver, thus indicating some traces of muriatic acid. As it appeared of importance to determine how much of this acid the basalt contained, I subjected some of it to examination for that purpose.

Experiments to ascertain the Quantity of Muriatic Acid in the Basalt of Staffa.

One hundred grains of the stone, in fine powder, were mixed in a small retort with some nitric acid; and a receiver being adapted, the mixture was boiled gently, till the greater part of the acid had distilled over. The liquor in the receiver being examined with nitrate of barytes, remained unaffected; but gave a slight cloud with nitrate of silver, which shewed that it contained some muriatic acid.

The mass in the retort being diluted with water, the whole was filtered; and this filtered liquor produced no cloud with nitrate of barytes, but gave, like the former, a slight precipitate with nitrate of silver.

In the next place, the undissolved residuum was mixed with twice its weight of very pure caustic pot-ash, and exposed to a low red heat, for an hour, in a silver crucible. The mass was then diluted with water, supersaturated slightly with nitric acid, and filtered. With this solution nitrate of barytes produced no effect; consequently, these experiments show, that the stone in question does not contain any traces of sulphuric acid. With nitrate of silver, however, the solution gave a white precipitate, more abundant than the two preceding. The different portions of muriate of silver, being collected, and dried on a sand bath, weighed only 4 grains.

As a fourth part of muriate of silver consists of acid, according to the most correct experiments hitherto published, these 4 grains consequently indicate, in 100 parts of this basalt, only about one of muriatic acid. All the whins and lavas, to be mentioned in the remaining part of this paper, were found, by a similar process, to contain about the same quantity.

* In the 26th volume of the *Annales de Chimie*, p. 119. M. Scherer, in a letter to Van Mons, says, that he was informed by Dr. Black, that Dr. Hutton had, long ago, found *pot-ash* in zeolite. In this statement M. Scherer is incorrect; because it was soda, as above-mentioned, which Dr. Hutton obtained from that substance.

According

According to the refults of thefe different proceffes, there are, in 100 parts of the bafalt of Staffa,

Silex,	48
Argil,	16
Oxyde of iron,	16
Lime,	9
Moifture, and other vol. matter,	5
Soda, about	4
Muriatic acid, about	1
	99

I have thus detailed exactly the various experiments performed in analyzing this fpecies. As all the other whins, and the lavas, which follow, were analyzed in the fame manner, and exhibited nearly the fame chemical properties, the refults only, in each example, fhall be mentioned.

ANALYSIS II.

Whin of Salifbury Rock.

The fpecimen employed was chofen from the fouth fide of the cragg, and was perfectly hard and free from decompofition, as the particular fpot from which I broke it had been quarried a fhort time before.

A defcription of its external characters may be found in p. 54 of the 5th vol. of the Tranfactions of the Royal Society of Edinburgh, (or p. 15 of our prefent vol.) Its powder is light greenifh grey; but after being wet, acquires a dirty green colour. When heated to rednefs, it becomes light brown. Though not attracted by the magnet in its natural ftate, it becomes magnetic after being heated red hot. It does not effervefce with acids. When expofed to a low red heat for half an hour, whether in fragments or in powder, it lofes 4 *per cent*. in weight. It foftens at 55 of Wedgwood. Its fpecific gravity is 2.802. After being boiled, in the ftate of fine powder, in ten or twelve parts of muriatic acid, the infoluble refiduum amounted to 65 *per cent*.

I analyzed 200 grains of this whin, and found that it contained, in 100 parts,

Silex,	46
Argil,	19
Oxyde of iron,	17
Lime,	8
Moifture, and other vol. matter,	4
Soda, about	3.5
Muriatic acid, about	1
	98.5

Further

VII.

Further Remarks on the Enquiries of Dr. Herschel respecting Light and Heat. In a Letter from Mr. John Leslie.

To Mr. NICHOLSON.

SIR,

London, Nov. 17, 1800.

MY last Letter was written under the persuasion that your Journal for October contained, at least in substance, the *whole* of Dr. Herschel's late experiments and conjectures. Had I foreseen that the subject would be resumed in a future number, I should certainly have deferred my remarks until the recital was closed. I now, therefore, feel myself reluctantly compelled to reconsider the question, and to state still farther difficulties, shall I say fallacies, which press the sequel. In most cases, the toil of criticism is miserable waste of time; in every case, it is equally painful and inglorious; and in coming forward to attack the solidity of facts and conclusions sanctioned by high authority, I shall probably, with men of a certain class, incur the charge of temerity and presumption. But such prudential considerations I utterly disregard; being convinced that, on occasions like the present, I may promote the cause of genuine science as effectually by detecting errors as by announcing positive discoveries. Fortunately, I need not at this time engage in much elaborate discussion. The objections which I formerly urged subsist in their full force, nor do I find on recollection any material assertion which I should desire to correct.

The additional experiments which claim examination contain little indeed that can be strictly called original. They are employed for the most in ascertaining facts which have been long established, or which are familiar to every person who has the smallest tincture of science. The paper opens with a formality and apparent caution that might lead us to expect a chain of proofs scarcely inferior to mathematical evidence. We are soon forcibly reminded however, that *such* is not always the surest road to truth; and that the minute spirit of subdivision has prevailed most in the dark ages, and in the barbarous departments of literature, when sense and reason were alike buried in the *verbiage* of scholastic syllogisms, definitions, and distinctions. To the mystic number *seven*, the child of judicial astrology, the Doctor bows with reverence. Light not only consists in seven primitive rays, but each ray has seven *properties*; and so likewise corresponding have the "rays of heat." To muster up precisely those seven analogous properties, however, required some degree of management, since one of them is to inform us, that the rays of light and those of heat " are liable to be scattered *on* rough surfaces;" an expression which, if it has any meaning at all, must denote irregular reflection, and therefore, to common apprehension at least, seems comprized under a former head. The parallel so nicely drawn between the visible and invisible rays, changes, in the last article, into a curious contrast, which asserts, in despite of vulgar prejudices, that Light *may not* give Heat, and yet that Heat *may* compose Light.

Of

Of twenty experiments which are related in detail not fewer than sixteen have no direct connection with the question agitated. They refer merely to the heat occasioned by the rays of light from the sun, a candle, a fire, or a red-hot poker, when condensed in the focus of a lens or speculum; or to the heat collected by convergent reflexion from a heated mass or from the vicinity of a luminated substance. In all this, I can discern nothing either new or striking. But the Doctor thinks fit or convenient to substitute the word heat for light; and that easy change operates like a magical charm. Whatever property has been attributed to the rays of light, belongs henceforth exclusively to the "invisible rays of heat," and thus comes directly in support of his darling hypothesis. Such a mode of reasoning, or rather assumption, hardly deserves any serious refutation. But the Doctor seems quite transported with the discovery. In the eleventh experiment, entitled, "On the Refraction of Solar *Heat*," a large Newtonian telescope, with a compound eye-piece, was directed to the sun, and by the concentration of the broad beam of light in the focus, a very considerable heat, as every body knows, was produced. The philosopher stops, as usual, to wonder. "How *artfully*," he exclaims, " in our present instance, was heat sent from one place to " another!" And, borrowing the language of Newmarket, he continues:—" Heat crossing " heat, through many intersecting courses without jostling together, and each parcel arriving " at last safely to its destined place." The grand conclusion is, that " it cannot be doubted " that the rays of heat are subject to the laws of refraction." In the whole of this strange passage, perhaps the only thing which should excite surprise in a sober mind, is the glaring confusion of ideas.

The 7th and 8th experiments, which undertake to " reflect and condense the invisible " solar rays," have nothing remarkable but their title. I formerly showed, I hope in a convincing manner, that those imaginary invisible solar rays were merely the warm portions of air which environ an illuminated body. Of course, this heat may, as in other cases, be collected by reflection. It is not my present design to discuss the nature and propagation of heat. But I cannot forbear mentioning a single argument, which, if I mistake not, is decisive against the adherents of Radiant Heat. No fact is better known than that the rays of light, in traversing an uniform medium, are not in the smallest degree affected by its most violent agitation, but pursue unvaried their rectilineal course. The case is very different with what is termed radiant heat. The experiments on which it rests succeed only in a close room, and at very moderate distances from the source of heat. It is clear, therefore, that the heating matter must flow with such feeble impulse as to experience obstruction in its passage through the air, and suffer much derangement from the accidental motions of that fluid. If this supposed radiant heat darted with a celerity in any degree comparable to that of light, nay even to the velocity of those projectiles with which we are acquainted,— it would be exactly directed and concentrated in open air, and that at distances limited only by the unavoidable imperfection in the figure of the reflecting surface. Hence, the heating matter, whatever it really is, must have a grossness of constitution, and a slowness of progress, commensurate with the denseness and ordinary mobility of our atmosphere.

The 17th, 18th, 19th, and 20th, are the only experiments which require any particular notice, as seeming to countenance Dr. Herschel's paradoxical opinions. In the first, a semicircular piece of pasteboard, covering the half of a large lens, received the coloured prismatic speculum, but permitted the "invisible rays" beyond the margin of the red to pass through the glass. A heat of 45 degrees was caused in the focus. But with what care and nicety the experiment was performed may be judged from the circumstance, that the bulb of the thermometer appeared illumed by a reddish tint. This perplexing occurrence, however, only inflames the love of the marvellous. The Doctor very gravely proceeds to enquire whether *invisible* rays can, by condensation or accumulation, become *visible*. He must entertain indeed a lofty idea of the nature and value of experiment, thus to set it in opposition to what are accounted the dictates of common sense. Had the result proved successful, what a triumph gained over the frailty of human reason ! It would have taught us to listen with humility to the dreams of a Platonist or visionary of the present day, who announces the new and sublime discovery, that the addition of *nothings* makes *something*. But Dr. Herschel's surmise was not confirmed ; and on repeating his experiments with some little more attention, the effect was only 21 degrees, being not the half of what was before produced. This, among other instances, may serve as a sample of the author's accuracy and circumspection. But how did he discover that pasteboard would intercept the *whole* of the incident light ? He confidently regards this cover, applied too on the very surface of the burning glass, as a perfect diaphragm. Yet when an experiment, and the only one in any degree conclusive, is adduced, tending to support an opinion most repugnant to our general ideas, we might reasonably expect that every precaution would be used, and the previous steps at least scrupulously determined. Common writing-paper, I have found, transmits about one-half of the whole incident beam. What was the thickness or the texture of the Doctor's pasteboard, we are not informed. But even granting his experiments to be performed with accuracy, if the pasteboard permitted only the sixth part of the light to pass, this would have been sufficient to produce the alledged effect. And after all, what reliance ought to be placed on observations which are at variance with every known fact, and every established principle ?

The 19th experiment, whose object is " the refraction of invisible culinary heat," absolutely proves nothing. A hot cylinder of iron was placed near 3 inches from a lens above an inch in diameter, and a thermometer at an equal distance behind it in a position corresponding to the secondary focus. The thermometer rose a degree or two as the iron cooled, and diffused its heat among the neighbouring bodies. Nor did it require any vast stretch of ingenuity, still less an actual appeal to experiment, to perceive that, each time a small screen was interposed, the bulb would suffer a certain depression of temperature.

The 20th, and last experiment is intended to confirm the preceding ; with what success will be presently seen. Another thermometer was placed near the former, but constantly exposed to the stream of heat: In 4 or 5 minutes it acquired its maximum rise, amounting to about a degree and a half. The other thermomer which was screened and exposed alter-

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nately, gained an equal increafe of heat; but, for an obvious reafon, required to that effect double the time. The obfervation was next repeated by placing both thermometers in a fituation to be equally affected by the fcreen, the one in the focus of the lens and the other befide it. The thermometers kept pace together in their progrefs, and their fluctuations, as often as the fcreen was interpofed or withdrawn, correfponded with tolerable precifion. The irregular difference of perhaps a quarter of a degree is furely a quantity too fmall to form the ground of any legitimate inference. Yet mark with what confidence Dr. Herfchel pronounces his precipitate conclufion : " All which fo clearly confirm the effect of the re-" fraction of the lens, that it muft now be evident that there are rays iffuing from hot iron, " which, though in a ftate of total invifibility, have a power of occafioning heat, and obey " certain laws of refraction, very nearly the fame with thofe that affect light." It is truely aftonifhing to obferve how the mind, when once occupied by fome favourite idea, recalls it at every ftep, and tortures every flight appearance into an argument for its fupport.

In another paper fince read at the Royal Society, Dr Herfchel purfues his fubject with the fame monotonous prolixity. It chiefly confifts of experiments directed to afcertain the quantities of light tranfmitted through different coloured glaffes. But fuch refearches are of no real utility in a philofophical view, unlefs many other points had been fixed which the Doctor entirely overlooks. The thicknefs of the glafs, its compofition, and the intenfity of its fhade, were of the moft effential confequence. To determine that depth of tranfmitted colour, would have required a comparifon with the triangular cells of Mayer, as improved by Lambert, Achard, and Burja. The variable force of the fun's rays ought alfo to enter into the account, whether as affected by his different altitude above the horizon, or by the condition of our atmofphere, and principally in refpect to humidity. I have obferved a very fenfible difference in the power of folar light in confecutive days at noon, even when the fky was apparently clear. If colours offend the eye by excefs or defect, fome grofs eftimate may be made of their refpective degrees of illumination; but to determine thofe in general and with precifion by ocular contraft, feems altogether an impracticable, if not an abfurd, attempt. In fact, what juft comparifon can obtain, for example, between green and red, which are ftrictly things as heterogeneous as tafte and fmell. To increafe the embaraffment, we cannot always judge of the colour of the tranfmitted light by that of the fubftance through which the rays are fent. Thus a fun-beam, in emerging from pafteboard, ivory, or white enamel, will, according to their thicknefs, exhibit all the tints from yellow to deep red. Yet admitting, in its fulleft extent, the accuracy of Dr. Herfchel's refults, they furnifh no evidence whatever in fupport of his hypothefis, or indeed of any other hypothefis.

There is only a fingle paffage in the concluding paper that requires particular notice. Dr. Herfchel had anticipated one confequence which I urged as an infurmountable objection to his fyftem; namely, that a burning glafs would act moft fiercely at fome diftance behind the bright focus. The Doctor tried this, and, as he afferts, found it to fucceed; though he acknowledges at the fame time, and with great truth, that the experiment was

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but a *coarse* one. After such a confession, your readers will judge what degree of credit it really deserves. Is it at all likely, nay possible, that so many able philosophers, who for above a century back, have employed their talents on the improvement and application of the burning glafs, should have totally overlooked a fact so obvious and so palpable. The Doctor has a remarkable faculty, not peculiar to him however, of slurring over those points which immediately affect his opinions, and of dwelling with minutenefs on what all the world may be presumed to know. Had he uniformly exercifed the same laudable scepticism which he professes at the outfet, he might perchance have doubted whether his precurfors were not fometimes right, and himself mistaken. It would require more than ordinary docility to believe Dr. Herschel's " coarfe" experiment, in oppofition to all the former concurrent testimonies.

I now take leave, I hope for ever, of this controversy. Without queftioning Dr. Herfchel's fidelity, I have shown that his affertions are not only inconfiftent with all our general and beft founded notions, but ftand directly contradicted by actual obfervations made with peculiar advantages; that his experiments were injudicioufly contrived, executed without circumfpection, and liable to a multiplicity of inaccuracies; that his reafonings, how boldly foever advanced, were ftill more defective; that his later experiments are the more vulnerable in proportion to the confident tone which he affumes; that by far the major part of them is totally unconnected with the fubject in difpute, and only twifted to ferve his purpofe by the fophiftical tranfpofition of terms; and that the few which actually apply are of fuch obfcure and ambiguous character as to afford no certain evidence. In fhort, thofe experiments and conjectures, taking their combined impreffion, may for a while gratify vulgar curiofity, but muft foon haften to final oblivion. I fhould be forry if my ftrictures gave offence to Dr. Herfchel. If I have fpoken with freedom, I truft it is in the language which conviction and the love of truth naturally infpire. Undue authority, always depreffing, is capable of producing moft fatal effects, when fuffered to gain poffeffion of the fciences. How long did the afcendancy of Ariftotle, of Des Cartes, and fhall I add in fome few points that of the venerable name of Newton, retard the advancement of real knowledge. I refpect Dr. Herfchel's talents, I admire his aftronomical difcoveries, and I am perfuaded that England, in the decline of her fcience and philofophy, needs the importatio of genius from abroad, and is honoured by becoming his adopted country. If I cannot equally approve of feveral of his late fpeculations, I reflect that men feldom eftimate aright their own powers, feldom know where their real ftrength lies. Adventuring on new fubjects they are not likely all at once to acquire the fkill, precifion, and caution which are generally the fruit of experience and patient application.

But I cannot finifh the letter without addreffing a few words to you, Sir, as editor of a refpectable Journal. I advert to the note with which you have honoured me at page 348. I certainly prefumed that you gave unchanged the ideas, if not the words of your author. In this it appears I was miftaken; and I chearfully retract the expreffion to which it gave rife, and which were merely the fpontaneous effufion of the moment. Dr. Herfchel only

fuppofes,

suppofes " that the cooling caufes *muft* muft have a ftronger effect on the mercury in a fmall bulb ;" but *why* they fhould have fuch effect, is left for others to difcover. Nor do I think, Sir, that you were happy in the attempt to extricate him from the dilemma. I readily grant that currents of air may afcend, which, though warmer than the encircling mafs of atmofphere, are colder than the body itfelf. But I maintain that thofe currents *have already* received their heat, and afcend only *in confequence* of that communication.— The air which touches the hot body receives its full fhare of heat, and, at the fame inftant, its force of afcenfion : after it has begun to mount, the effect has ceafed ; nor can it exert its cooling energy unlefs it again defcends, to renew the procefs*. To argue otherwife would require the affiftance of a fort of aitiology like that with which fome chemical writers have amufed us of late years concerning pre-difpofing or pre-exiftent affinities. But I will not dwell on a difpute of trivial moment. The author of a periodical work, who is obliged, frequently perhaps without premeditation, to fatisfy the urgent calls of his compofitor, is entitled to much indulgence. I moft readily excufe the paffage which was criticifed, and which might inadvertently flip from your pen ; and by printing the above, you will give to the public one proof more of your impartiality and candour.

<div align="center">

I am, S I R,

Your moft obedient Servant,

JOHN LESLIE.

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[*Erratum*—in laft Number, page 346, line 9, before *vifual* read *fame*.]

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VIII.

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Experiments and Obfervations on the Light which is fpontaneoufly emitted, with fome Degree of Permanency, from various Bodies. By NATHANIEL HULME, M. D. F. R. S. and A. S.†

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INTRODUCTION.

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THE difcoveries which have been made with refpect to light, as it proceeds immediately from the fun, are many and important ; but the obfervations on that fpecies of light which is fpontaneoufly emitted from various bodies, are not only few in number, but in general very imperfect. The author is therefore defirous of drawing the future attention

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* The air, which is firft heated, afcends upon the fame principle as other floating bodies ; namely, becaufe its fpecific gravity is diminifhed, and the upward preffure of the fluid beneath, is, therefore, greater than that of the fuper-incumbent column added to that of the heated mafs. The lower part of the whole afcending current (which Mr. L. feems to overlook) is as cold as the reft of the atmofphere ; and it is obvioufly this portion which maintains the cooling procefs, by ftriking the inferior furface of the thermometer in its afcent.---N.

† Philof. Tranf. 1790, p. 161.

of the philofopher more particularly to this fubject, and of communicating his own experiments and obfervations upon it, to this learned Society.

By the fpontaneous emiffion of this light, the author wifhes to diftinguifh it from all kinds of artificial phofphorus; which, as he apprehends, differ effentially, in fome of their properties, from that light of which he means to treat. And, by its adhefion to bodies with fome degree of permanency, he diftinguifhes it from that tranfient fort of light which is obfervable in electricity, in meteors, and in other lucid emanations. The light which is the fubject of this paper, he fhall therefore beg leave to difcriminate by the name of *fpontaneous light*.

The fubftances from which fuch light is emitted, are principally the following.

Marine animals, both in a living ftate, and when deprived of life. As inftances of the firft may be mentioned, the fhell-fifh called *pholas*, the *medufa phofphorea*, and various other *mollufca*.

When deprived of life, marine fifhes in general feem to abound with this kind of light. The honourable Mr. Boyle commonly obtained light, for his ufe, from the whiting, as appears from many parts of his works: the author of thefe experiments and obfervations procured his fifh light chiefly from the herring and the mackerel.

The flefh of quadrupeds has alfo been obferved to emit light. Inftances of this are mentioned by Fabricius *ab Aquapendente;* by T. Bartholin; by Mr. Boyle; and by Dr. Beale; for which, fee T. Bartholin, *de Luce Animalium*, p. 183; Boyle's Works, Vol. III. p. 304; Phil. Tranf. Vol. XI. p. 599.

In the clafs of infects are many which emit light very copioufly, particularly feveral fpecies of *fulgora* or lantern-fly, and of *lampyris* or glow-worm; alfo the *fcolopendra electrica;* and a fpecies of crab, called *cancer fulgens*.

Rotten wood is well known to emit light fpontaneoufly. Peat earth alfo has the fame property. Of the effects of the latter, a remarkable inftance is related in Plot's Natural Hiftory of Staffordfhire, p. 115.

The place where the following experiments were made, was a dark wine-vault, which, for diftinction's fake, the author calls the *laboratory*. The heat of this laboratory varied, throughout the year, from about 40 degrees of temperature to 64°. The thermometer made ufe of was that of Fahrenheit.

The weight is always to be fuppofed that called troy weight. The liquid meafure employed, was that ufed for wine in this country: the ounce containing 8 drams avoirdupois; and the pint, 16 ounces.

The water ufed in general for the experiments, was pure fpring water, drawn up from under ground by means of a pump; and it was always employed cold, unlefs otherwife expreffed.

SECTION

SECTION I.

The Quantity of Light emitted by putrefcent Animal Subſtances, is not in Proportion to the Degree of Putrefaction in ſuch Subſtances, as is commonly ſuppoſed; but, on the contrary, the greater the Putrefcence, the leſs is the Quantity of Light emitted.

EXPERIMENTS.

Exper. 1. Two very freſh herrings were bought in the morning, and hung up in the laboratory; on examining them in the evening, they were beginning to be luminous.

Exper. 2. Three herrings, which were quite freſh, after being ſcaled and gutted, were hung up by a ſtring in the laboratory. The next evening they were become exceedingly luminous in every part, and much lucid matter had exuded, as it were, upon their ſurface, which was eaſily ſcraped off by the blunt edge of a knife; it alſo adhered to the fingers, or other parts of the body, when touched; but, as they grew more putrefcent, the quantity of light diminiſhed, and at laſt was extinguiſhed.

Exper. 3. A ſingle herring, that was perfectly ſweet, was hung up in the laboratory. On the ſecond night, it was covered with light; on the third, not ſo lucid; on the fourth, leſs ſo; and ſo on, in proportion to the degree of putrefcence.

Exper. 4. Two herrings, ſomewhat ſtale, were hung up in the morning, and at 8 P. M. one of them was pretty luminous, but the other leſs ſo. On the next evening, the former was but ſlightly luminous, and the latter was dark; on the ſucceeding evening, they were both dark.

Exper. 5. Two mackerels were brought from the market at 1 P. M. which, to the ſight and ſmell, were perfectly ſweet and good. Being then carried into the dark laboratory, and examined, the one was found to be a little luminous, and the other pretty much ſo eſpecially about its belly.

Exper. 6. A fine freſh mackerel, with a bright eye, was purchaſed about noon, and placed as uſual in the laboratory, the temperature of which, at that time, was about 54°. At 11 P. M. this beautiful fiſh was luminous about the head and upper parts; and the inſide of the mouth, which was wide open, ſhone with moſt brilliant light. The next evening, the whole body of the fiſh was very luminous: on the third night, it was leſs ſo; and on the fourth the light was nearly extinguiſhed.

Exper. 7. In the forenoon, about ten o'oclock, a couple of fine-looking mackerels were hung up in the laboratory, at the temperature of 56°, and at 10 P. M. they began to ſhine in various parts, the light ſeeming to proceed from within outwards. On the ſecond night, they put on a luminous appearance all over their ſurface: on the third, the light was not ſo vivid; and on the fifth it was almoſt extinct.

N. B. In experiments of this kind, for the production of light, the fiſhes ſhould always be gutted, the roes taken out, and the ſcales, if any, carefully removed. As the roes are likewiſe very productive of light, they ſhould be preſerved.

OBSER-

OBSERVATIONS.

Obs. 1. Thefe experiments clearly prove, that light begins to be emitted by marine fifhes, before any figns of putrefaction appear: they likewife demonftrate, that as foon as a great degree of putrefcence has taken place, the luminous property of the fifhes is deftroyed, and the light extinguifhed.

Obs. 2. In the inftance of light proceeding fpontaneoufly from animal flefh, recorded by Aquapendente, the flefh emitted light before any fenfible putrefcence had taken place, the meat being hung up in the larder for ufe. In that alfo mentioned by Bartholin, in 1641, the flefh muft have been frefh and fweet, for it was not intended to be dreffed until the next day. Mr. Boyle, in his report of light iffuing from flefh, exprefsly fays, that neither he, nor any of thofe who were about him, could perceive in it any offenfive fmell, whence to infer any putrefaction; the meat being judged very frefh, and well conditioned, and fit to be dreffed. And, laftly, Dr. Beale, in his account of a luminous neck of veal, fays, that when it was dreffed, on February the 27th, fome of the neighbours, who faw it fhining, were invited to eat of it, and all efteemed it as good as they had ever tafted; that a part of it was kept for February 28th and 29th, in which time it loft nothing of its fweetnefs.

Obs. 3. Whenever I wifh to obtain a plentiful fupply of light from fifhes, for the purpofe of experiments, I always endeavour to procure the frefheft that can be had: long experience and frequent difappointments have taught me to adopt fuch a precaution.

SECTION II.

The Light here treated of is a conftituent Principle of fome Bodies, particularly of Marine Fifhes, and may be feparated from them, by a peculiar Procefs; may be retained, and rendered permanent for fome Time. It feems to be incorporated with their whole Subftance, and to make a Part thereof, in the fame Manner as any other conftituent Principles.

EXPERIMENTS.

The Flefh of Herring *.

Exper. 1. A frefh herring was fplit, or divided longitudinally, by a knife, into two parts. Then, about four drams of it, being cut acrofs, were put into a folution, compofed of two drams of Epfom falt or vitriolated magnefia, and two ounces of cold fpring water drawn up by the pump. The liquid was contained in a wide-mouthed three-ounce phial, which was placed in the laboratory. Upon carefully examining the liquid, on the fecond evening after the procefs was begun, I could plainly perceive a lucid ring (for the phial was round) floating at the top of the liquid, the part below it being dark; but, on fhaking the phial, the whole at once became beautifully luminous, and continued in that

* The quantity ufed in each experiment was about four drams.

3   ftate.

ftate. On the third evening, the light had again rifen to the top; but the lucid ring appeared lefs vivid, and, on fhaking the phial as before, the liquid was not fo luminous as on the preceding night.

Exper. 2. The fame experiment was repeated. On the fecond night, the liquid, being agitated, was very luminous; on the third, not fo lucid; and on the fourth, the light was extinguifhed.

Exper. 3. With fea falt or muriated natron half a dram, and two ounces of water. On the fecond night, the liquid, when agitated, was dark; on the the third, lucid; on the fourth, very luminous; on the fifth, it began to lofe light; on the fixth, it continued to decreafe; and on the feventh it was quite gone. Neither the liquid, nor the herring, had contracted any putrid fmell.

Exper. 4. With fea water two ounces. On the fecond night, dark; on the third, fourth, and fifth, luminous; on the fixth, nearly extinct; and on the feventh, totally. The piece of herring, when taken out and examined, was remarkably fweet.

Roe of Herring *.

Exper. 5. With Epfom falt two drams, and water two ounces. On the fecond night, the liquid was pretty luminous; on the third and fourth, ftill luminous; and on the fifth its light was extinct.

Exper. 6. With Glauber's falt or vitriolated natron two drams, to two ounces of water. On the fecond night, when the phial was fhaken, as ufual in all thefe experiments, the liquid was pretty luminous; on the third, lefs fo; and on the fourth the light was fcarcely vifible.

Exper. 7. With fea water two ounces. On the fecond night, dark; on the third, the liquid was moderately luminous; on the fourth and fifth, it had extracted much light; and on the feventh it was ftill fhining. After this procefs, both the roe and the fea water remained perfectly fweet.

The Flefh of Mackerel.

Exper. 8. With Epfom falt two drams, and water two ounces. On the fecond night, the liquid was finely illuminated; on the third, a fimilar appearance; on the fourth, a diminution of light; on the fifth, it continued lucid in a fmall degree; annd on the fixth the light was extinguifhed.

Roe of Mackerel.

Exper. 9. With Epfom falt two drams, and water two ounces. On the fecond night, the liquid, when agitated, was exceedingly bright; on the third, the fame; and on the fourth and fifth, ftill lucid.

* The quantity ufed in each experiment was about four drams.

The Tadpole.

Exper. 10. It occurred to my mind, in the year 1797, to try what effect a saline menstruum would have upon the tadpole. Accordingly, I procured some tadpoles on the 10th of June, and put six of them into a solution of two drams of Glauber's salt in two ounces of water. On the 11th, in the evening, the menstruum was dark; on the 12th, after shaking the phial, I was agreeably surprised to find it impregnated with light; on the 13th, the light was so abundant as to float on the top of the menstruum; on the 14th, the same phænomenon appeared; on the 15th and 16th, it was still present; on the 17th, the lucidness began to diminish; on the 18th, it was faint; and on the 19th it had vanished.

Exper. 11. On the 11th of June, six other tadpoles were dropped into a solution of one dram of common salt in three ounces of water. On the 12th and 13th, the menstruum was dark; on the 14th, it had extracted from the tadpoles a very beautiful bright light; on the 15th, the menstruum was exceedingly luminous; on the 16th and 17th, nearly the same: the light then gradually faded, so that on the 21st it was merely visible; and on the 22d it disappeared.

Exper. 12. On the 21st of June, the above two experiments were repeated; when the tadpoles remained in the menstruums till the 27th, but no light was emitted. What was the cause of this failure in these two last experiments? Was it the ten days' increased growth of the animal, which was taken from the same pond, that made the difference?

Exper. 13. The above experiments were repeated, when the tadpole had just put on the state of a frog, but without producing any lucid appearance.

The Light is incorporated with the whole Substance of Marine Fishes.

Exper. 14. A fine fresh herring, being gutted, was divided longitudinally into two parts, both of which were hung up, by pieces of string, in the laboratory. On the 2d night, they were very lucid on the skinny side, but not on the fleshy or inward part; on the 3d, the fleshy or central parts of the fish, were thickly covered with a rich azure light; on the 4th, they continued exceedingly luminous; and on the 5th and 6th they were still lucid. It is surprising to think what a profusion of light was emitted from the interior substance of this single fish.

Exper. 15. A similar experiment was made with a mackerel, and with similar effects. These two experiments were frequently repeated.

Exper. 16. But the soft-roe, of both the herring and the mackerel, abounds more with light than even the flesh. When it is in its most luminous state, which generally happens about the 3d or 4th night, it will sometimes shine so very splendidly, as to appear like a complete body of light. It is remarkable that the hard-roe, in general, does not emit so much light as the soft-roe. When the roes were used, they were laid upon plates, and deposited in the laboratory.

OBSER-

OBSERVATIONS.

Obs. 1. The above experiments clearly prove, as I apprehend, that this light is a conftituent principle of marine fifhes: and that it is feparated, by the menftruum employed on this occafion, in the fame way that the principles of any other body are feparated, by the menftruum fitted to decompofe it. They likewife fhow, that it is not partially but wholly incorporated with every part of their fubftance, and makes a part thereof, in the fame manner as any other conftituent principle.

Obs. 2. Light is probably the firft conftituent principle that efcapes, after the death of marine fifhes. The experiments of the firft Section teach us that it appears foon after death, even in fifhes which, to the eye, feem quite frefh and fweet; or, at leaft, long before any fenfible putrefcence takes place. And we have feen that the flefh and roes, infufed in the faline menftruums, continued to emit light for feveral days, without undergoing any apparent putrefactive change.

Obs. 3. The experiments likewife render it probable, that no offenfive putrefaction takes place in the fea, after the death of fuch myriads of animals as muft needs daily perifh in the vaft ocean, (quite contrary to what happens on land;) and that the flefh of marine fifhes remains pretty fweet for fome time, and may become wholefome food for many kinds of thofe which ftill remain alive. An eminent inftance this, of the wifdom of the Creator, in the conftruction of the aqueous part of the world, which comprehends, by far, the greateft portion of the terraqueous globe, and is the moft replete with animal life!

SECTION III.

Some Bodies or Subftances have a Power of extinguifhing fpontaneous Light when it is applied to them.

EXPERIMENTS.

The luminous matter proceeding from the herring and the mackerel, was quickly extinguifhed when mixed with the following fubftances: 1. Water alone. 2. Water impregnated with quick-lime. 3. Water impregnated with carbonic acid gas. 4. Water impregnated with hepatic gas. 5. Fermented liquors. 6. Ardent fpirits. 7. Mineral acids, both in a concentrated and diluted ftate. 8. Vegetable acids. 9. Fixed and volatile alkalis, when diffolved in water. 10. Neutral falts: *viz. faturated* folutions of Epfom falt, of common falt, and of fal ammoniac. 11. Infufions or chamomile flowers, of long pepper, and of camphor, made with boiling-hot water, but not ufed till quite cool. 12. Pure honey, if ufed alone.

(To be concluded hereafter.)

IX.

On Areometry; more particularly as it relates to Alcohol of different Strengths and Temperatures. By Cit. HASSENFRATZ [*]

THE Author calls the inftruments made ufe of for determining the proportion of mixtures of alcohol and water, alcogrades; and after a fhort preface refpecting the ufes of fpirituous and vinous fluids, he proceeds to enumerate the following fix methods of meafuring the ftrength of brandies, &c. 1. By the bubble or bead which appears when the fluid is fhaken. 2. The fwimming or finking of oil poured therein. 3. Diftillation, to fhow the quantity of fpirit. 4. Burning the fluid in a filver veffel, and noting the refidue. 5. Wetting a known quantity of dry gunpowder with a little of the fpirit, and obferving the facility or difficulty with which it inflames. Thefe five methods are, as he remarks, not only too uncertain to fhew the difference of fpirit with the defired precifion, but will even give very different refults, according to the management, with famples of the very fame fpirit. 6. The fixth method confifts in determining the fpecific gravity with the Areometer, which as it has an appearance of precifion, deferves to be more minutely examined.

The Areometer or inftruments for determining this fpecific gravity of fluids are of two kinds, namely, 1. a floating ball, with a ftem above, either graduated or fupporting a difh to receive weights, and a counterpoife below to preferve the erect pofition; and 2. a bottle terminating in two fmall apertures, and capable of holding the fame invariable bulk of fluid.

The firft, namely, the hydrometer with the graduated ftem is moft in ufe. Inftruments of this kind have been graduated in France by obferving certain points to which the fubfidence was made in water and in alcohol, or in a faline folution. Nine of thofe more generally ufed in that country are exhibited in the following table:

---

[*] Abftracted from the fourth Memoir on this fubject, in the Annales de Chimie, XXXIII. 3.

Table of Comparison for the Areometers of

Names of the Liquors.	Lantenay	Cartier	Baumé	Buffat	Machi		Mac Daniel	Juges d'Aunis	Marchands de Paris	Struve	Specific gravity
					Names of the Inventors or Companies.						
Rectified Alcohol	80	36	40	100	66	0	33	16	130	37.5	0.8276
Melasses Spirit	78	35	38	93	64	1	30.75	15.5	127	35.5	0.8372
Common Alcohol	74	33	35.3	87	62	2	27.8	13	121	33	0.8508
Cognac 6—11	65	31	32.75	79	52	7	25	12	106	30.75	0.8636
Brandy 4—7	60	30	32	75	49	9	24	11.75	100	30	0.8675
Brandy of Barcelona	61	31	32.75	79	51	7.75	25	11.5	102	30.7	0.8636
— of Montpellier	59	29.75	31	75	47	9.75	23	11.3	95	29	0.8727
— potable simp. 4 years	30	20	20.5	40	23.75	22	11.3	3.5	48	19.6	0.9320
of 20 years	28	20	20	40	23	22.75	10.5	3.5	46	19.1	0.9351
— artificielle 6—11	25	19	18.75	34	20	23.75	9.3	2.5	40	17.	0.9427
Red Champagne wine	5	12	11	10	2	33	0.5	0	13	11.5	0.9931
White Burgundy	4	11.75	11	9.	2	33	0.5	0	14	11.	0.9931
White Orleans Vinegar	0	10	9	2	0			0	0	9.	1.0070
Distilled Water	0	10	10	5	0	34	0.5	0	0	10.	1.0000

From the imperfection of these instruments, particularly with regard to the indication of the component parts of spirituous mixtures, it has been an object of importance to philosophers, and the projectors of legislative imposts, to examine the subject with great precision. Among the philosophers who have made researches on this head, our author confines his notice to Baumé, Brisson, Gouvenain, Struve, Jacob Fagot, of Stockholm, and Blagden. Of the results obtained by these philosophers, he gives concise tables, by means of which he has traced curves to exhibit the same with their comparative degrees of regularity and respective differences. See Plate XVIII.

In examining these curves, he found that those of Fagot and Struve abound with sinuosities, which prove that their experiments were not made with due care, and ought to be rejected out of the comparison: besides which, the fluids they used were too remote in their densities, from those which were subjected to experiment by the four other philosophers.

On examining the curves constructed from the experiments of the last, it was found that the curve of Baumé contains also a great number of sinuosities; that of Blagden less, and those of Brisson and Gouvenain were the most regular. These curves also appear to indicate, that the alcohol used by Brisson had a stronger attraction for water than that of Gouvenain; that this last was stronger in this respect than that of Blagden: and lastly, that the spirit used by Baumé had a weaker attraction than either of the other three. This important observation shews, that the practical use of any set of tables in determining the specific gravities of different mixtures of spirit and water, must, to a certain degree, be confined to that species of liquor from which the tables, or equivalent instrument was formed.

3,

Notwith-

Notwithſtanding the regularity of the tables of Briſſon and Gouvenain, Cit. Haſſenfratz was obliged to rejeƈt them in his farther proceedings, becauſe thoſe of Briſſon were made at one temperature only; and from the very uniform courſe of the differences in thoſe of Gouvenain, he ſaw reaſon to conclude, that they muſt have been made by interpolation between few and diſtant experiments. He gives, nevertheleſs. tabulated abſtraƈts of their reſults. The Royal Society's experiments publiſhed by Sir Charles Blagden, are conſequently the foundation of his conſtruƈtion of an alcograde.

If from theſe experiments curves be traced, expreſſing the denſities of the ſeveral combinations of water and alcohol taken at different temperatures, he obſerves, that with regard to alcohol and its ſeveral combinations, as low as nine parts of alcohol, and one of water, the denſity follows the inverſe proportion of the temperature, and the curve becomes a ſtrait line; but that every leſs proportion of alcohol follows a different inverſe proportion; that is to ſay, the line which paſſes through the extremities of the ordinates, is a curve of which the radius at curvature is longer the greater the proportion of water.

After ſome diſcuſſion of the method of forming the combinations by weight and by meaſure, the firſt of which has the great advantages of accuracy and facility of experiment, with regard to thoſe operations which require change of temperature; and the latter poſſeſſes that of being more accommodated to the commercial habits of ſociety; Citizen H. makes ſome obſervations on the difference between brandies, and the mixtures of alcohol and water. Brandy, ſays he, is the produƈt obtained by diſtillation from a vinous liquor, in which the aƈtion of fire changes the order of the component parts, and carries over this fluid: alcohol is the produƈt of a ſecond operation of the ſame nature, in which ſimilar effeƈts are produced upon the brandy. In proof of the force of this method of conſidering the ſubjeƈt, he remarks, that the brandy will not again form wine by mixing it with the reſidue left in the ſtill: neither will the alcohol form brandy by mixture with the reſidue, from which it was produced and driven over.

The differences of the vinous fluids in denſity above or below that of water, in flavor, and other obvious charaƈters, are extreme; and the brandies from wine properly ſo called, from beer, perry, cyder, &c. are no leſs remarkable for their peculiar qualities. All theſe afford alcohol, which has been ſuppoſed to be then the ſame fluid; but our author conſiders theſe alſo as differing in their immediate and intrinſic qualities. The alcograde is inſufficient for the ſtriƈt exhibition of the variations of theſe with water, and moſt obviouſly with reſpeƈt to brandies, of which the price is governed ſo much by qualities and circumſtances, not at all commenſurate with their ſpecific gravities. For wines, beer, and other immediate produƈts of fermentation, this method of examination does not appear to be of any uſe.

He admits, nevertheleſs, that the alcograde is beſt adapted of any inſtrument for giving that approximate indication of value which the tranſaƈtions of life demand. He propoſes the conſtruƈtion of a floating inſtrument, which ſhall ſhew the proportion of alcohol by inſpeƈtion at any known temperature by graduation. The following table, is derived

rived from the Experiments of Gilpin, in the Philofophical Tranfactions 1794; but I muft, for the fake of brevity, leave the confideration of the method of its fabrication to the fcientific reader.

Table of the lengths of the Tubes of Alcogrades, which fhall fhew the Proportions by meafure of Alcohol in 1000 parts of any Mixture, at different Temperatures.

Proportion of alcohol in 1000 parts of the whole mixture.	Length of the Tube for the Temperatures Centigrade.						
	0°	5°	10°	15°	20°	25°	30°
0	—9	—10	—7	0	+9	+21	+36
100	+102	+102	+113	118	130	141	152
200	180	182	197	205	220	243	257
300	240	260	279	300	328	344	362
400	327	356	424	427	442	468	493
500	446	482	515	552	588	620	660
520	484	516	559	587	624	658	698
540	508	551	585	623	661	697	737
560	550	587	622	660	699	737	777
580	589	624	660	698	739	778	819
600	630	662	700	738	780	820	862
620	670	702	742	779	823	863	906
640	711	743	786	822	868	908	952
660	753	785	831	866	914	954	1001
680	796	828	877	912	963	1001	1052
700	839	878	925	961	1012	1050	1104
720	885	921	976	1006	1062	1101	1157
740	933	970	1023	1061	1113	1154	1211
760	983	1021	1081	1127	1165	1209	1266
780	1036	1075	1135	1178	1217	1267	1322
800	1091	1130	1190	1231	1270	1327	1379
820	1147	1188	1247	1288	1327	1388	1437
840	1205	1248	1305	1348	1388	1450	1496
860	1266	1310	1364	1411	1451	1513	1556
880	1327	1375	1424	1477	1518	1577	1618
900	1390	1441	1485	1545	1588	1641	1682
920	1456	1507	1550	1616	1660	1709	1751
940	1524	1578	1620	1690	1734	1781	1826
960	1595	1652	1696	1767	1809	1858	1907
980	1669	1729	1778	1847	1885	1941	1994
1000	1741	1810	1864	1930	1961	2025	2086

In applying this table to the purpofe of graduating the (cylindrical or prifmical) ftem of an alcograde or hydrometer, the author fuppofes the inftrument to be plunged in a liquid of the fpecific gravity 90,000, and afterwards in another liquid * of the fpecific gravity

100,000

* In this procefs, fince thefe divifions indicate ten thoufandth parts of the portion moft deeply immerfed, and there are 2096 divifions on the whole ftem, it will conftitute rather more than one-fifth part of the bulk

of

100,000, and that the difference or interval upon the ſtem between the interſection by the ſurface of the fluid in the firſt and in the laſt ſituation, ſhall be taken as a ſcale of 1000 parts; and of theſe parts the requiſite portions being ſet off from the table upon the ſtem, as marked Fig. 1. Pl. XVIII. the inſtrument is then ready for uſe.

It ſeems almoſt unneceſſary to remark, that the ſcale upon the ſtem muſt be read upon that vertical line which is marked with the temperature which a thermometer ſhews the fluid to poſſeſs. The inclined croſs line, which interſects at the ſame place that vertical line and the ſurface of the fluid, is marked with the number denoting the proportion of alcohol contained in 1000 parts of the fluid. The reduction of this reſult to any deſired ſtrength or proof, of which the proportions of alcohol and water are known, will be eaſy to moſt arithmeticians, but would extend our limits too far if examples were to be given here.

of the inſtrument. Hence by a rough calculation, if the length of the ſtem be about five times the diameter of the ball, the diameter of the ſtem will prove about one-ſixth part of the diameter of the ball.

It is not neceſſary to prepare two fluids of the preciſe denſities above mentioned in order to find the interval, as it may eaſily be had from any two fluids whatever. Thus, if water at 1000, and alcohol at 840, were uſed, the interval on the ſtem would be denoted by $\frac{1000-840}{1000}$ or $\frac{160}{1000}$ or $\frac{1600}{10000}$ of the portion moſt deeply immerſed, which are nearly the diviſions of the table. Or, if a ſingle fluid be preferred, then--- 1. Weigh the whole inſtrument ſuſpended to the diſh of a pair of ſcales. 2. Find the apparent loſs the inſtrument ſuffers by immerſion in water, with a load or ballaſt attached to it, of which the reſidual weight is known, and can be allowed for. This immerſion muſt be adjuſted to the upper diviſion of the ſtem. 3. Place a weight in the oppoſite ſcale, equal to one-fifth of that apparent loſs. The inſtrument will riſe, and the ſurface of the water will interſect a point at the diſtance of 2000 diviſions below the firſt. 4. Or if any ſubdiviſion of that fifth part be put into the ſcale, the riſe of the ſtem will be a proportionate part of 2000.---W. N.

Wheel for shewing the Planetary motions.

Fig. 2.

Fig. 1.

Fig. 3.

Fig. 5.

Fig. 4.

Specific Gravity & Strength of ardent Spirit

Fig. 2.

Jacob Payer

Soure

Blagden

Baume

Gourcauin

Dickson

Fig. 7.

Proportion of alcohol in 100 parts of the Mixture

Centigrade Temperature

A

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AND
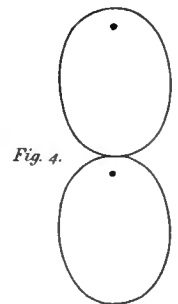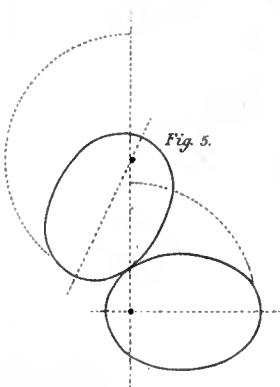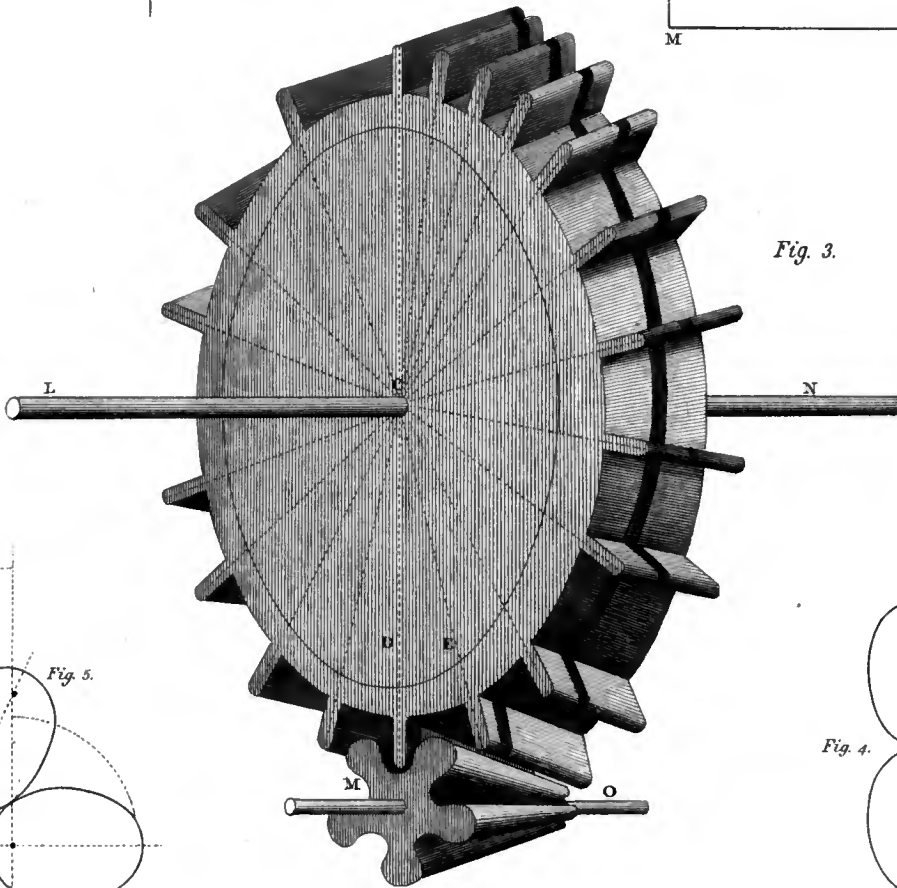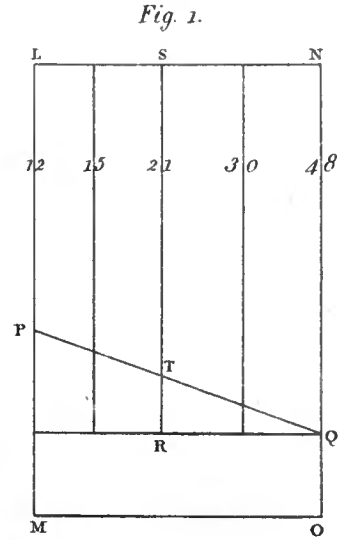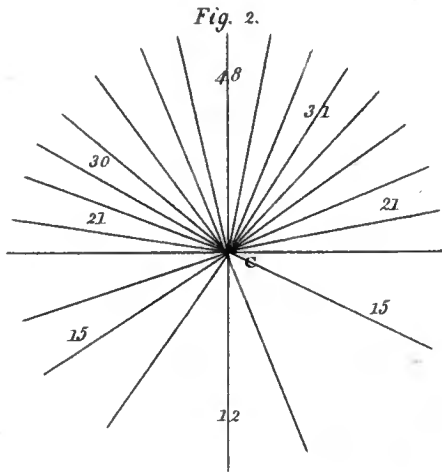
THE ARTS.

ARTICLE I.

Conſtruction and Uſe of an univerſal Table of Intereſt. By H. Goodwyn, Esq.

To Mr. NICHOLSON.

SIR,

As you have inſerted other tables in your valuable periodical work, perhaps the encloſed may be thought worthy your notice.

It is not only applicable to the calculation of intereſt in this, but alſo in all other nations, and probably is as complete and conciſe as the nature of ſuch a performance will admit. It is derived from that principle of prime numbers you have already honoured with publication.

I am,

SIR,

Your moſt humble ſervant,

II. GOODWYN.

Eaſt Smithfield, Dec. 15, 1800.

3 K

A De-

A Description of the Table.

This table consists of ten principal divisions.

In the left hand column of the first nine divisions are two cyphers, which in use have always the precedence.

The second column from the left of each of the first nine divisions contains at the top a cypher, followed by the nine digits, both arithmetically and perpendicularly arranged. And, either the cypher, or one of the digits, is always to be placed next to and on the left hand of the two cyphers first above-mentioned.

At the top of each of the first nine principal divisions is an horizontal line, containing eight circulating figures, which are to be annexed to the two first mentioned cyphers, and the other cypher, or digit, next mentioned above, according to such an arrangement as will be hereafter described.

In the body of each of the first nine divisions, and opposite either to the cypher or to one of the nine digits under the arithmetical arrangement before-mentioned, are ten broken horizontal rows of days.

The	1st, or uppermost rows, against the cyphers, contain all the days from		1 to 36
	2d,	do.	1	do.	37 to 72
	3d,	do.	2	do.	74 to 109
	4th,	do.	3	do.	110 to 145
	5th,	do.	4	do.	147 to 182
	6th,	do.	5	do.	183 to 218
	7th,	do.	6	do.	220 to 255
	8th,	do.	7	do.	256 to 291
	9th,	do.	8	do.	293 to 328
	10th,	do.	9	do.	329 to 364

The tenth principal divisions contains five sets of days, with the interest of 1*l. per cent. per annum*, placed against each of them respectively.

Rules for applying the Table.

Seek the number of days, for which the interest is required, in one of the first nine principal divisions, and these will be easily found by the arrangement above stated.

In the left hand column of the division adjoining the given number of days are two cyphers, with a decimal point on their left.

To these two cyphers add, from the next column, the cypher or single digit, which is placed in a line with the days first found.

Next to these cyphers or figures, and on their right, place the eight circulating figures, which are at the top of the division, beginning with that immediately over the given number of days (and which will be the first of the new circulating part of the complete interest)

3   and

and take out the whole eight in the order they ſtand, ending with that immediately on the left hand (which will be the laſt figure of the circulating part.) You will then have obtained the complete intereſt of 1*l.* at 1 *per cent.* for the given number of days.

To find the intereſt at 1 *per cent.* of the decuples of 1*l.* viz. 10*l.* 100*l.* 1000*l.* &c. move the decimal point one, two, three, &c. places more to the right hand, and the complete intereſt of the decuples will be obtained.

The intereſt of 1*l.* to its decuples at 1*l. per cent.* for any given number of days having been found, the intereſt of any other ſum, at the ſame rate, may eaſily be derived from it by the rule of practice.

And this amount being obtained, let it be multiplied by any given rate of intereſt, and the product will be the complete intereſt of any given ſum, for any given number of days at the given rate of intereſt.

EXAMPLE I.

Required the intereſt of 1*l.* for 1 day, at the rate of 1 *per cent. per annum.*

In the firſt line of the firſt principal diviſion is one day, and in the
left hand column are two cyphers, viz. - - ·00

In the ſecond column from the left, and againſt the one day, is a
cypher, viz. - - - - 0

In the line of circulates, immediately over the one day, is a cypher,
viz. - - - - - 0

And this is the firſt circulate of the complete intereſt.

The other ſeven circulates follow in rotation, thus - - 2739726

This ſum is the anſwer £. ·00002732726

EXAMPLE II.

Required the complete intereſt of 1*l.* at 1 *per cent. per annum* for 364 days.

By the foregoing part or deſcription of the table 364 days muſt be in the tenth, or laſt, column of one of the firſt nine principal diviſions; and accordingly it is found at the bottom of the firſt diviſion.

As before, in the left hand column are two cyphers - ·00

In the ſecond column from the left, and againſt the 364 days, is fig. 9

In the line of circulates immediately over 364 days, is fig. 9

The remaining circulates are - - - 7260273

Anſwer £. ·00997260273

EXAMPLE

EXAMPLE III.

Required the interest of 10*l.* 100*l.* and 1000*l.* for 1 day, at 1 *per cent.*
The interest of 1*l.* at 1 *per cent.* for 1 day, *per example 1,* is   £· 00002739726

do.	10*l.*	at	do.	·0002739726
do.	100*l.*	at	do.	·002739726
do.	1000*l.*	at	do.	·02739726

EXAMPLE IV.

Required the interest of 1000000·*l.* for 364 days at the rate of 1*l. per cent. per annum.*
By examples 2 and 3, applied properly, the answer will be $9972·60273 =$
£·9972·12·0½.

EXAMPLE V.

Required the interest of 1000000·*l.* for 247 days at 5*l. per cent.*
The interest of 1000000·*l.* for 247 days, at 1*l. per.*
   cent. is   -   -   -   -   £6767·12328
               5

      Answer $£· 33835·61643 = £· 33835 \ 12 \ 3$

EXAMPLE VI.

Required the interest of 1220 at 4*l. per cent. per annum* for 263 days.
The interest of 1*l.* at 1*l. per cent. per annum* for
   263 days is ·00720547945, and of £. 1000·   is £. 7·20547
         200 or ⅓   1·44109
         20 or ¹⁄₁₀   ·14416

           £· 8·79066
             4

      Answer $£· 35·1624 = £· 35 \ 3 \ 3$

A Table

A TABLE

Shewing, by Inſpeƈtion, the complete Intereſt of One Pound at the Rate of £. 1 per cent. per annum ; and by Induƈtion the complete Intereſt of any Sum, at any Rate, and for any given Number of Days, from 1 to 365, in Decimals of a Pound Sterling.

ö 2 7 3 9 7 2 ö

	Days.
0	1 10 27 ——————22
1	——51 72 63 46——
2	74 83 100 ————95
3	——124 145 136 119——
4	147 156 173 ————168
·00 5	——197 218 209 192——
6	220 229 246 ————241
7	——270 291 282 265——
8	293 302 319 ————314
9	——343 364 355 338——

i 0 9 5 8 9 0 ¼

	Days.
0	4———35———33———15
1	——40———58 69.———38——
2	77———108———106———88
3	——113———131 142———111——
4	150———181———179·———161
·00 5	——186———204 215———184——
6	223———254———252———234
7	——259———277 288———257——
8	296———327———325———307
9	——332———350 361———330——

2̇ 4 6 5 7 5 3 ¼

	Days.
0	9 17 24 21 ———————
1	————64 56 49 52
2	82 90 97 94 ———————
3	————137 129 122 125
4	155 163 170 167 ———————
·00 5	————210 202 195 198
6	228 236 243 240 ———————
7	————283 275 268 271
8	301 309 316 313 ———————
9	————356 341 348 344

ö 5 4 7 9 4 5 2̇

	Days.
0	2 20———29 ———19.——
1	——54———71 53———44
2	75 93———102 ———92——
3	——127———144 126———117
4	148 166———175 ———165——
·00 5	——200———217 199 ———190
6	221 239———248 ———238·——
7	——273———290 272———263
8	294 312———321 ———311——
9	——346———363 345———336

i 3 6 9 8 6 3 ö

	Days.
0	5———36———23 11——
1	——50 62———68———37
2	78———109———96 84——
3	——123 135———141———110
4	151———182———169 157——
·00 5	——196 208———214———183
6	224———255———242 230——
7	——269 281———287———256
8	297———328———315 303——
9	——342 354———360———329

⅓ 2 8 7 6 7 1 ½

	Days.
0	12———32 28———26——
1	——47———61———41 45
2	85———105 101———99——
3	——120———134———114 118
4	158———178 174———172——
·00 5	——193———207———187 191
6	231———251 247———245——
7	——266———280———260 264
8	304———324 320———318——
9	——339———353———333 337

ö 8 2 -1 9 1 7 ⅞

	Days.
0	3 30 8 7——————
1	——70 43 65 66
2	76 103 81 80——————
3	——143 116 138 139
4	149 176 154 153——————
·00 5	——216 189 211 212
6	222 249 227 226——————
7	——289 262 284 285
8	295 322 300 299——————
9	——362 335 357 358

i 6 4 3 8 3 5 ö

	Days.
0	6———16 14———13——
1	——60———67———57 59
2	79———89 87———86——
3	——133———140———130 132
4	152———162 160———159——
·00 5	——206———213———203 205
6	225———235 233———232——
7	——279———286———276 278
8	298———308 306———305——
9	——352———359———349 351

¼ 9 3 1 5 0 6 ⅛

	Days.
0	18 34——————25 31
1	——48 42 55 39————
2	91 107———98 104
3	——121 115 128 112———
4	164 180——————171 177
·00 5	——194 188 201 185——
6	237 253——————244 250
7	——267 261 274 251——
8	310 326——————317 323
9	——340 334 347 331——

Days.	Intereſt.	Days.	Intereſt.	Days.	Intereſt.	Days.	Intereſt.	Days.	Intereſt.
73	£. ·002	146	£. ·004	219	£. ·006	292	£. ·008	365	£. 01

A Chemical

II.

A Chemical Analysis of Three Species of Whinstone, and Two of Lava.   By ROBERT KENNEDY, *M. D. F. R. S. & F. A. S. Edin.*

(Concluded from page 415.)

ANALYSIS III.

Whin of the Calton Hill, near Edinburgh.

THE rock of this hill varies much in different parts; but its general character is that of porphyry. The piece I chose for analysis was taken from the south side, about ten or twelve feet below its highest point; and was free from calcareous spar.

The external characters of this particular piece are as follow: it consists of a greyish basis, containing rhomboidal crystals of felspar of a light reddish-brown colour; and small spherical masses of green earth. The basis, in its fracture, is uneven, and earthy, and has no lustre. It can be scratched easily with a knife, and gives an earthy smell when breathed on. The green earth is soft, and is affected in some degree by water; and being decomposed by the weather, as well as the veins and nodules of calcareous spar, which are very common in this hill, the rock in many places is extremely porous.

The specimen I have described may be called argillaceous porphyry. It effervesces slightly with acids; so that the lime which it contains must be united to carbonic acid. Its powder is light grey, with a certain shade of purple. When heated to redness, it becomes of a brown colour. It is not attracted by the magnet, either in its natural state, or after ignition. By being exposed to a low red heat for half an hour, it loses 5 *per cent.* of its weight. It softens at 44 of Wedgwood. Its specific gravity is 2.663, as nearly as I could ascertain, from the effect which the water had in making it crumble down.

One hundred parts contain,

Silex,	50
Argil,	18.50
Oxyde of iron,	16.75
Carbonate of Lime,	3
Moisture, and other vol. matter,	5
Soda, about	4
Muriatic acid, about	1
	98.25

ANALYSIS

ANALYSIS IV.

Lava of Catania. Ætna.

This lava, and the species to be next mentioned, were brought from Mount Ætna by Sir James Hall and Dr. James Home. At their request I analyzed specimens of each. Mineralogists are well acquainted with these lavas, from the descriptions which have been given of them by M. Dolomieu; therefore it is unnecessary for me to mention their external characters.

The lava of Catania gives a powder of a light grey colour, which is very little changed in appearance by being heated red hot. After it is wet, it becomes dark grey. When this lava is reduced to small fragments, some parts of it are attracted by the magnet, and others are not. In fine powder it is but feebly attracted; and after ignition its qualities in this respect do not seem to be altered. It softens at 33 of Wedgwood. The specific gravity of the pieces most free from air bubbles, is 2.795.

I have exposed this lava, in the manner already described, to various degrees of heat, from redness to 158 of Wedgwood, and constantly observed that it never lost the smallest weight.

When boiled like the whins in muriatic acid, the part remaining undissolved weighed 68 *per cent.*

There are in 100 parts,

Silex,	51
Argil,	19
Oxyde of iron,	14.50
Lime,	9.50
Soda, about	4
Muriatic acid, about	1
	99.

ANALYSIS V.

Lava Sta Venere, Piedimonte, Ætna [*].

This species gives, like the preceding, a greyish powder, which becomes dark grey when wet, and its colour is scarcely affected by being exposed to a low red heat. It is but feebly attracted by the magnet, whether in small fragments or in powder; and its qualities in this respect are not changed by low ignition.

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[*] Dolomieu, in describing this lava, says, that its fracture is conchoidal, like that of silex. The specimen which I analyzed had an uneven fracture; and its colour was blackish-blue: in other respects, however, it answered to Dolomieu's description.

It

It softens at 32 of Wedgwood, and does not lose any weight in fires between 150 and 160. After being boiled in muriatic acid, it left 68 parts *per cent.* undissolved. Its specific gravity is 2.823.

In 100 parts I found,

Silex,	50.75
Argil,	17.50
Oxyde of iron,	14.25
Lime,	10
Soda, about	4
Muriatic acid, about	1
	97.50

In analyzing these two lavas, I examined the different solutions, with particular attention, for magnesia, and sulphuric acid; but could not detect any traces of either of these substances.

The results of these analyses show, that whins, and a certain class of lavas, taken from remote quarters of the globe, consist of the same component elements, united in each, nearly in the same proportion. The only circumstance in which they materially differ, is the loss of some volatile matter in the fire, which is peculiar to the whins alone.

We need not be now surprised at the facts mentioned by Dolomieu, and others, of soda being found about volcanos, or upon the surface of lavas; as it has thus been shown to exist in these substances in combination with their earthy bases.

The facts and experiments I am next to mention will prove, that whins and lavas are not the only stones which contain soda; and will even render it probable, that this alkali is widely diffused through mineral bodies.

Soon after I first discovered it in whins, and had communicated the circumstance to Sir James Hall, he sent me, from a high sandstone rock in his estate, a quantity of the stone decomposed; and informed me that there was a saline efflorescence mixed with it, which was collected along with the loose matter, and which seemed, by the taste, to be sea-salt. The place has been long called by the common people the *Salt Heugh*. By simply boiling some of the decomposed sandy part in water, and afterwards filtering and evaporating the water, I obtained regular crystals of sea-salt, mixed with a small quantity of sulphate of soda.

Hence it appeared likely, that common salt would be found to be one of the component parts of ordinary sandstone strata. To verify this important observation, I next examined two hard and solid specimens, taken from some depth below the surface, and perfectly free from decomposition. The first was broken from a quarry about two miles to the westward

of

of Edinburgh. A portion of it being reduced to minute grains, of such a size as the particles of the stone seemed to have consisted of originally, was mixed with some diluted nitric acid, and boiled with it gently for three hours. The acid, after being filtered, was examined with nitrate of barytes, with which it produced only a slight cloud. But nitrate of silver, when poured into it, threw down a copious white precipitate of muriate of silver, indicating the presence of muriatic acid.

After this precipitate was separated by filtration, the liquor which passed through was evaporated to dryness. A saline matter remained, which being mixed with some charcoal, and heated, deflagrated like nitrous salts. Having washed the coaly residuum, and evaporated the water, I got some perfectly pure carbonate of soda. There had been, therefore, in the sandstone, some common salt, which, by this process, was decomposed, and its acid and alkali collected separately; but whether the whole of the salt was obtained, and what proportion it bore to the earthy parts, I cannot determine, as the stone itself was not analyzed.

The next specimen was taken from a stratum of sandstone, which lies below the hill of Salisbury Craig; and I chose this species, because the whin to which it is contiguous has already been shown to contain soda. Some of it being reduced to the state of fine sand, and treated in every respect as the preceding, gave a portion of muriate of silver, and of carbonate of soda. The existence, therefore, of sea-salt in these varieties of sandstone, is thus fully established *.

The celebrated Mr. Klaproth of Berlin has already shown, that pot-ash enters into the composition of several stony substances; and by the experiments described in this paper, the other fixed alkali, soda, has also been proved to exist in mineral bodies, as it has been separated from nine different varieties; all of which also contain a certain quantity of muriatic acid.

As caustic fixed alkali was much used in these analyses, I shall conclude this paper by describing, in a few words, the method by which I prepare it; both because its purity is of the greatest importance, and because the process I employ differs, in some circumstances, from that of most chemists. Having obtained an alkali free from all earthy matter, either by burning white tartar, or by repeated solutions and crystallizations of carbonate of soda, I dissolve it in a considerable quantity of water. The requisite proportion of lime being slacked, and allowed to cool, it is diluted with water, and then mixed with the solution of the alkali. This mixture is frequently stirred during two or three days; for when no heat is applied, the lime requires a certain time to attract the whole of the carbonic acid. In the next place, the mixture is filtered through a piece of linen placed in a funnel, and water poured on it till the whole of the alkali is washed out. As it passes through, it is poured, at intervals, from the bottle which receives it first, into a second that is closely stopped.

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* Since these experiments were performed, I have seen several decomposed sandstones, on the surfaces of which there was an efflorescence of common salt.

In making the fixed alkalis cauftic, it is ufual to boil them with the lime; but as moft kinds of limeftone contain a fmall quantity of filex or argil, and as thefe earths, when in a ftate of divifion, are foluble in boiling cauftic alkali, there is a probability, when heat is applied, of its being thus rendered impure. This is my reafon for carefully avoiding heat in the firft part of the procefs; and I have not found that lime made from chalk, or from the purer kinds of limeftone *, give any impurity whatever to the alkali, when mixed with it cold.

The folution, in its weak ftate, is firft evaporated in a bafon of hammered iron, polifhed; but when it is fomewhat concentrated, I carry on the evaporation in a difh made of the pureft filver, reduced from *luna cornea*. After being boiled to a fmall quantity, it is allowed to cool, and then put into a well-ftopped bottle for fome days; during which, if the evaporation has been continued long enough, the neutral falts cryftallize, as Mr. Lowitz has pointed out. Afterwards, the folution is carefully decanted from thefe falts, and again evaporated in the filver difh, till it acquires the confiftence of thin oil †. In this ftate, fo little water is prefent, that any part of the alkali, which may be united to carbonic acid, cryftallizes, and alfo any neutral falt that may remain; and the folution being decanted a fecond time, is obtained perfectly pure, colourlefs, and tranfparent.

In the laft boiling it is fomewhat difficult to obferve the exact degree of concentration at which all the alkaline carbonate will cryftallize; and if the evaporation is carried too far, the cauftic alkali, if pot-afh, cryftallizes itfelf: fo that feveral evaporations are fometimes requifite.

When no more water remains in the folution than is juft fufficient to hold the cauftic alkali diffolved, it contains nearly half its weight of alkali; but the exact quantity is eafily known, by evaporating a portion to drynefs in a filver crucible. Before ufing fuch a folution for analyfes, I afcertain its purity in the following manner: fome of it being fuperfaturated with perfectly pure nitric acid, is examined with nitrate of barytes and of filver; with neither of which, if properly made, it will give the fmalleft cloud; confequently it can contain no fulphuric or muriatic acid.

Another portion being faturated exactly with a pure acid, the whole is evaporated to drynefs, and the falt left is rediffolved in a little water. If any earth were contained in the cauftic alkali, it would remain thus undiffolved; but when made as above defcribed, I have never, in this examination, obferved the fmalleft fediment.

After the alkali is purified from neutral falts, and from the part united to carbonic acid, it may itfelf be cryftallized by farther evaporation, as Mr. Lowitz has fhown. But this procefs feems of no ufe in chemical analyfis, as the alkali is previoufly obtained altogether pure.

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* Mr. Klaproth ufes lime made of Carrara marble, which he boils with the alkali. (*Beitrage*, vol. i. preface.)

† Mr. Lowitz, in defcribing his procefs for cryftallizing cauftic pot-afh, directs the laft evaporation to be performed in a glafs retort. This method is very erroneous; as the alkali, when heated and concentrated, will diffolve large quantities of the glafs.

4

Defcription

III.

Description and Use of a cheap and simple Apparatus for teaching the first Principles of Mechanics: By Richard Lovell Edgeworth, Esq. F. R. S. and M. R. I. A. *

WE do not mean to undervalue either the application of strict demonstration to problems in mechanics, or the exhibition of the most accurate machinery in philosophical lectures; but we wish to point out a method of giving a general notion of the mechanical organs to our pupils, which shall be immediately obvious to their comprehension, and which may serve as a sure foundation for future improvement. When a person perceives the effect of his own bodily exertions with different engines, and when he can compare in a rough manner their relative advantages, he is not disposed to reject their assistance, or to expect more than is reasonable from their application. The young theorist in mechanics thinks he can produce a perpetual motion! When he has been accustomed to refer to the plain dictates of common sense and experience, on this, as well as on every other subject, he will not easily be led astray by visionary theories.

To bring the sense of feeling to our assistance in teaching the uses of the mechanic powers, the following apparatus was constructed, to which we have given the name Panorganon.

It is composed of two principal parts; a frame to contain the moving machinery; and a *capstan* or *windlass*, which is erected on a *silt* plank, that is sunk a few inches into the ground; the frame is by this means and by six braces or props rendered steady. The cross rail, or *transom*, is strengthened by braces and a king-post to make it lighter and cheaper. The *capstan* consists of an upright shaft, upon which are fixed two *drums*; about which a rope may be wound up, and two levers or arms by which it may be turned round. There is also a screw of iron coiled round the lower part of the shaft, to shew the properties of the screw as a mechanic power. The rope which goes round the *drum* passes over one of the pulleys near the top of the frame, and under another pulley near the bottom of the frame. As two *drums* of different sizes are employed, it is necessary to have an upright roller to conduct the rope in a proper direction to the pulleys, when either of the *drums* is used. Near the frame, and in the direction in which the rope runs, is laid a platform or road of deal boards, one board in breadth, and twenty or thirty feet long, upon which a small sledge loaded with different weights may be drawn. Plate XLX. Fig. 1.

F. F. The frame. b. b. Braces to keep the frame steady. a. a. a. Angular braces to strengthen the transom; and also a *king-post*. S. A round, taper, shaft, strengthened above and below the mortices with iron hoops. L L. Two arms or levers by which the

* Extracted by permission from " Practical Education by Maria Edgeworth, and Richard Lovell Edgeworth, F. R. S. and M. R. I. A."

shaft

fhaft, &c. are to be moved round. D D. The drum, which has two rims of different circumferences. R. The roller to conduct the rope. P. The pulley, round which the rope paffes to the larger drum. P 2. Another pulley to anfwer to the fmaller drum. P 3. A pulley through which the rope paffes when experiments are tried with levers, &c. P 4. Another pulley through which the rope paffes when the fledge is ufed. Ro. The road of deal boards for the fledge to move on. Sl. The fledge with pieces of hard wood attached to it to guide it on the road.

Ufes of the Panorganon.

As this machine is to be moved by the force of men or children, and as their force varies not only with the ftrength and weight of each individual, but alfo according to the different manner in which that ftrength or weight is applied, it is, in the firft place, requifite to eftablifh one determinate mode of applying human force to the machine; and alfo a method of determining the relative force of each individual whofe ftrength is applied to it.

To eftimate the Force with which a Perfon can draw horizontally by a Rope over his Shoulder.

Experiment 1. Hang a common long fcale-beam (without fcales or chains) from the top or *tranfom* of the frame, fo as that one end of it may come within an inch of one fide or poft of the machine. Tie a rope to the hook of the fcale-beam, where the chains of the fcale are ufually hung, and pafs it through the pulley P 3, which is about four feet from the ground; let the perfon pull this rope from 1 towards 2, turning his back to the machine, and pulling the rope over his fhoulder, Fig. 6. As the pulley may be either too high or too low to permit the rope to be horizontal, the perfon who pulls it fhould be placed ten or fifteen feet from the machine, which will leffen the angular direction of the cord, and the inaccuracy of the experiment. Hang weights to the other end of the fcale-beam, till the perfon who pulls can but juft walk forward, pulling fairly without propping his feet againft any thing. This weight will eftimate the force with which he can draw horizontally by a rope over his fhoulder *. Let a child who tries this walk on the board with dry fhoes; let him afterwards chalk his fhoes, and afterwards try it with his fhoes foaped: he will find that he can pull with different degrees of force in thefe different circumftances; but when he tries the following, let his fhoes be always dry, that his force may be always the fame.

To fhew the Power of the three different Sorts of Levers.

Experiment 2. Inftead of putting the cord that comes from the fcale-beam, as in the laft experiment, over the fhoulder of the boy, hook it to the end 1 of the lever L, Fig. 2. This lever is paffed through a focket, Fig. 3. in which it can be fhifted from one of its

* Were it thought neceffary to make thefe experiments perfectly accurate, a fegment of a pulley, the radius of which is half the length of the fcale-beam, fhould be attached to the end of the beam; upon which the cord may apply itfelf, and the pulley (P 3.) fhould be raifed or lowered, to bring the rope horizontally from the man's fhoulder when in the attitude of drawing.

ends

ends towards the other; and can be faftened at any place by the fcrew of the focket. This focket has two gudgeons, upon which it, and the lever which it contains can turn. This focket and its gudgeons can be lifted out of the holes in which it plays, between the rail R R, Plate XIX. Fig. 2. and may be put into other holes at R R, Fig. 5. Loop another rope to the other end of this lever, and let the boy pull as before. Perhaps it fhould be pointed out, that the boy muft walk in a direction contrary to that in which he walked before; viz. from 1 towards 3. The height to which the weight afcends, and the diftance to which the boy advances, fhould be carefully marked and meafured; and it will be found, that he can raife the weight to the fame height, advancing through the fame fpace as in the former experiment. In this cafe, as both ends of the lever moved through equal fpaces, the lever only changed the direction of the motion, and added no mechanical power to the direct ftrength of the boy.

Experiment 3. Shift the lever to its extremity in the *focket*; the middle of the lever will be now oppofite to the pulley, Fig. 4; hook to it the rope that goes through the pulley P 3, and faften to the other end of the lever the rope by which the boy is to pull. This will be *a lever of the fecond kind,* as it is called in books of mechanics; in ufing which, *the refiftance is placed between the centre of motion or fulcrum, and the moving power.* He will now raife double the weight that he did in Experiment II. and he will advance through double the fpace.

Experiment 4. Shift the lever, and the focket which forms the axis, (without fhifting the lever from the place in which it was in the focket in the laft experiment) to the holes that are prepared for it at R R, Fig. 5. The free end of the lever E will now be oppofite to the rope, and to the pulley (over which the rope comes from the fcale-beam). Hook this rope to it, and hook the rope by which the boy pulls to the middle of the lever. The effect will now be different from what it was in the two laft experiments; the boy will advance only half as far, and will raife only half as much weight as before. This is called *a lever of the third fort.* The firft and fecond kinds of levers are ufed in quarrying; and the operations of many tools may be referred to them. The third kind of lever is employed but feldom, but its properties may be obferved with advantage whilft a long ladder is raifed, as the man who raifes it is obliged to exert an increafing force till the ladder is nearly perpendicular. When this lever is ufed, it is obvious, from what has been faid, that the power muft always pafs through lefs fpace than the thing which is to be moved; it can never, therefore, be of fervice in gaining power. But the object of fome machines is to increafe velocity, inftead of obtaining power, as in a fledge-hammer moved by mill-work. (V. the plates in Emerfon's Mechanics, No. 236.)

The experiments upon levers may be varied at pleafure, increafing or diminifhing the mechanical advantage, fo as to balance the power and the refiftance, to accuftom the learners to calculate the relation between the power and the effect in different circumftances; always pointing out, that whatever excefs there is in the power*, or

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* The word *power* is here ufed in a popular fenfe, to denote the ftrength or efficacy that is employed to produce an effect by means of any engine.

in

in the refiftance, is always compenfated by the difference of fpace through which the inferior paffes.

The experiments which we have mentioned are fufficiently fatisfactory to a pupil, as to the immediate relation between the power and the refiftance; but the different fpaces, through which the power and the refiftance move when one exceeds the other, cannot be obvious, unlefs they pafs through much larger fpaces than levers will permit.

Experiment 5. Place the fledge on the fartheft end of the wooden road, Fig. 1.; faften a rope to the fledge, and conduct it through the loweft pulley P 4, and through the pulley P 3, fo as that the boy may be enabled to draw it by the rope paffed over his fhoulder. The fledge muft now be loaded, till the boy can but juft advance with fhort fteps fteadily upon the wooden road; this muft be done with care, as there will be but juft room for him befide the rope. He will meet the fledge exactly on the middle of the road, from which he muft ftep afide to pafs the fledge. Let the time of this experiment be noted. It is obvious that the boy and the fledge move with equal velocity, there is therefore no mechanical advantage obtained by the pulleys. The weight that he can draw will be about half a hundred, if he weigh about 9 ftone; but the exact force with which the boy draws is to be known by Experiment 1.

The Wheel and Axle.

This organ is ufually called in mechanics, *The axis in peritrochio.* A hard name, which might well be fpared, as the word windlafs or capftan would convey a more diftinct idea to our pupils.

Experiment 6. To the largeft drum, Fig. 1. faften a cord, and pafs it through the pulley P downwards, and then through the pulley P 4 to the fledge placed at the end of the wooden road, which is fartheft from the machine. Let the boy, by a rope faftened to the extremity of one of the arms of the capftan, and paffed over his fhoulder, draw the capftan round; he will wind the rope round the drum, and draw the fledge upon its road. To make the fledge advance twenty-four feet upon its road, the boy muft have walked circularly 144 feet, which is fix times as far, and he will be able to draw about three hundred weight, which is fix times as much as in the laft experiment.

It may now be pointed out, that the difference of fpace, paffed through by the power in this experiment, is exactly equal to the difference of weight, which the boy could draw without the capftan.

Experiment 7. Let the rope be now attached to the fmaller drum; the boy will draw nearly twice as much weight upon the fledge as before, and will go through double the fpace.

Experiment 8. Where there are a number of boys, let five or fix of them, whofe power of drawing (eftimated as in Experiment I.) amounts to fix times as much as the force of the boy at the capftan, pull at the end of the rope which *was* faftened to the fledge; they will balance the force of the boy at the capftan: either they, or he, by a fudden pull may

advance,

advance, but if they pull fairly, there will be no advantage on either part. In this experiment the rope fhould pafs through the pulley P 3, and fhould be coiled round the larger drum. And it muft be alfo obferved, that in all experiments upon the motion of bodies, in which there is much friction, as where a fledge is employed, the refults are never fo uniform as in other circumftances.

<center>*The Pulley.*</center>

Upon the pulley we fhall fay little, as it is in every body's hands, and experiments may be tried upon it without any particular apparatus. It fhould, however, be diftinctly inculcated, that the power is not increafed by a fixed pulley. For this purpofe, a wheel without a rim, or, to fpeak with more propriety, a number of fpokes fixed in a nave fhould be employed, (Fig. 9.) Pieces like the heads of crutches fhould be fixed at the ends of thefe fpokes, to receive a piece of girth-web, which is ufed inftead of a cord, becaufe a cord would be unfteady; and a ftrap of iron with a hook to it fhould play upon the center, by which it may at times be fufpended, and from which at other times a weight may be hung.

Experiment 9. Let this fkeleton of a pulley be hung by the iron ftrap from the tranfom of the frame; faften a piece of web to one of the radii, and another to the end of the oppofite radius. If two boys of equal weight pull thefe pieces of girth-web, they will balance each other; or two equal weights hung to thefe webs will be in equilibrio. If a piece of girth-web be put round the uppermoft radius, two equal weights hung at the ends of it will remain immoveable; but if either of them be pulled, or if a fmall additional weight be added to either of them, it will defcend, and the web will apply itfelf fucceffively to the afcending radii, and will detach itfelf from thofe that are defcending. If this movement be carefully confidered, it will be perceived, that the web in unfolding itfelf, acts in the fame manner upon the radii as two ropes would if they hung to the extremities of the oppofite radii in fucceffion. The two radii which are oppofite, may be confidered as a lever of the firft fort, where the center is in the middle of the lever; as each end moves through an equal fpace, there is no mechanical advantage. But if this fkeleton-pulley be employed as a common *block* or *tackle,* its motions and properties will be entirely different.

Experiment 10. Fig. 9. Nail a piece of girth-web to a poft, at the diftance of three or four feet from the ground; faften the other end of it to one of the radii. Faften another piece of web to the oppofite radius, and let a boy hold the fkeleton-pulley fufpended by the web; hook weights to the ftrap that hangs from the center. The end of the radius to which the fixed girth-web is faftened will remain immoveable; but, if the boy pulls the web which he holds in his hand upwards, he will be able to lift nearly double the weight, which he can raife from the ground by a fimple rope, without the machine, and he will perceive that his hand moves through twice as great a fpace as the weight afcends: he has therefore the mechanical advantage, which he would have by a lever of the fecond fort, as

<div align="right">in</div>

in experiment III. Let a piece of web be put round the under radii, let one end of it be nailed to the poft, and the other be held by the boy, and it will reprefent the application of a rope to a moveable pulley; if its motion be carefully confidered, it will appear that the radii, as they fucceffively apply themfelves to the web, reprefent a feries of levers of the fecond kind. A pulley is nothing more than an infinite number of fuch levers; the cord at one end of the diameter ferving as a fulcrum for the *organ* during its progrefs. If this *fkeleton-pulley* be ufed horizontally inftead of perpendicularly, the circumftances which have been mentioned will appear more obvious.

Upon the wooden road lay down a piece of girth-web; nail one end of it to the road; place the pulley upon the web at the other end of the board, and bringing the web over the radii, let the boy, taking hold of it, draw the loaded fledge faftened to the hook at the center of the pulley: he will draw nearly twice as much in this manner as he could without the pulley *.

Here the web lying on the road fhews more diftinctly, that it is quiefcent where the loweft radius touches it; and if the radii, as they tread upon it, are obferved, their points will appear at reft, whilft the center of the pulley will go as faft as the fledge, and the top of each radius fucceffively (and the boy's hand which unfolds the web) will move twice as faft as the center of the pulley and the fledge.

If a perfon, holding a ftick in his hand, obferves the relative motions of the top, and the middle, and the bottom of the ftick, whilft he inclines it, he will fee that the bottom of the ftick has no motion on the ground, and that the middle has only half the motion of the top. This property of the pulley has been dwelt upon, becaufe it elucidates the motion of a wheel rolling upon the ground; and it explains a common paradox, which appears at firft inexplicable, " The bottom of a rolling wheel never moves *upon* the road." This is afferted only of a wheel moving over hard ground, which, in fact, may be confidered rather as laying down its circumference upon the road, than as moving upon it.

The inclined Plane and the Wedge.

The *inclined plane* is to be next confidered. When a heavy body is to be raifed, it is often convenient to lay a floping artificial road of planks, up which it may be pufhed or drawn. This mechanical power, however, is but of little fervice without the affiftance of wheels or rollers: we fhall therefore fpeak of it as it is applied in another manner, under the name of *the wedge*, which is in fact a moving inclined plane; but if it is required to explain the properties of the inclined plane by the Panorganon, the wooden road may be raifed and fet to any inclination that is required, and the fledge may be drawn upon it as in the former experiments.

* In all thefe experiments with the fkeleton-pulley fomebody muft keep it in its proper direction; as from its ftructure, which is contrived for illuftration, not for practical ufe, it cannot retain its proper fituation without affiftance.

Let

Let one end of a lever, Fig. 7. with a wheel at one end of it, be hinged to the poſt of the frame, by means of a gudgeon driven or ſcrewed into the poſt. To prevent this lever from deviating ſideways, let a ſlip of wood be connected with it by a nail, which ſhall be faſt in the lever, but which moves freely in a hole in the rail. The other end of this ſlip muſt be faſtened to a ſtake driven into the ground at three or four feet from the lever, at one ſide of it, and towards the end in which the wheel is fixed, Fig. 10. which is a *vue d'oiſeau*, in the ſame manner as the treadle of a common lathe is managed, and as the treadle of a loom is ſometimes guided *.

Experiment 11. Under the wheel of this lever place an inclined plane or half-wedge, Fig. 7. on the wooden road, with rollers under it, to prevent friction †; faſten a rope to the foremoſt end of the wedge, and paſs it through the pulleys (P. 4. and P. 3.) as in the fifth experiment. Let a boy draw the ſledge by this rope over his ſhoulder, and he will find, that as it advances it will raiſe the weight upwards; the wedge is five feet long, and elevated one foot. Now, if the perpendicular aſcent of the weight, and the ſpace through which he advances be compared, he will find that the ſpace through which he has paſſed will be five times as great as that through which the weight has aſcended; and that *this* wedge has enabled him to raiſe five times as much as he could raiſe without it, if his ſtrength were applied, as in Experiment I, without any mechanical advantage. By making this wedge in two parts hinged together, with a graduated piece to keep them aſunder, the wedge may be adjuſted to any given obliquity; and it will be always found, that the mechanical advantage of the wedge may be aſcertained by comparing its perpendicular elevation with its baſe. If the baſe of the wedge is 2, 3, 4, 5, or any other number of times greater than its height, it will enable the boy to raiſe reſpectively 2, 3, 4, or 5 times more weight than he could do in Experiment I, by which his power is eſtimated.

The Screw.

The ſcrew is an inclined plane wound round a cylinder; the height of all its revolutions round the cylinder taken together, compared with the ſpace, through which the power that turns it paſſes, is the meaſure of its *mechanical advantage* ‡. Let the lever, uſed in the laſt experiment, be turned in ſuch a manner as to reach from its gudgeon to the ſhaft of the Panorganon, guided by an attendant lever as before, Fig. 8. Let the wheel reſt upon

* In a loom this ſecondary lever is called *a lamb*, by miſtake, for *lam*; from *lamina*, a ſlip of wood.

† There ſhould be three rollers uſed; one of them muſt be placed before the ſledge, under which it will eaſily find its place, if the bottom of the ſledge near the foremoſt end is a little ſloped upwards. To retain this foremoſt roller in its place till the ſledge meets it, it ſhould be ſtuck ſlightly on the road with two ſmall bits of wax or pitch.

‡ *Mechanical advantage* is not a proper term, but our language is deficient in proper technical terms. The word *power* is uſed ſo indiſcriminately, that it is ſcarcely poſſible to convey our meaning, without employing it more ſtrictly.

the

the lowest *helix* or thread of the screw ; as the arms of the shaft are turned round, the wheel will ascend, and carry up the weight which is fastened to the lever *. As the situation of the screw prevents the weight from being suspended exactly from the center of the screw, proper allowance must be made for this in estimating the force of the screw, or determining the mechanical advantage gained by the lever : this can be done by measuring the perpendicular ascent of the weight, which in all cases is better, and more expeditious, than measuring the parts of a machine, and estimating its force by calculation ; because the different diameters of ropes, and other small circumstances, are frequently mistaken in estimates.

The space passed through by the moving power, and by that which it moves, are infallible data for estimating the powers of engines. Two material subjects of experiments yet remain for the Panorganon ; friction, and wheels of carriages. We repeat, that it is not intended in this, or in any other part of our design, to write treatises upon science ; but merely to point out methods of initiating young people in the rudiments of knowledge, and of giving them a clear and distinct view of those principles upon which they are founded. No preceptor, who has had experience, will cavil at the superficial knowledge of a boy of twelve or thirteen upon these subjects ; he will perceive, that the general view, which we wish to give our pupils of the useful arts and sciences, must certainly tend to form a taste for literature and investigation. The *sciolist* has learned only to *talk*—we wish to teach our pupils to *think*, upon the various objects of human speculation.

The Panorganon may be employed in trying the resistance of air and water ; the force of different muscles ; and in a great variety of amusing and useful experiments. In academies, and private families, it may be erected in the place allotted for amusement, where it will furnish entertainment for many a vacant hour. When it has lost its novelty, the shaft may from time to time be taken down, and a swing may be suspended in its place. It may be constructed at the expence of five or six pounds : that which stands before our window was made for less than three guineas, as we had many of the materials beside us for other purposes.

* In this experiment, the boy should pull as near as possible to the shaft, within a foot of it, for instance, else he will have such mechanical advantage as cannot be counterbalanced by any weight which the machine would be strong enough to bear.

IV. *Experiments*

IV.

Experiments and Observations on the Light which is spontaneously emitted, with some Degree of Permanency, from various Bodies. By NATHANIEL HULME, M. D. F. R. S. and A. S.

(Concluded from page 427.)

SECTION IV.

Other Bodies or Substances have a Power of preserving spontaneous Light for some Time, when it is applied to them.

EXPERIMENTS.

EXPER. 1. Some luminous matter scraped from the herring, was mixed with a solution of two drams of Epsom salt in two ounces of cold pump water: after shaking very well for some time the phial which contained them, the whole liquid became richly impregnated with light, and continued shining above twenty-four hours. This experiment was frequently repeated, and with the same effect.

Exper. 2. Two drams of Glauber's salt and two ounces of water being mixed with herring light, the solution was thereby quickly made very lucent, and remained so until the succeeding evening.

Exper. 3. Mackerel-light, being mixed with two drams of Rochelle salt or tartarized natron and two ounces of water, caused the fluid to be very luminous.

Exper. 4. Two drams of soda phosphorata and two ounces of water, mixed with herring-light, formed a very lucent fluid, which retained the light for a long time.

Exper. 5. Herring-light, with one dram of saltpetre or nitrated kali and two ounces of water, made the solution pretty luminous.

Exper. 6. Half a dram of common salt dissolved in two ounces of water, with the addition of mackerel-light, composed a very shining mixture, which retained its splendour for the space of a day or two. The same effect was produced by herring-light.

Exper. 7. Two ounces of sea water, being agitated with the light of a mackerel, soon obtained a brilliant illumination. The sea water preserved its luminousness for several days. The experiment was successfully repeated.

Exper. 8. Two drams of pure honey, that had not been clarified, or exposed to heat, were dissolved in two ounces of water; and, after the admission of some mackerel-light, and shaking the phial, the solution was fully impregnated with light, which was visible the next evening.

Exper.

Exper. 9. Two drams of purified or refined fugar being diffolved in two ounces of water, and mixed with the fhining matter of a herring, the fluid acquired a great degree of luciduefs. The fame effect took place when the experiment was made with foft brown fugar.

N. B. It is almoft needlefs to mention, that the degree of illumination in thefe liquids muft depend upon the quantity of lucific matter applied; but, in general, as much as can be fcraped off by the blunt point of a moderately-fized knife, at a few times, will be fufficient, being affifted by a ftrong agitation of the containing phial.

OBSERVATION.

Thefe experiments enable us to take light and diffufe it through water, fo as to render the whole liquid moft brilliantly luminous, or, in other words, to impregnate water with light. By thefe means, the light is fo extended in its furface, and combined in fuch a manner, as to become exceedingly convenient and ufeful for various other experiments.

SECTION V.

When fpontaneous Light is extinguifhed by fome Bodies or Subftances, it is not loft, but may be again revived in its former Splendour, and that by the moft fimple Means.

EXPERIMENTS.

Exper. 1. On the 1ft of June, 1795, the following experiments were made, to know what was the beft proportion of Epfom falt to water, in order to produce the moft luminous liquid. Some fhining matter was taken from a mackerel, and mixed with a folution of feven drams of the falt in one ounce of water; and its light was immediately extinguifhed. The fame effect enfued, but in a lefs degree, with a folution of fix, and one of five drams. In a folution of two drams, in the fame quantity of water, the liquid was luminous; but much more fo when only one dram of falt was ufed. Obferving the extinction of light to take place, as above, in the more faturated folutions, while the diluted folutions were luminous, it occurred to me to endeavour to difcover what became of the extinguifhed light, in the former cafe, and whether it might not be revived by dilution. For this purpofe, I took the folution of feven drams of falt in one ounce of water, in which the lucid matter from a mackerel had been extinguifhed, and diluted it with fix ounces of cold pump water; when, to my great aftonifhment, light in a moment burft out of darknefs, and the whole liquid became beautifully luminous! This revived light remained above 48 hours, that is, as long as other light in general does, which has never been extinguifhed. Hence, it had loft nothing of its vivid luminous powers by its extinction.

Exper.

Exper. 2. The laſt experiment was then reverſed. A ſolution of one dram of Epſom ſalt in one ounce of water, was brilliantly illuminated with mackerel light. Then, ſix drams of the ſalt were put into this luminous liquid; and, after ſhaking the phial very well for a little time, to promote the ſolution of the ſalt, the light was totally extinguiſhed. But the ſame light was again recovered, by the addition of ſix ounces of water.

In this manner the light may be frequently extinguiſhed, and as often revived. In one inſtance, the ſame light, by a repetition of this method, was made to undergo ten extinctions.

Exper. 3. A good quantity of herring-light, being mixed with a ſolution of four drams of common ſalt in two ounces of water, was immediately extinguiſhed. Then, fourteen ounces of cold pump water were added thereto, and the whole liquid was at once finely illuminated. On the next evening it appeared ſtill very lucid; and likewiſe on the ſucceeding night.

Exper. 4. The experiment was reverſed. Half a dram of the ſalt, being diſſolved in two ounces of water, had herring-light mixed therewith, ſo as to be made very luminous. On the addition of two drams more of the ſalt, the lucidneſs wrs inſtantly deſtroyed: but the light was again recovered, by pouring eight ounces of cold water upon the extinguiſhed luminous fluid. The revived light was very vivid the next evening.

Exper. 5. Two ounces of ſea water were illuminated with mackerel-light, and then extinguiſhed by adding two drams of common ſalt. The light was again reſtored, by diluting the ſolution with eight ounces of cold ſpring water.

N. B. If the illuminated liquid be uncommonly brilliant, it may ſometimes require more ſalt to extinguiſh the light completely, than is here ſpecified; in that caſe, the meaſure of water for dilution, muſt be always calculated in exact proportion to the weight of ſalt employed.

SECTION VI.

Spontaneous Light is rendered more vivid by Motion.

EXPERIMENTS.

Exper. 1. A quantity of illuminated liquid was poured into a broad veſſel, which was placed in the laboratory. The next evening, on examination, it appeared to be quite dark. But a finger, or rod, being drawn through it, was followed by a luminous line.

Exper. 2. A phial, containing a pretty large portion of liquid impregnated with light, having been at reſt a number of hours, the liquid ſeemed to have loſt its luminous quality, except a little glimmer floating at the top. It was then gently moved, and the light diffuſed itſelf gradually through the whole liquid: on agitation, the lucidneſs was much increaſed; and, the briſker the motion, the more vivid was the illumination.

SECTION

SECTION VII.

Spontaneous Light is not accompanied with any Degree of sensible Heat, to be discovered by a Thermometer.

EXPERIMENTS.

Exper. 1. A luminous herring, and another that was quite fresh and not luminous, were placed for a considerable time in the same degree of temperature. A thermometer was then applied to each of them, but no difference of heat could be discovered.

Exper. 2. The soft roe of a herring, in an exceedingly lucid state, and a thermometer, were kept together for some time in the laboratory. The roe was then put upon the bulb of the thermometer, without affecting it.

Exper. 3. A mackerel, which shone with very brilliant light, was also put to the test of a thermometer, but the instrument remained stationary.

Exper. 4. The bulb of a thermometer was surrounded by many small pieces of shining wood, uncommonly luminous, which were kept in that situation for some time; but the light made no alteration upon the thermometer.

Exper. 5. Illuminated liquids, and spring water, being kept together in the laboratory, always preserved the same degree of temperature.

SECTION VIII.

The Effects of Cold on spontaneous Light.

EXPERIMENTS.

The Light of Fishes.

Exper. 1. Five small gallipots, containing three pieces of soft-roe of herring, and two of the herring itself, all very luminous, were placed in a frigorific mixture, composed of snow and sea-salt; and, in about an hour and a half, the light was quite extinct, and the bodies totally frozen. The gallipots were then removed into a vessel of cold water, that their contents might be gradually thawed; which being done, they all recovered their pristine luminous state. The pieces were afterwards observed to shine during three succeeding nights.

Exper. 2. A small phial, containing three or four drams of liquid impregnated with light, was placed in a frigorific mixture. As the liquid froze, its lucidness gradually diminished; and, when it was quite congealed, the light perfectly disappeared. The phial was then taken out, and put into cold water, at about 49° temperature, that the ice might be gradually liquefied; and, when that was accomplished, the whole fluid became as luminous as before.

4

The Light of shining Wood.

Exper. 3. A fragment of shining wood was put into a small wide-mouthed phial, which was plunged into a frigorific mixture. As the cold affected the wood, the light gradually faded, and at last was totally imperceptible. The phial was then taken out, and placed in water at about 62°; by this change of temperature, the frozen wood gradually thawed, and then regained its former lustre.

The Light of Glow-worms.

Exper. 4. A small phial, containing a luminous dead glow-worm, was exposed to the cold of the frigorific mixture; as the coldness penetrated the phial, the light diminished, and at length was totally extinct. But, by placing the phial in water at about 62°, the glowing property of the infect soon returned. In this experiment, the glow-worm was evidently congealed; for it adhered to the side of the glass, and was covered with a hoar-frost. This experiment was frequently repeated, and with the same result.

OBSERVATION.

By these experiments we learn, that cold extinguishes spontaneous light in a temporary manner, but not durably, as the substances of the third section do; because the light revived again in its full splendour, as soon as it was exposed to a moderate degree of temperature.

SECTION IX.

The Effects of Heat on spontaneous Light.

EXPERIMENTS.

The Light of Fishes.

Exper. 1. One side of a luminous herring was held before the fire, for a short space of time, but so as to receive its heat very strongly. It was then conveyed into the laboratory; when that side which had been exposed to the fire was found quite dark, but the other continued still luminous. The fish was preserved till the next evening, but the extinguished light did not re-appear.

Exper. 2. A whole herring, finely shining, was thrown into a quantity of boiling-hot water, and the light was immediately extinguished: after keeping it there for some time, it was taken out, but the light did not revive.

The

The Light of shining Wood.

Exper. 3. A piece of shining wood, its light being very faint, was put into tepid water at about 90 degrees of temperature, and it became in a short time much more lucid. Another piece, at 96°, was rendered beautifully luminous.

Exper. 4. A pretty thick piece of shining wood was put into a gallipot, and sunk under water by means of a weight, together with a thermometer, at the temperature of 64°. Boiling-hot water was then added by spoonfuls; and the light, at first, was rendered much more vivid, but soon after began to decrease, and was apparently extinct at about 110°. I say apparently, because on the next evening the light had somewhat revived; which shows, that the heat of 110° was not sufficient to extinguish totally all the light inherent in this piece of wood.

Exper. 5. Finding that 110 degrees of heat did not wholly extinguish the light of shining wood, a good many fragments, of different sizes, were then submitted to the power of boiling water, and detained therein for some time, in order that the heat might penetrate them thoroughly. The effect was, that the light became quickly extinct, and did not, as before, re-appear on the following evening.

The Light of Glow-worms.

Exper. 6. A dead shining glow-worm was put upon two ounces of water, contained in a wide-mouthed phial, at the temperature of 58°. The phial was then sunk, about two or three inches deep, in boiling-hot water; and, as the heat communicated itself to the contents of the phial, the light of the glow-worm became much more vivid.

Exper. 7. Another lucid dead glow-worm was put into warm water, at 114°, to see if that degree of heat would extinguish the light; but, on the contrary, its glowing property was augmented. All the water was then poured off, yet the insect continued to shine for some length of time.

Exper. 8. The effect of that heat which is obtained from dry solid bodies by friction, was next tried upon the light of the glow-worm. Two living glow-worms were put into a one-ounce phial, with a glass stopple; and, though they were perfectly dark at the time, yet, if the phial was briskly rubbed with a silken or linen handkerchief, till it became pretty warm, it seldom failed to make them display their light very finely. This experiment was very frequently repeated. It had the same illuminating effect upon the light of a dead glow-worm.

Exper. 9. The complete influence of 212 degrees of heat was now applied to the light of a glow-worm, by pouring upon one when dead, but in a luminous state, some boiling water. Its light was instantly extinguished thereby, and did not revive. The experiment was repeated, and with the same result.

Any

Any of the faline Solutions mentioned in the fourth Section, being impregnated with luminous Matter, and left fome Time at reft, are rendered more lucid by a moderate *Degree of Heat.*

Exper. 10. A quantity of illuminated folution was depofited in the laboratory. The next evening, when it was examined, it appeared in a manner quite dark; but, by putting the phial which contained it into hot water, the light revived, and was foon rendered exceedingly vivid.

Exper. 11. About a pint of folution impregnated with light, had become obfcure, by time and reft, as is the nature of this mixture. Such a quantity of boiling-hot water was then added to it, as only to give it a fmall degree of warmth, and it quickly caufed it to appear luminous.

Exper. 12. Illuminated liquid, to the quantity of four ounces, was placed in the laboratory until the next evening, when it had become almoft dark. One fpoonful of boiling-hot water being put into it, the light re-appeared; and, by means of two more, it was rendered confiderably lucid.

Their Light is extinguifhed by a great *Degree of Heat.*

Exper. 13. Some boiling water being poured upon three or four ounces of illuminated liquid, in an earthen veffel, the light was immediately extinguifhed; and, though afterwards kept a confiderable time for infpection, and often agitated, to ftir up the hidden light, yet no remains of any fhining property could be perceived. This experiment was frequently repeated, and always with the fame refult.

Exper. 14. Four ounces of very luminous liquid, together with a thermometer, were put into a fmall earthen veffel, glazed white, the better to reflect light. Boiling-hot water was then added, by fpoonfuls at a time, and by flow degrees. The firft few fpoonfuls made it confiderably more lucid; and then, by adding more, the light began to fade, and at length was gradually extinguifhed. This effect took place, in one inftance, when the liquid was heated to 96°; in another, to 98°; and in a third, to 100°. Hence, this fpecies of light, when thus united with water, feems to be extinguifhed at from 96 to 100 degrees of heat. This is a very elegant and pleafing method, of knowing how much heat is required to extinguifh the light; becaufe it meafures it exactly, provided the hot water be added in fmall quantities, and by flow degrees, as above directed. To prevent the poffibility of any light reviving after an experiment of this kind, would require a much greater heat than that of 100°. The intention of the prefent experiment was only to fhow, that all light may be apparently extinguifhed, at fo low a degree of temperature as from 96° to 100°.

Exper. 15. A phial of an ounce and a half was filled with fome very luminous liquid, but not corked. It was then fufpended by a ftring, in a quart of boiling-hot water

contained in a white earthen mug, and the light was wholly extinguifhed in about three or four minutes. After this, the phial was kept in the water fome time longer, was then taken out to cool, and well fhaken, but the light did not revive. It was examined the next day, and agitated again, but no luminous appearance could be difcovered; a proof that all the light had been totally extinguifhed by the power of heat.

If much Heat be applied to the Bottom of a Tube filled with illuminated Liquid, which has been fome Time at reft, the Light will defcend in luminous Streams, from the Top of the Tube to the Bottom, and be gradually extinguifhed.

Exper. 16. A glafs cylindrical tube, clofed at one end, being 9 inches long, with a bore of $1\frac{1}{10}$ inch, when ufed, was put into a gallipot $3\frac{1}{2}$ inches deep, and $3\frac{1}{2}$ wide, which held about 12 ounces of boiling water, and was placed in another larger veffel, to receive the overflowing water upon the immerfion of the tube. The tube being filled over night with fome very luminous liquid, was placed in the laboratory until the next evening. The light had then afcended plentifully to the top of the fluid, (the reft being dark,) and, taking the circular fhape of the tube, formed a very lucid ring. The veffels with the boiling-hot water were then carried into the dark laboratory; and the tube being gently and carefully placed (without fhaking) in the gallipot, the light was, generally in about half a minute, feen plainly to defcend in ftreams from the top to the bottom; illuminating the whole fluid in its defcent in a beautiful manner, and then was gradually extinguifhed. The extinction of the light began at the top of the tube, and ended at the bottom.

Exper. 17. The experiment was alfo made with a tube 19 inches high, $\frac{1}{2}$ an inch in bore, having feveral curvatures, and fealed hermetically at its lower end. Both extremities were made ftraight for a few inches; the one to be immerfed in the water, and the other to prevent the liquid running out. The luminous ring being formed as above mentioned, the tube was put into the gallipot of boiling-hot water; and, in a fhort time, the light began to defcend from the top, and came waving down, in a pleafing manner, to the bottom of the tube in the hot water, and then was by degrees extinguifhed. The whole length of the tube, including the curvatures. was 26 inches.

The moft eligible folutions for this curious experiment, are thofe made with Epfom falt, Glauber's falt, fea-falt, and fal ammoniac: if either of the two former be ufed, the proper proportion is, one dram of falt to each ounce of water; if either of the two latter, 15 grains to each ounce of water will be fufficient.

N. B. The experimentalift, before he views the defcent of the light in the tube, fhould always remain in the dark for fome little time, in order to get rid of all extraneous light adhering to the organs of vifion, and to accommodate the eye to darknefs.

SECTION

SECTION X.

The Effects of the human Body, and of the animal Fluids, upon fpontaneous Light.

The living Body.

Exper. 1. On touching the luminous matter of fifhes, the light adhered to the fingers and different parts of the hands; remained very lucid for fome little time, and then gradually difappeared. But the fame kind of matter heing applied to pieces of wood, ftone, and the like, of the fame temperature as the laboratory, continued luminous on thefe fubftances for many hours.

Exper. 2. A piece of red blotting-paper, about one inch fquare, and four times doubled, was finely illuminated by matter from a herring, and applied to the upper part of the infide of the thigh. After the expiration of 15 or 20 minutes, it was taken off; and, on examination, the light was quite extinguifhed. The experiment was repeated feveral times, and with the fame effect. Another piece of the like paper was illuminated at the fame time, and placed in the laboratory; where it retained its light above 48 hours.

Exper. 3. A piece of fhining wood was placed upon the palm of the hand, and inclofed therein for fome time; on infpection, it was found to be more lucid than before. Many trials of this kind were made, with the like fuccefs.

Exper. 4. A dead glow-worm, being but flightly luminous, was breathed upon feveral times; and its light increafed both in magnitude and brightnefs. The experiment was frequently repeated, with the fame refult.

Animal Fluids.

Blood.

Exper. 5. A perfon having received a contufion, but otherwife in health, was bled. The next day, fome herring-light was mixed with about two ounces of the craffamentum or red coagulated part of the blood, by ftirring them well together with a knife: it caufed it to be flightly luminous, but the light was not of long duration. Nearly the fame refult followed the mixture of lucid matter with the recent craffamentum of perfons labouring under inflammatory difeafes, as the pleurify and rheumatifm.

Exper. 6. But, when mixed with craffamentum that had been kept for fome time, and become black and fomewhat offenfive to the fmell, the light feemed to be more quickly extinguifhed.

Exper. 7. A fingular phænomenon happened feveral times, on mixing fifh-light with putrefcent bloody ferum. It would not incorporate, but was ejected in globules, like quickfilver when rubbed with any unctuous fubftance, and afterwards adhered to the fide of the veffel in which the mixture was made, in the form of a lucid ring.

3 N 2

Exper.

Exper. 8. The luminous matter of a herring was mixed with about two ounces of pure ferum, from the healthy fubject of the 5th experiment: it foon became finely illuminated, and retained its fhining appearance for a long time, whenever it was ftirred or agitated.

Exper. 9. The recent ferum, drawn from patients afflicted with inflammatory complaints, was illuminated pretty much in the fame manner as in the 8th experiment; and often retained light above 48 hours.

Urine.

Exper. 10. Mackerel-light being mixed, by ftrong agitation, with fome frefh urine from a healthy perfon, a glimpfe of light was retained at firft, and then was gradually extinguifhed. But ftale and pungent urine, being incorporated with luminous matter, had ftill greater extinguifhing effect.

Bile.

Exper. 11. Some bile, taken from a perfon who died of a fuppreffion of urine, had herring-light mixed with it, which foon became extinct. Another trial was made, with a different bile, and with the fame refult.

Milk.

Exper. 12. Human milk not being eafily obtained, fome mackerel-light was incorporated, by agitation, with two ounces of frefh cow's milk, which was thereby rendered finely luminous, and continued fhining above 24 hours. Frefh cream alfo retained fome light; though it was not fo vifible as with milk, owing probably to its thicknefs. But, when either milk or cream turn four, they contract a very extinguifhing property. A quart of milk was kept five days, in a moderately cool place, in the month of June; by that time, it was changed into a mixture fomewhat refembling curds and whey, that is, into a fmall fmooth coagulated part, and a very thin one, both which were acidulous. Some fine mackerel-light was mixed with two ounces of each of them, in feparate phials, and they extinguifhed it immediately.

V. *Obfervations*

·V.

Obfervations on the Theory of Electric Attraction and Repulfion. By the Rev. George Miller, *F. T. C. D.**

BEFORE that the theory of a fingle electric fluid was propofed, no difficulty occurred in the explanation of the attractions and repulfions obferved to arife from electricity. If we admit that there are two diftinct electric fluids, each of which ftrongly attracts the other, but confifts of particles mutually repulfive; it becomes eafy to account for the attraction fubfifting between bodies in different ftates of electricity, and the repulfion between thofe in the fame. But when Dr. Franklin †, obferving that a man, ftanding upon a non-conductor, could not electrify himfelf, but that he could electrify another perfon alfo ftanding upon a non-conductor, was induced to regard the operation of exciting electricity only as a transfer of one and the fame fluid from one body to another; it was found to be difficult to reconcile to the new theory the mutual repulfion of bodies in that ftate which is, according to this theory, denominated negative electricity. Dr. Franklin ‡ acknowledged that he could not affign a fatisfactory reafon for it; and Dr. Prieftley § has propofed it, as one of the queries remaining to be folved for completing the fcience of electricity. Many attempts have been made to obviate this apparent objection to the fimple theory of a fingle fluid; but the difficulty feems ftill to be as great as it was in the time of Franklin.

‖ Æpinus has applied a very elaborate fyftem of mathematical reafoning to the folution of electrical phænomena, and has adopted as the bafis of his theory, the fame opinion which Franklin had entertained concerning the nature of the electric fluid; but he has combined with this opinion other principles fo inadmiffible, that his reafonings cannot be regarded as juft explications of the phænomena. He has affumed, apparently without any other reafon than its importance to his conclufion, that the particles of all other fubftances repel each other. His fyftem muft therefore be confidered, not as a phyfical folution agreeable to the known laws of natural operations, but merely as an ingenious exercife of mathematical ability.

M. De Luc, who rejected the folutions of Æpinus has endeavoured to fupply the deficiency. ** Having remarked that the divergence of the balls of an electrometer, included in the receiver of an air-pump, is continually diminifhed during the progrefs of exhauftion; he confiders it as proved, that the caufe of all electrical movements, whether of attraction or of repulfion, is the action of the air. This principle he applies in the following manner. When two bodies are in fimilar ftates of electricity, either pofitive or negative, they will

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* Irifh Tranfactions, VII. 139.   † Dr. Prieftley's Hiftory of Electricity, p. 161.
‡ Dr. Prieftley's Hiftory of Electricity, p. 165.   § Ibid. p. 492.
‖ Journal de Phyfique, Dec. 1787.   ** Ibid. Juin 1790.

confpire

conſpire to modify, either by giving or receiving the electric fluid, the ſtate of the inter-
mediate air, whilſt that of the exterior air is only modified by either of them ſingly; and
therefore the ſtate of the exterior air will differ more from that of the electrified bodies,
than the ſtate of the intermediate air.   In this caſe he contends that a repulſion muſt take
place, becauſe each body muſt move towards that part of the ſurrounding medium, whoſe
electrical ſtate is moſt different from its own.   On the other hand, when bodies are in dif-
ferent ſtates of electricity, they will mutually counteract the changes, which they might
ſeparately produce in the ſtate of the intermediate air; but each will operate on the exte-
rior air without any compenſation,   In this caſe the ſtate of the intermediate air will con-
tinue to differ from that of each body as much as at the firſt inſtant, whilſt the ſtate of the
exterior air is ſeparately modified by each body according to its reſpective ſtate of electri-
city.   The two bodies therefore, moving towards that part of the ſurrounding medium,
whoſe electrical ſtate is moſt different from their own, will at the ſame time move towards
each other.

This theory very ingeniouſly avoids the difficulty of explaining the caſe of electrical re-
pulſion, by reſolving it into an attraction towards the ſurrounding medium.   It ſeems
however to be liable to two objections.   In the firſt place, inſtead of aſſuming unauthorized
principles with the preceding theory, it omits the conſideration of one whoſe exiſtence
ſeems to be aſcertained by experiments.   If a body be in either ſtate of electricity, it will
induce in an adjacent body the contrary ſtate, until it ſhall have come within a certain
diſtance.   This property, which has been aſcertained by various experiments, indicates a
repulſive force ſubſiſting between the portions of the electric fluid that belong to the adja-
cent bodies; and this theory makes no allowance for ſuch a repulſion.   The fundamental
principle of it is merely a diffuſion of the electric fluid, and is * thus ſtated by M. de Luc:
" the electric matter tends towards all ſubſtances, and the more ſtrongly in the ſame pro-
" portion in which they poſſeſs a ſmaller quantity."   In the ſecond place, it does not ap-
pear, when carefully conſidered, to afford any aſſiſtance towards the removal of the grand
difficulty, the mutual repulſion of bodies negatively electrified.   If two bodies negatively
electrified be placed at a ſmall diſtance, they will both, according to M. de Luc's explana-
tion, receive the electric fluid from the intermediate air, which will conſequently retain a
ſmaller portion than the ſurrounding atmoſphere.   From the law above-mentioned it
ſhould follow, that the redundant fluid of the exterior air ſhould by diffuſion be commu-
nicated both to the bodies and to the intermediate ſpace; but no reaſon appears, which
would induce us to ſuppoſe that the bodies themſelves ſhould recede to a greater diſtance.
M. de Luc does indeed endeavour to prove that ſuch a motion ſhould take place, but by an
experiment whoſe ſolution contradicts his own theory.   He ſuſpended by a ſilk thread a
large, but light, metallic ball, and preſented it in a ſtate of poſitive electricity to a body

* " La loi ſuivante ſuffit ſeule: La matière électrique tend vers toutes les ſubſtances, d'autant plus
" fortement, qu'elles en poſſèdent moins."   Journal de Phyſique, Juin 1790.

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negatively electrified. The former was attracted towards the latter until it arrived at a certain diſtance, at which it diſcharged its electricity. Hence he concluded, in general, that when a body has more of the electric fluid than the neighbouring bodies, and is leſs diſpoſed to reſiſt its own motion than to abandon the exceſs of its electric matter, it will move towards that place which contains leſs of this matter. But in this experiment he conſiders the two bodies as acting on each other at a diſtance without any reference to the intermediate air.

Mr. Cavallo *, in the laſt edition of his treatiſe on electricity, has obſerved, that the mutual repulſion of two bodies negatively electrified is ſtill ſuppoſed to contradict the theory of Franklin ; and has therefore deemed it neceſſary to obviate the objection by a very particular detail. For this purpoſe he has premiſed the following propoſitions : Prop. 1. No electricity can appear on the ſurface of a body, or no body can be electrified either poſitively or negatively, unleſs the contrary electricity can take place on other bodies contiguous to it. Prop. 2. There is ſomething on the ſurface of bodies, which prevents the ſudden incorporation of the two electricities, viz. of that poſſeſſed by the electrified body with the contrary electricity poſſeſſed by the contiguous air, or other ſurrounding bodies. Prop. 3. Suppoſing that every particle of a fluid has an attraction towards every particle of a ſolid ; if the ſolid be left at liberty in a certain quantity of that fluid, it will be attracted towards the common centre of attraction of all the particles of the fluid. To this laſt propoſition he has ſubjoined the two following corollaries : 1. † the ſame thing muſt happen, when the quantity of fluid is ſmaller than the bulk of the body ; 2. if the attraction of the particles of the fluid be exerted only towards the ſurface of the ſolid, the effect will be the ſame when the body is of a regular ſhape ; but the difference will in any caſe be inconſiderable.

With regard to the ſolution founded upon theſe principles it muſt be remarked, that it is not derived ſimply from a conſideration of the ſuppoſed nature of the electric fluid ; but from a mixed ſtatement of that nature and of properties aſſumed merely from experiments as matters of fact. The firſt and ſecond propoſitions expreſs thoſe properties, and, though the experiments to which the former refers, may be explained by aſcribing the phænomena to the repulſive nature of the fluid, yet the latter is aſſumed without any ſuch reference. " Without examining," ſays Mr. Cavallo, " the nature, the extent, and the laws of this " property in bodies, it will be ſufficient for the preſent purpoſe to obſerve, that the fact " is certainly ſo ; for otherwiſe a body could not poſſibly be electrified, or it would not " remain electrified for a ſingle moment." From theſe principles thus aſſumed, Mr. Cavallo deduces the exiſtence of atmoſpheres of contrary electricity exiſting in the air contiguous to the bodies ; and from the attractions which are thereby occaſioned he infers the apparent repulſion of the electrified bodies.

* Vol. III. p. 192.

† Of this corollary Mr. Cavallo does not appear to make any diſtinct application.

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If thefe atmofpheres be conceived to be formed by the repulfive nature of the fluid, fome allowance fhould be made for the mutual repulfion of the two redundant portions belonging to bodies pofitively electrified. This however feems to be neglected for the purpofe of explaining the repulfion of bodies negatively electrified. But the difficulty feems to be only changed. If the negative atmofphere adjacent to a body pofitively electrified be caufed by the repulfion of the redundant fluid of the body, it will be neceffary to fhew that this repulfion is overpowered by the attraction fubfifting between that redundant fluid and the portion of air thus deprived of a part of its electric fluid.

But the reality of thefe atmofpheres of contrary electricity may well be queftioned. It feems to require, that we fhould conceive a portion of air contiguous to each body to be permanently, during the mutual repulfion of the bodies in a ftate of electricity oppofite to that of the bodies. But * it is afcertained experimentally, that the air furrounding any electrified body acquires the fame electricity which had been poffeffed by the body, and retains it even after the removal of the body. This muft be fuppofed, agreeably to the known laws of electricity, to be communicated by the alternate attraction and repulfion of the adjacent particles of air. Each particle muft be firft attracted towards the body, and, when by contact it has acquired the electricity of the body, repelled from it. Inftead therefore of a permanent ftate of contrary electricity conftituting thefe fuppofed atmofpheres, each adjacent fpace muft be occupied by particles, fome of which are attracted and others repelled. The time requifite for thus reducing the electricity of the body to an equilibrium with that of the furrounding air, is fufficient for explaining the continuance of the electricity of the bodies, without the aid of the fecond propofition; and the firft propofition is deduced only from a confideration of bodies in a folid ftate.

Poffibly a more diftinct application of a principle, already in fome degree adopted both by Doctor Prieftley and Mr. Cavallo, may remove all the difficulties of this inquiry. At leaft I will hope, that it may lead to fuch a confideration of the queftion, as may fubject the merits of the theory itfelf to a fair and decifive difcuffion. This principle is faturation. † Doctor Prieftley has explained the communication of the redundant fluid of a body pofitively electrified to another, a part of whofe fluid had been previoufly expelled, by fuppofing that it was more ftrongly attracted by the other body, than by its own which had more than its natural fhare; and ‡ Mr. Cavallo has in the fame manner accounted for the mutual attraction of bodies in different ftates of electricity.

In applying this principle to the folution of electric phænomena three forces muft be confidered: 1ft. the attraction fubfifting between each body and its own portion of the electric fluid; 2dly, the attraction which may fubfift between each body and the portion of fluid belonging to the other; and 3dly, the repulfion fubfifting between the two portions of the electric fluid.

* Cavallo's Complete Treatife on Electricity, Vol. I. p. 326.
† Hiftory of Electricity, p. 253.   ‡ Vol. I. p. 109.

That

That the attraction fubfifting between two bodies in oppofite ftates of electricity may be explained, it is neceffary to confider previoufly the cafe of two bodies in their natural or ordinary ftate. In this cafe the force fubfifting between each body and its own portion of the electric fluid is not in a ftate of faturation, becaufe it muft be fufficiently ftrong to counterbalance the elafticity of the fluid. Each body is therefore ftill capable of being attracted by the fluid belonging to the other, and each portion of the fluid is alfo capable of fuch attraction. This force, if it fhould operate alone, would draw the bodies together; but the mutual repulfion of the two portions of the fluid tends to produce the oppofite effect. The quiefcence of the bodies proves the equality of thefe forces.

If two bodies in oppofite ftates of electricity be brought together, the body pofitively electrified cannot be attracted towards the remaining electric fluid belonging to the other, becaufe this body may be confidered as faturated with the fluid, and that portion of the fluid as faturated with folid matter. For the oppofite reafons an attraction will take place between the body negatively electrified and the fluid belonging to the former. It remains to be fhewn, that this attractive force may exceed the mutual repulfion of the two portions of fluid. It muft be obferved, that the repulfion remains the fame, becaufe the fum of the two quantities of fluid is not altered; whereas the attraction is augmented by the unequal diftribution of the fluid. The one body is charged with more fluid than that which its own attracting force is capable of retaining, and the redundant fluid will confequently be ftrongly impelled towards the other body, whofe attractive power is at the fame time increafed by the deficiency of its own portion of fluid.

In the cafe of two bodies fimilarly electrified the bodies may be either both pofitively, or both negatively electrified. When they are both pofitively electrified, they are both faturated with the electric fluid; and when they are both negatively electrified, both remaining portions of the electric fluid are reciprocally faturated with folid matter. In neither cafe therefore can any attraction take place between either body and the fluid belonging to the other. Confequently, the repulfion exifting between the two portions of the fluid muft operate without refiftance, and the two bodies be repelled from each other.

Should this folution of electric attraction and repulfion be admitted, it will perhaps alfo remove the difficulty of magnetic repulfion. In this part of philofophy it has been found difficult to explain the repulfion of the correfponding poles agreeably to the theory of a magnetic fluid. In every magnetical body the equilibrium of this fluid is fuppofed to be difturbed, and one part of the body is conceived to be overcharged with the fluid, whilft the other is undercharged. The difficulty was to explain the repulfion of the undercharged poles, as in electricity to explain the repulfion of bodies negatively electrified. Mr. Kirwan has indeed, in a Memoir contained in the Sixth Volume of the Tranfactions of the Academy, referred the phænomena of magnetifm to cryftallization; but his mention of the term *faturated* in that Memoir feems to imply, that he does not mean to exclude the fuppofition of a magnetic fluid. If this be adopted, the preceding folution may be applied to the phenomena of magnetifm, in the fame manner in which it has been already applied to thofe of electricity.

The theory, according to which the preceding folution has been propofed, fuppofes the electric fluid a *fingle* fluid ; but it is not neceffary that it fhould be conceived to be abfolutely *fimple*. We know, for inftance, that atmofpheric air is a combination of at leaft two diftinct fluids ; and yet explain the phænomena of the barometer, air-pump, and condenfer, as depending merely on its prefence or abfence, without any reference to the compofition of its nature. In the fame manner fome electric phænomena may be juftly explained by confidering them as the effects of the different diftribution of the fame fluid ; whilft its phofphoric fmell, its power of changing blue vegetable colours to red, and its combuftion may poffibly be derived from its decompofition.

VI.

Defcription of a new Rotatory Engine for raifing Water, and for other Purpofes: By a Correfpondent.

To Mr. NICHOLSON.

SIR,

I SUBMIT to your judgment, and fhall be much gratified by your opinion on the engine of which I fend you the inclofed drawing. If you think it may be intitled to a place in your collection, either as an apparatus, or as affording fome hints to the improvement of hydraulics, the whole of my views refpecting it will be anfwered. I will confefs to you, that I have not conftructed it upon a fcale of actual work, and am well aware how many fubordinate points of organization require to be fettled, before any mechanical contrivance can obtain its beft and moft effective form : but as it has already been the fubject of a confiderable degree of meditation, I have ventured to offer it to you in its prefent ftate.

I am, Sir,

Your obliged Servant,

London, Dec. 14, 1800.   O. B.

DESCRIPTION.

In Plate XX. the upper figure reprefents an horizontal fection of the main part of the engine, and the lower figure fhews the fame engine, as it would appear to an obferver viewing it at right angles to the former direction of fight : the fame letters in both denoting the fame parts. A A reprefents an elliptical veffel of wood, or rather of metal, having its top and bottom flat, and joined to the fides by flanches or borders. The fides are upright, and may be of any height according to the intended capacity. In this veffel revolves the cylinder B, B, having the fame axis, and touching the internal circumference at two
oppofite

oppofite lines, where the place of contact is made water tight by leathering, packing, or any of the ufual methods. At oppofite parts of the furface of the cylinder are two flaps C, C, which are urged outwards by fprings, which caufe them to bear againft the interior furface of the ellipfis, and when preffed down they fall into cavities in the cylinder, fo as to complete the defective portion of its face. It muft be underftood that thefe flaps, as well as the cylinder itfelf, are of the height to fill up the interval between the flat faces of the top and bottom, and are fecured fo as to be water tight. Laftly, E E are two pipes leading downwards to the water intended to be raifed, and F F are two pipes leading upwards to the refervoir, or receptacle, to which it is to be conveyed. The firft mover is applied at the extremity D of the axis.

The action. Let us fuppofe an engine of this conftruction to be fixed on board a fhip centrically beneath one of the capftans, and that by the common contrivance of a gear bar, or other equivalent piece, the lower extremity of the axis of the capftan is connected with that of the machine, and a convenient number of men fet to work it in the direction H G. The immediate confequence will be, that the fpaces H, H, behind the flaps C, C, will be enlarged, and as they are air tight, the preffure of the atmofphere will drive water from the well through the pipes E E into thofe fpaces. As the flaps C proceed towards F, they will be preffed in by the elliptical concavity, and at laft the fpaces G G will difappear, and the greateft part of the fpaces H H (then conftituting the whole internal part of the veffel) will be nearly full of water. By the continuance of the procefs the two flaps will have paffed the places of contact, and begin to open on the oppofite fides of the ellipfis, driving before them the water and air up the pipes F F; at the fame time that the fpaces H H become again enlarged, and draw a frefh fupply of water through E, E. A conftant and almoft equable ftream is thus produced through the apparatus, which has this advantage, that by increafing the number of men at the capftan, the velocity of this current may be increafed, fo as to equal, as it fhould feem, any exigency the ftate of the veffel might produce.

If the fuction pipes were occafionally led into the fea, and the forcing pipes into a moderate fized air veffel, like that of the fire engine, a very powerful ftream of water might be directed to any part of the fhip, in cafe of the dreadful accident of fire; or for the common purpofes of cleanlinefs of the births of men or animals, upon which fo much of their health and comforts depend.

Shall I extend my fpeculations farther, and exprefs my conjecture, that this might form no contemptible fteam engine, if fteam were admitted through E into H, and condenfed fo as to render the preffure of the atmofphere active in G upon the flap, when the condenfation were made; or that it might receive the action of a defcending ftream of water, and work a mill, or other ufeful machine, &c. &c. Or fhall I not rather leave this vifionary region of fanciful indulgence, and commit the fate of my engine at once to the cenfure or praife of yourfelf and readers?

O. B.

*** Many

*** Many engineers have exercifed their talents in contriving machines which fhould raife water without the alternate action of the pump ; but I do not recollect having feen any conftruction precifely the fame as that of my correfpondent. In the *Recueil des Machines et Inventions approuvées par l'Academie Royale des Sciences*, tom. I. page 103, there is a machine by the celebrated M. Amontons, for raifing water by a drum or cylinder, fixed with its axis horizontal, and an ellipfis revolving in it againft two flaps or valves attached to the cylinder ; the effect of which was to afford two pair of variable fpaces fimilar to the engine before us. It is dated 1699.

Mr. Thomas Dickinfon in 1790 obtained a patent for a new engine on a rotatory principle, of which the contrivance fhews a mind habituated to mechanical refearch and invention. The principal organ confifts of a cylinder, in which another eccentric cylinder revolves, leaving a gibbous fpace on one fide, and out of this laft cylinder iffues two fliders croffing each other, and fo contrived as to length and other expedients, that they fweep the cavity, and afford variable fpaces for the introduction and extrufion of water. The drawings may be feen in the fecond Volume of the Repertory of Arts, and the whole differs very much from the invention of my correfpondent.

The action of fteam againft flaps or valves between two concentric cylinders, forms part of fome imperfect defcriptions in the fpecifications of the celebrated James Watt, and there is alfo a contrivance of this kind loofely defcribed in the firft volume of the Irifh Tranfactions, by John Cooke, Efq.

I fhall not attempt to difcufs the relative values of thefe inventions, either with regard to each other, or to the engines more generally ufed. The advantage of an inceffant or continued motion is admitted by all mechanics, and might be eafily difplayed. But in engines like every one of the foregoing, the difficulties of ftuffing or packing the parts which move in contact, and the confiderable friction and wear they muft be fubject to, unlefs improved far beyond their prefent ftate, are fo great, that I think few practical men would venture to undertake them. I do by no means prefume to fay, that they are on that account ufelefs. Undoubtedly they may be claffed at leaft with thofe products of the underftanding, by which our knowledge and mental habits are improved, and which form a ftock whence the moft ftriking and unforefeen difcoveries are occafionally drawn. Sheep were marked, loadftones were examined, and the covers of kettles were blown off many ages before the art of printing, the mariner's compafs, and the fteam engine were thought of.

<div align="right">W. N.</div>

VI.

Account of a new Method of bleaching Cotton, as publifhed by Chaptal, Member of the National Inftitute. By *J. C.* DELAMETHERIE *.

THE fuccefsful experiments made by Berthollet in bleaching vegetable cloth, by means of the oxygenated muriatic acid, feem to have brought this art nearly to a ftate of perfection; but this method is not in every inftance equally œconomical: it requires to be performed by fkilful operators, in order that the goods may not be affected by a ley too corrofive, or applied at an improper time, independent of which confideration, it is defirable that every procefs fhould be completely difclofed, in order that the artift may choofe fuch means as may be beft fuited to his purfuits.

This confideration has induced me to defcribe a very fimple and œconomical procefs for bleaching cotton thread; it is as follows:

At the height of about four decimetres (18 inches) and an half above the grate of a common furnace, a copper boiler is placed, of a round form, five decimetres (20 inches) in depth, and one metre and a third, (3½ feet) in diameter. The projecting rim of the cauldron, which is about two decimetres (8 inches) refts upon the brick work of the furnace. The remainder of the kiln is raifed of free ftone, and forms an oval boiler or digefter about two metres (6 feet and a half) in height, and its width, when meafured at the centre, is about one metre and a third, or 3½ feet.

The upper part of this veffel has a round orifice about half a metre (19½ inches) in diameter, which is clofed, when neceffary, by a large moveable ftone, or by a copper lid, adapted for the purpofe. On the flanch of the copper veffel, which forms the bottom of this kind of digefter, a grating is laid, which confifts of bars of wood, placed near enough to prevent the cotton that is put on them from falling through, and fufficiently ftrong to fupport the weight of eight hundred kilogrammes, (or 1½ cwt.)

When this ftructure is completed, the cotton having been previoufly divided into hanks or parcels is flightly impregnated with a folution of foda, rendered cauftic by the addition of lime. This operation is performed in a trough of wood or ftone, in which the cotton is worked by men with their feet defended with wooden fhoes. As foon as the cotton is fufficiently impregnated with the alkaline liquor, it is conveyed to the digefter, and piled upon the wooden grate. In this fituation the exfuding liquor runs through the bars into the copper boiler, where it forms a ftratum of fluid, and which allows the whole mafs to be heated without danger of burning either the cotton or the metal. The alkaline ley is compofed of Alicant foda, in quantity one tenth part of the weight of the cotton, and in a veffel of the dimenfions above defcribed, there may be employed at each time, forty myria-

* Journal de Phyfique, Vendemaire, an. 9, page 305.

grammes

grammes (about 800 French pounds, or 880 avoird.) of cotton. The denfity of the ley is in general about two degrees, (qu. by what inftrument?) After the cotton is properly difpofed in the boiler, the cover is put on, and fcarcely any iffue left for the vapours difengaged by the heat, in order that they may acquire a more confiderable degree of heat, and re-act with increafed force upon the cotton. When the digefter is charged, the fire is lighted in the furnace *, and the ley fubmitted to a gentle ebullition from twenty to thirty-fix hours. It is then left to cool, the cover is taken off, the cotton carefully wafhed, and expofed in the bleaching ground for two or three days, by fpreading it on frames during the day, and fpreading it on the grafs at night. Thus the cotton acquires a beautiful degree of whitenefs; and if by accident fome portions of the fkains fhould remain unbleached, they are replaced in the boiler, and fubjected to a fecond operation, or otherwife they are left in the field for feveral days longer. Thefe fhades in the bleached cotton are the confequence of its not having been completely and equally impregnated with ley; but they may alfo proceed from too clofe preffure in certain parts, at the time of ftowing it in the boiler. When the ley is fuppofed to be exhaufted by ebullition, the boiler is uncovered, and the cotton wetted with an additional quantity of the folution of foda; for unlefs this precaution is taken it may eafily burn.

From thefe particulars it would be eafy to form a notion of the œconomy of this procefs, by calculating the price of the articles, and the time employed in the operation, if there were not a more fimple method of eftimating its advantages, namely, the low price at which cotton is bleached in all the manufactories where this difcovery is practifed. In the fouth of France, where it is now generally adopted, cotton is bleached at the rate of about eight francs, for forty kilogrammes, (or rather cheaper than a penny a pound.) This procefs was brought from the Levant, a fhort time after we had obtained the method of producing the Adrianople red. It was reduced to practice, but hitherto kept fecret, under the name of *blanchiment à la fumée*, (vapour bleaching.)

Extract from the Bulletin of the Philomatic Society.

This procefs has been employed with the greateft fuccefs by Bourlier, at Bons-Hommes, near Paris.

The Englifh, who never neglect an opportunity of improving their manufactures, made trial of the procefs of Chaptal, as foon as they received information of it. It has completely fucceeded, as appears by the following extract of a letter from a man of fcience at London †.

" A new method of bleaching has juft been tried at Balynah, and has completely fuc-
" ceeded. The principle of the procefs appears to have been publifhed by a French

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* The conftruction here defcribed is for a fire of pitcoal; but if wood be ufed, the dimenfions of the fire-place muft be different. In the latter cafe the grate would be ufelefs, and the bottom of the veffel too far from the fire.---Delametherie.

† The extract is in French, from which I retranflate.---N.

" chemift,

" chemist, Chaptal, who is much respected by our manufacturers. I speak of the art of
" discolouring piece goods in a digester, by caustic alkaline ley. Though our first at-
" tempts did not perfectly succeed, we were not discouraged. The linen was exposed
" to the action of vapour in the apparatus, but it was not equally affected, as it appeared
" to be blotched in several places; we were, therefore, obliged to construct an apparatus,
" in order to unroll and separate the goods, and to expose the greatest surface possible to
" the action of the vapour. Suppose the boiler of a steam engine, in the form of an
" elongated ellipsis, provided with a safety valve, two tubes with cocks, to shew the con-
" sumption of the liquor, and a mercurial gage, to ascertain the strength of the steam.
" This boiler is bedded in masonry, or brick work, that it may resist the excessive pressure
" which necessarily takes place. In the interior part of the apparatus are six reels, three
" at each end, alternating with each other, in order that the action of the steam may be
" more equable upon the goods. These reels are slowly and uniformly carried round
" by simple tooth and pinion work of wood, and the first motion is given to an axis
" which passes out of the boiler through a stuffing box, which prevents the escape
" of vapour. At the top is an opening of about sixteen inches diameter, with
" a rim or flanch, on which the cover is fitted, and firmly secured by screws. Between
" the two metallic faces are placed strips of soaked leather, to prevent the vapour from
" escaping. When the cover is taken off, the workmen can enter the boiler, to dispose
" the goods upon the rollers, each of which contains about fifteen or twenty pieces,
" making in the whole about forty-five or sixty. The raw material, namely, cunnamara
" kelp, is an article of inconsiderable expence, or else the soda extracted from sea salt,
" in which there remains indeed a small portion indecompofed, but which we procure at a
" very reasonable price. It is rendered caustic by the addition of some good lime, which
" is made from our lime-stone of Parre, with these a ley is formed, which is equal to
" fourteen degrees of our hydrometer. In this lixivium the piece goods are boiled, and
" then conveyed to the digester, on the bottom of which the ley stands to about five inches
" in depth. The workman stands upon a perforated stage, which prevents him from
" stepping into the ley while he is arranging the pieces: after which, having placed them
" on the rollers, the apparatus is closed, the fire lighted, and the operation begins. As
" soon as ebullition takes place, the handle on the outside is incessantly turned, and as
" soon as the roller at one end is filled, the handle is shifted to the other roller, and the
" turning performed in the contrary direction. In this manner the operation is continued
" till the whole of the contents is bleached. From this description you may easily understand
" how this operation is performed; I shall, however, take the first opportunity of sending
" you a plan and description of the apparatus, if you wish for further information. You
" are at liberty to make whatever use you please of this account: the expence of bleaching
" is not more than one farthing per yard, including coals, workmen's wages, &c. as well
" as interest for the capital employed in the apparatus."

3   VII. *On*.

VII.

On the Chemical Effects of the Pile of Volta. By a Correfpondent.

To Mr. NICHOLSON.

S I R,

THERE is fomething fo fafcinating in the eafe with which the prefent fyftem of chemiftry can be applied to moft of the phenomena of nature and art, that I may fuffer the derifion perhaps of many philofophers, if I even queftion its application in any. However, as truth fhould be the object of all who cultivate a knowledge of nature, I fhall venture to mention a few circumftances.

I have lately repeated moft of the experiments which have been made on the pile of Volta, and have read with much attention the opinions which you have publifhed of your own, and of fome of your learned correfpondents.

When two wires of platina are ufed, and when they are placed·in water, two gafes are produced, the one having the properties of oxygen, the other of hydrogen gas. Thefe wires may be placed at very confiderable diftances from each other, and yet if they are in the fame veffel of water, they produce thefe airs as fpeedily and in as great quantities as when they are ever fo clofe to each other.

Now, Sir, I wifh to know how it happens, according to any fyftem, that the two component parts of water fhould be made to appear at fuch diftances from each other. Does the hydrogen of the decompofed particle of water on the zinc fide of the pile, fly away inftantly as the oxygen is produced on that fide, to the wire connected with the filver? If it does, why do we not fee the bubbles in its paffage? Or does the oxygen pafs from the wire connected with the filver to that connected with the zinc? Or are there two currents.

In the ordinary modes of reafoning on thefe fubjects, we generally fuppofe that when one of the component parts of a fubftance is feparated or is fixed, the other appears inftantly in fome way or other, and clofe to it. If, for inftance, a bar of red-hot iron be immerfed in water, the oxygen unites with the iron, and forms an oxide, and the hydrogen arifes immediately from it, wherever the oxygen becomes fixed. This feems perfectly well explained by the prefent theory of chemiftry. In the pile, if copper wires be fubftituted for platina, one wire will be oxidated whilft the hydrogen arifes from the other at a diftance. When the phofphuret of lime is dropped into water, the bubbles of phofphorated hydrogen appear clofe to it, as foon as the oxygen unites with the faid phofphorus. This alfo appears to be well explained by the modern theory. If the particles of water be compofed of the two bafes of oxygen and hydrogen gas, and if one of thofe particles be decompofed, and one of its principles appear, I contend that fome

4   account

account fhould be given of the other. It is a new principle for it infenfibly to hurry through the water for a diftance of fix inches or more, and there to make its appearance in the character of gas. Volta's difcovery of the pile feems to open a very large field of inquiry; it has already altered the arrangement of many facts in philofophical chemiftry. As an individual experimentalift, I can fay it has led me to many inveftigations which have explained difficulties which heretofore confiderably perplexed me.

I may hereafter take the liberty of fubmitting to you fome further experiments and obfervations on this fubject, and fome opinions refpecting the generally received doctrine of the decompofition of water.

If you fhould deem this worthy a place in your learned Journal, by inferting it you will oblige

<p style="text-align:center">Your obedient humble fervant,</p>

<p style="text-align:right">AN EXPERIMENTALIST.</p>

December 21, 1800.

*** The elucidation of the above and many other difficulties of the new galvanic philofophy muft be left to the experimental refearches of thofe able men who are now employed upon it. But I may here remark that it is probable that the local proximity of chemical effects dependant on each other may not be fuch as my correfpondent apprehends, but that fome diftance both of fpace and time may intervene between all fuch phenomena. The current from the pile may perhaps teach us to generalize and correct our notions on this head. Is it not a parallel fact in chemiftry that the vegetations of a metal (precipitated from an acid by the contemporaneous folution of another metal) are formed and depofited at the diftance of many inches from the place of folution? When lead, for example, is precipitated by zinc, and the vegetation towards the end of the procefs is formed near the bottom of a tall veffel,—is not the folution of the zinc an evidence, according to our common procefs of reafoning, that lead exifts alfo in the folvent at that place of action; and if fo, why is not that lead feparated inftead of the other very remote portion?—N.

VIII.

Memoir of several newly discovered Properties of phosphorated Hydrogen Gas. By Citizen RAYMOND, *Professor of Chemistry at the Central School of the Department of the Ardéche**.

WE are indebted to Citizen Gengembre for the important discovery of phosphorated hydrogen gas. Before his time no chemist had discovered an elastic fluid, which possessed the remarkable property of taking fire by the contact of air alone, without the necessity of increasing its temperature, or placing it in contact with a body in a state of ignition.

The undulated, and continually increasing crown, which is afforded by this gas when burned by a single bubble at a time, in any place where the air is perfectly calm; the lustre and magnificence which attend this combustion, when effected in pure oxygen gas; the sudden penetration of the two gases, and their total conversion into water and phosphoric acid. These were the only known facts which had interested chemists in the history of phosphorated hydrogen gas when I attempted to discover whether this elastic fluid did not possess other properties, which, although they might not afford a spectacle of equal brilliancy with those above-mentioned, might not on that account be less worthy the attention of philosophers.

The well known property imparted by sulphur to hydrogen, and reciprocally by hydrogen, namely, that both, when combined, are soluble in water, though each, taken separately, is perfectly indissoluble, had already led me to imagine that the same effect might take place in a combination of phosphorus and hydrogen; and that these substances being previously united, might then perhaps become susceptible of partaking of the liquidity of water, by communicating to the latter some new properties, nearly similar to those possessed by the solution of sulphurated hydrogen gas, commonly called hepatised water.

To destroy or effectually to confirm this suspicion, I took a flint glass decanter, which I filled with newly distilled water, of the temperature of twelve degrees; I then inverted it on the shelf of a pneumatic trough, in order to receive the phosphorated hydrogen gas, which is afforded by the decomposition of water, by a mixture of phosphorus and recently flaked lime. (This process is described in the Annales de Chimie, for the year 1787.) As soon as the decanter was about half full of phosphorated hydrogen gas, I removed it from the shelf, carefully closing its orifice with my finger, and then briskly shaking it, in order to cause a more rapid combination of the gas with the water, in the same manner as is done to promote the solution of carbonic acid or sulphurated hydrogen gases.

* Annales de Chimie, XXXV. 225.

By

By the clofe adhefion of my finger to the mouth of the decanter, I foon perceived that a confiderable vacuum was produced, or, in other words, that a great portion, or perhaps even the whole of the phofphorated hydrogen gas contained in the decanter had become diffolved in with the water.

I then, not without difficulty, withdrew my finger from the orifice of the decanter, in order to examine more attentively the properties of the fluid it contained, as well as to difcover whether fome particles of gas, capable of inflammation by the contact of air, might not remain. But fcarcely had the atmofpheric air entered the decanter, when a ftrong explofion took place, attended with a very brilliant light. From this effect I was well convinced that the whole of the phofphorated hydrogen gas had not been abforbed by the water contained in the decanter, and therefore I fuddenly clofed the orifice, to prevent the continuance of the combuftion, which would certainly have produced a confiderable quantity of phofphoric acid, and, confequently, have produced a degree of uncertainty in the refult of my enquiries.

The decanter being exactly clofed, I again fhook it feveral times, from a notion that I fhould thus fucceed in completely fixing the laft portions of hydrogen gas, which ftill remained undiffolved.

In hopes of fuccefs, and impatient to afcertain the new properties which might have been acquired by the impregnated water, I refolved to open the bottle a fecond time, and expofe its contents to the contact of the air. Another explofion foon followed, but lefs violent than the firft. After this event, I did not again clofe the bottle, but obferved that an extremely pale flame continued to efcape from it during feveral minutes.

When there no longer remained any apparent figns of combuftion, I proceeded to fmell and tafte the fluid in the decanter. Its fmell was extremely difagreeable, and very different from that afforded by gafeous phofphorated hydrogen: and its tafte, though very bitter, was neverthelefs infipid and naufeous. Its colour inclined to yellow.

With the tincture of turnfol it foon became perceptibly red, which I attributed to the fmall portion of phofphoric acid produced at the moment when the explofions took place, as well as during the time of the gradual inflammation which fucceeded the fecond detonation.

I was now, therefore, obliged to repeat the operation, by diffolving fome phofphorated hydrogen a fecond time in water, in order to afcertain, by a greater attention to my experiment, in what exact proportion this folution might be effected, and alfo to prevent the combuftion from taking place in the veffel: otherwife my trials would have remained inconclufive.

(*To be concluded in our next.*)

3 P 2

Scientific

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Premiums offered by the Board of Agriculture.

THIS Board having been required by a committee of the Houfe of Lords, " to examine " into, and report to their lordfhips, the beft means of converting certain portions of grafs " lands into tillage, without exhaufting the foil, and of returning the fame to grafs, after " a certain period, in an improved ftate, or at leaft without injury," and being defirous that their information, on a fubject of fo great importance, fhould be compleat—adapted to every fort of foil, and founded on the moft ample experience—have come to the refolution of offering the following premiums for that purpofe, viz.

To the perfon who fhall produce, on or before the firft day of February, 1801, the beft and moft fatisfactory effay on the fubject before mentioned, diftinguifhing refpectively, what part of the plan recommended, or of the details given, is the refult of actual experiment, accurate obfervation, or well-authenticated information — *Two Hundred Pounds.*

For the fecond beft—*One Hundred Pounds.*

For the third beft—*Sixty Pounds.*

For the fourth beft—*Forty Pounds.*

And to fuch perfons who may communicate information, which, though ufeful, may be confidered of lefs importance, fmaller rewards, proportioned to the opinion of the Board.

It is required that each effay fhall fully detail the courfe of crops, regard being had to the varieties of foil, and the time propofed for continuing the land under tillage.

Alfo, to explain the cafes in which it may be eligible to drain land, previous to tillage.

In what cafes paring and burning are advantageous, with directions thereon, regard being had to the fubfequent cropping.

The depth to which grafs lands fhould, at firft breaking up, be ploughed.

Whether the crops, intended for cattle and fheep, are to be fed on the land, and by which kind of ftock, or carted off.

To ftate—

The crop with which the grafs feeds, in each cafe, ought to be fown, when the land fhall be again laid down :

The forts and quantities of grafs feeds for each kind of foil, and whether to be provided by landlord or tenant :

Whether it be beft to mow or feed the grafs in the firft year after laying down—to detail the management in each cafe :

The manuring which may be thought neceffary :

The

The principle on which an increase of rent ought to be estimated, where permission may be given to break up old pasture now under lease.

The Board requires that these objects should be particularly attended to, with relation to the leading qualities of land, viz.

Clay, in all its distinctions; and soils too strong or wet for turnips:

Loam, in all its distinctions, fit for turnips:

Sand, including warrens and heaths, as well as rich sands:

Chalk land, and downs:

Peat, including moory, sedgy, rough bottoms, and fens.

It is hoped that no person will be deterred from communicating his knowledge to the Board, on account of his experience being confined to one of these soils only.

The Board reserves the power of withholding any premium, in the case of no essay being deemed sufficiently important to merit it.

The essays which shall obtain any premium, or other reward, to remain the property of the Board.

Each essay to be sent (sealed) without a name, but with a mark, or motto; and accompanied by a sealed letter with the same mark or motto, containing the name and address of the author; and this letter will not be opened, unless one of the prizes, or some other reward, shall be adjudged to him.

All communications to be addressed to Lord Carrington, President, Sackville-street.

Prizes offered by the Class of Mathematical and Physical Sciences of the National Institute of France in its Public Sitting, 15th Germinal in the year 8. (April 4, 1800.)

The first Prize.

The class of Mathematical and Physical Sciences having proposed a second time in the year 6, as the subject of a prize to be determined at the public sitting of Germinal in the year 8, the anatomical comparison of the liver in the different classes of animals; and no memoir having been transmitted to them on this subject, the class has thought proper to withdraw it, and propose the following question:

It is required to determine by anatomical and chemical experiments, what are the phænomena of the torpid state which certain animals, such as the dormouse, the marmot, &c. experience during the winter, with regard to the circulation of the blood, respiration, and irritability: to ascertain what are the causes of this sleep, and why it is peculiar to those animals.

The candidates are invited to examine more particularly into the differences presented by those animals in their lethargic and their ordinary states, with regard to the pulse, the heat of the blood, the frequency of respiration, the quantity of oxygen consumed in a

given

given time, and the excitability by galvanifm. They will alfo examine the anatomical differences which diftinguifh thefe animals, from fuch as are not fubject to become torpid in the winter, and enquire how far thefe differences may be fufficient to explain the phæ-nomena of this lethargic ftate.

The prize, which is a gold medal of the value of one kilogramme (15450 grains, or about 128l. fterling) will be given at the public fitting of the 15th Germinal, in the year 11 (April 4, 1803.)

The memoirs will be received till the 15th Meffidor in the year 10, and not afterwards, (July 2d. 1802.)

The fecond Prize.

The fame clafs propofed in the year 6, as the fubject of a prize to be determined at the public meeting of Germinal in the year 8, the following queftion:

To afcertain by accurate experiment the influence of atmofpheric air, of light, of water, and of earth in vegetation.

Though the inftitution has received no memoir on this interefting queftion, it has thought proper to propofe it again; but as the different works which are neceffary for the complete folution require much time and many experiments, and it can fcarcely be ex-pected, that all the elucidation which is defired can be obtained from the candidates during the intended prolongation of the term;

The clafs has determined, that in cafe no memoir fhould be received at this fecond concurrence, in which the queftion fhall have been treated in its utmoft extent, the prize fhall be given to that piece which fhall contain only a feries of experiments, facts, and obfervations, which fhall appear the moft proper to increafe the information we already poffefs refpecting fome parts of the problem propofed.

The prize will be a gold medal of the value of one kilogramme (15450 grains, or about 128l. fterling.) It will be given at the public fitting 15 Meffidor, year 10, (July 2, 1802.) The memoirs muft be fent before the firft Nivofe in the year 10, (Dec. 21, 1801.)

Third Prize.

What are the characters which diftinguifh among vegetable and animal fubftances, thofe which ferve as ferments, from thofe in which they excite the fermentative procefs?

The prize will be a gold medal of the value of a kilogramme, and it will be diftributed at the public fitting of the 15 Germinal in the year 10.

The memoirs muft be fent before the firft Nivofe in the fame year.

Candle

Candles with wooden Wicks.

Profeſſor Medicus of Heidelberg has given an account of the candles with wooden wicks, which for ſome years paſt have been manufactured and uſed at Munich, from the original inſtructions of Count Rumford, whoſe active ſervices in advancing the comforts of ſocial life are ſufficiently known *. The profeſſor uſed them for a whole winter, and found them ſuperior to every other kind of candle in the following advantages : they give the ſame quantity of light as a wax candle (bougie) ; they burn with an equal and conſtant flame ; they do not ſputter, and never run over.

The only difference between theſe and common candles conſiſts in the wick. The profeſſor did not know the actual proceſs of the manufactory in wrapping the cotton round the wood. The editors of the Annales think, that the flake of cotton delivered from the carding engine, and then preſſed between rollers, would be exceedingly well adapted to the purpoſe ; and there is no doubt that various methods may be adopted for ſo eaſy a proceſs. Any kind of reſinous wood may be uſed, and tallow of the firſt quality is applied to this object in Bavaria. The candles are ſold one eighth dearer ; but as it is ſtated that they laſt one fourth longer, there muſt be a conſiderable ſaving.

The ſize of the wood is about that of a ſtraw, and the pieces are dipped in wax or tallow, ſo as to have a very ſlight covering of the material. They are then rolled up in the carded cotton till they have acquired the ſize of a common wick. The covering muſt be very equally diſtributed. The editors propoſe, in order to detach the wick from the reſt of the cotton without the uſe of ſciſſors, that a ſtraight edge of iron ſhould be preſſed on the place where the ſeparation is intended, and the wick then drawn away.

The candles are made in a mould as uſual, taking care to place the wick exactly in the middle. Some experiments would no doubt require to be made, particularly to aſcertain the beſt dimenſions or proportions of wood, cotton, and tallow, for candles of different ſizes. The ſnuffers would require to be ſharper than for cotton wicks. I do not remember how we ſnuffed the lob chock at China ; but I think we broke off the charred piece with the fingers, or any convenient implement.

Ink capable of reſiſting the Action of Oxygenated Muriatic Acid.

Since the diſcovery of the bleaching power of the oxygenated muriatic acid, and particularly that of diſcharging the traces of common ink, it has become an object of ſerious inveſtigation to form an atramentous fluid, which ſhould not be ſubject to a treatment ſo

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* The account is in Reim's Neue Fortgeſetzte Sammlung Vermiſchter OEkonomiſchen Schriften. 12 Th.----I have not this work, but recur to the Annales des Arts et Manufactures II. 100. I do not know whether the Count was led to this conſtruction by his own meditations, or from a knowledge of the Chineſe candle, called the *Lob chock*, for an account of which ſee our firſt Vol. page 72.----N.

4   obviouſly

obvioufly inimical to the written evidence of the moſt important contraſts in human fo-
ciety.   Many German chemiſts have made experiments with their uſual intelligence and
ſkill to reſolve this problem.   Among the receipts for this purpoſe the following are the
principal * :

1. By Weſtrumb.   Boil one part of Brazil wood, and three parts of pounded nut galls,
with forty-ſix parts (the whole by weight) of pure water.   When the liquid is reduced to
thirty-two parts, paſs it through a ſtrainer while hot upon one part and half of very pure
ſulphate of iron, one part and quarter of gum arabic, and a quarter of a part of fine ſugar.
When all theſe ſubſtances are diſſolved, add one part and a quarter of good indigo ground
as fine as poſſible, and three quarters of a part of purified lamp black.

As the indigo is the ingredient in this ink which reſiſts the oxygenated acid, and as it is
only mechanically ſuſpended, it is of importance that the fluid ſhould be ſtirred well every
time it is uſed, leſt the black colour of the fluid ſhould deceive the writer.

2. By Boſſe.   Boil for about a quarter of an hour, one part by weight of Brazil wood,
with twelve parts of water.   Add half a part of allum, evaporate the whole to eight parts,
and mix one ounce (q. part ?) of manganeſe, levigated to a very fine powder, and half a
part of powdered gum arabic.

3. Apparently by the editor of the Annales.   Boil for eight minutes one part by weight
of Brazil wood, with three parts of powdered nut galls in nine parts of vinegar, and an
equal quantity of water.   After ſtraining the liquid, diſſolve therein half a part of ſulphate
of iron, and one part of gum arabic, after which add a ſolution of half a part of indigo
in one part of concentrated ſulphuric acid.   The oxygenated muriatic acid, if applied, will
diſſolve the oxide of iron in this ink, but it cannot decompoſe the indigo, and the great
objeſt is to prepare the ink in ſuch a manner, as that it ſhall not be diſchargcd by
that means.

4. The ſame editor alſo obſerves, that the common ink may be rendered incapable of
being completely diſcharged by any aſtion which the paper can withſtand, if inſtead of
water, or other liquids, the expreſſed juice of green vegetables be uſed ; ſuch for example
as the lathiris linn. the ſambucus niger, or common grafs.

* Annales des Arts et Manufaſtures II. 106.

Panorganon.

Elevation. Fig. 1.

Shaft

Screw

L

3 ... 1 ... 2

h P4

Sledge

Wooden Road 30 feet long

Fig. 2.

Profile

Socket Fig. 3.

Fig. 4.

Fig. 5.

P3

P1

P3

P3
E

Scale for Fig. 1. 2. 4. 5. 6. 7. 10.

1 2 3 4 5 ... 10 ... 15

Scale for Fig. 3. 8. 9.

1 2 3 4 5 6

Fig. 6.

Part of the Post

Rope

Fig. 7.

Lever & Wheel

N

Inclined Plane

Fig. 8.

Screw, Lever & Wheel

Fig. 9.

Skeleton
Pulley

Fig. 10.

Lever

Wheel

Cam

Post

Plate XX. Philos. Journ. Vol. IV. facing p. 480.
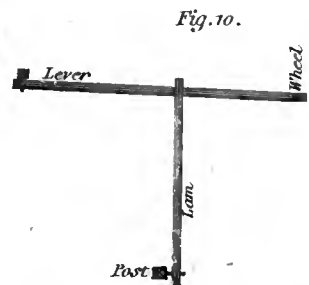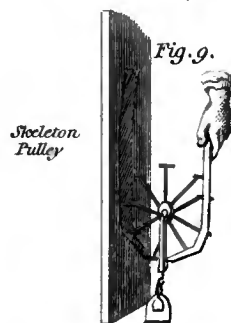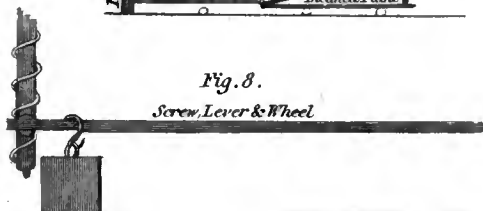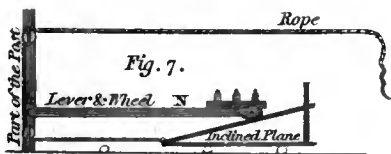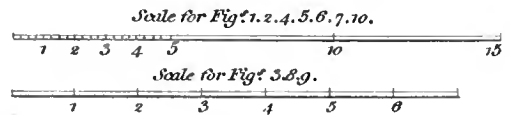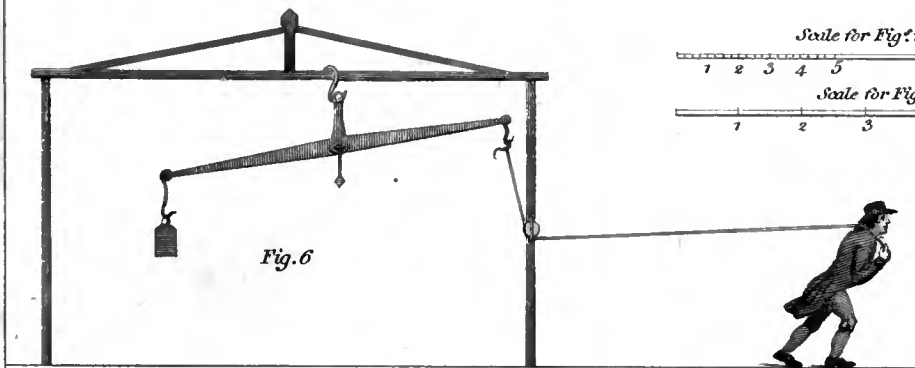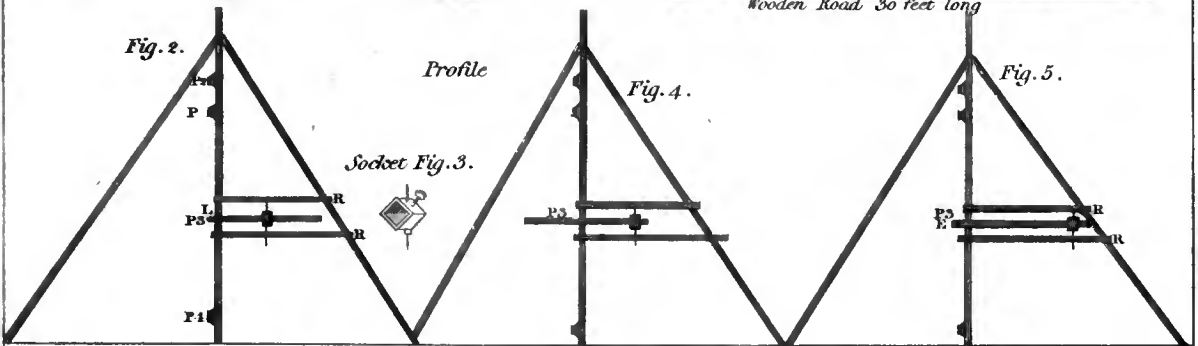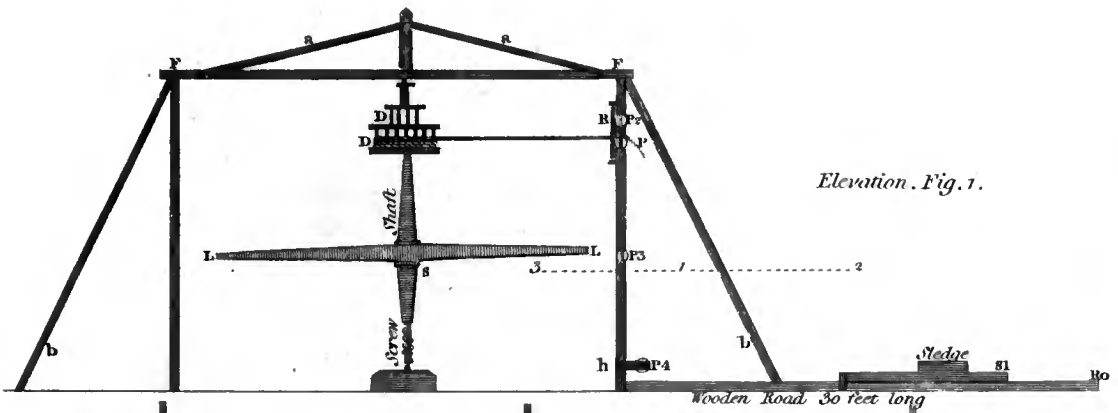
Rotatory hydraulic Engine.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

ARTICLE I.

Obfervations on a Method of reftoring the Utility of Wells, which have been abandoned in confequence of the Mephitization of the Ground. By Cit. CADAT-DE-VAUX, of the Society of Agriculture of the Department of the Seine (in France.) Communicated at the Sitting of that Society on the 16 Brumaire, in the year 8. *

THE well of an houfe in *la Rue de Beaune* was infected for feventeen years by the drainings, or tranfudations from the neighbouring privies. The proprietor defiring, if poffible, to reftore it to the ufe of the inhabitants, applied to C. Dufour, who refides in *la rue traverfe* f. St. Germain, and has fuccefsfully attended to that department of hydraulics which relates to fprings, wells, and fountains, who obtained the defired effect by an happy application of the *artefian procefs* (procedé artéfien.)

The artefian fountains and wells, as they are denominated in Flanders and Belgia, derive their name from the Province of Artois, where they were probably invented, and are made as follows:

* Tranflated from the *Decade Philofophique*, An. VIII. 10 frimaire, **p. 398.**

The ground where one of thefe fountains or wells is intended to be made, is perforated with the borer; and it depends upon circumftances, whether the refult fhall be a well or a fountain, that is to fay, upon the greater or lefs elevation of the refervoir from which the fluid is fupplied.

In the perforation in the ground is placed a wooden pipe, which is driven down with a mallet, after which the boring is again continued, in order that the pipe may be driven ftill lower. By means of the borer, the ftrata of gravel, ftones, and even rocks, if prefent, are perforated. In proportion as the cavity of the inftrument becomes loaded, it is drawn out and emptied, and in time, (for this is not a very fpeedy operation) by the addition of new portions of wooden pipe, the boring is carried to great depths, and water is at laft obtained, unlefs the labour fhall have proved to be in vain; a thing which fometimes, though very feldom, happens.

If the refervoir, or vertical head of the water obtained, fhould prove higher in its level than the furface of the ground, the water fprings up, and the refult is not a well, but a fountain. C. Dufour has thus obtained one at Courtalin, which affords water in fufficient abundance to fupply the paper manufactory of C. C. Réveillon and La Garde.

If on the contrary the level be lower, the water cannot rife above fome elevation beneath the upper orifice of the cylinder. It is then an artefian well.

A fountain or fpouting fpring may be hoped for when the boring is performed on the fide of an hill, or in a diftrict environed by mountains or hills; but in the plains at a great diftance from thefe natural refervoirs, it is fcarcely to be expected that this operation will afford more than a well.

In thofe cafes where no more than a metre (yard) or two is wanting to give the water that elevation which would produce a fountain fpring, an excavation or bafon is made to the proper depth round the pipe, which being fawed off affords a jet of water, and fills the bafon, whence it may be drawn off by a bucket, or pump.

The artefian wells are preferable in every refpect to our wells. They are lefs expenfive, and the fupply of water is more certain and abundant. In fact, in the common conftruction of our wells, when the workmen have arrived at the water, and the fprings gain upon them, it becomes neceffary to fix the windlafs, and too frequently a well is made which fupplies but a moderate quantity of water, and is dry in fummer.

In this cafe there is no remedy but to ufe the artefian procefs; that is to fay, in order to recover the water in thefe dry wells, it is neceffary to perforate the bottom, infert a cylindrical pipe, and proceed to feek for water at a greater depth.

This method of adding to the depth of wells already in exiftence, and affording a fcanty fupply of water, is due to an inhabitant of Thuringia. The œconomical fociety of Leipfic in its Collection of Memoirs (Semeftre St. Michel 1785) has publifhed this procefs. One of its members, who had a well which had become dry, applied the artefian procefs with great fuccefs. The author fpeaks of this method as capable of fpeedily affording a large quantity of water, as of great utility in a camp or fortrefs; and when the waters near

the

the furface are not of a good quality, this is the beft method of proceeding in fearch of better at a great depth, as Cit. Dufour has done in the well which forms the fubject of the prefent obfervation.

After the well in queftion had been emptied, C. Dufour bored the bottom, and placed a pipe of ten feet in the hole. This depth was fufficient to pafs through the bed of mephitized earth, and reach the lower body of water, which being thus infulated by the cylinder, rifes pure into the body of the pump, which is fixed for that purpofe.

I have thought it ufeful to give publicity to this laft procefs, more efpecially with regard to Paris, where the neceffaries and drains have fucceffively infected the ground by their bad conftruction, and rendered a great number of wells ufelefs. This method appears to be the only one capable of remedying this inconvenience, and reftoring the utility of thefe abandoned wells.

I fhould reproach myfelf for not entering into a longer detail refpecting thefe artefian wells and fountains, on account of the preference they deferve over our ordinary wells, by the advantages they prefent. But knowing that my colleague C. Gillet Caumont poffeffes very interefting facts refpecting them, I have invited him, and he has promifed the Society to extract from a work undertaken on this object, whatever may relate to its rural and œconomical ufes, and to publifh it *.

II.

Memoir on feveral newly difcovered Properties of phofphorated Hydrogen Gas. By Citizen RAYMOND, Profeffor of Chemiftry at the Central School of the Department of the Ardèche.

(Concluded from Page 475.)

FROM the quantity of water and gas which I had at firft attempted to diffolve, as well as from the explofions which took place the inftant the bottle was unftopped, I was certain that the diftilled water could not, at a temperature of twelve degrees, diffolve an equal volume of phofphorated hydrogen gas. In the fecond experiment, therefore, I had the precaution to fill the decanter only one third with the above gas; after which I fhook the mixture feveral times, in order to facilitate the combination of the gas with the water, and

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* It is at prefent a common practice in London, fince the general introduction of the fteam engine into private manufactories, to fink a well, below the ftratum containing the furface water, into a quickfand, where plenty of foft water is met with. The artefian procefs undoubtedly deferves to be better known from its fimplicity, cheapnefs, and other good qualities. Dr. Darwin has defcribed a fomewhat fimilar operation in the Philof. Tranf. Vol. LXXV. 1, and in the fame work, Vol. LXXIV. may be feen the Account of the King's Wells at Harwich, by Tho. Hyde Page, Efq. and Mr. Vulliamy's overflowing well, in our Journal II. 276.----N.

at

at the same time to make the union as complete as possible. I then unstopped the decanter, and inverted it in a small tub, full of newly distilled water, to ascertain whether it would be entirely filled with this water, by the mere pressure of the air, and that I might also by this means be enabled to judge whether the whole of the gas had become liquefied.

I accordingly saw that a portion of the water in the tub was suddenly pressed into the decanter, but not so much as to fill it entirely, which shewed that some of the gas still remained uncombined with the water. I next made trial of the absorbed gas by passing a few bubbles through the liquid, which took fire by contact of the external air, and thus proved that the phosphorated hydrogen gas had not been at all decomposed either by the agitation, or by its contact with the distilled water; whereas it speedily loses its high degree of combustibility, when collected in vessels filled with undistilled water, or with water that had been distilled for a length of time. This effect I attribute to the quantity of air held in solution by common water, the oxygen of which coming in contact with a portion of phosphorus separates it from the hydrogen, by converting it into phosphoreus oxide, which being perfectly insoluble, is deposited on the inside of the glass, without affording any perceptible combustion in this kind of oxidation; because the oxygen of the atmospheric air, which partakes of the liquidity of water, being thus deprived of a great part of the caloric and light combined with it, during its aerial aggregation, cannot produce both those effects in a perceptible manner, when it passes from one combination to another in this state of liquid aggregation.

Having again shaken the decanter, after closing its mouth, I succeeded by means of the small quantity of water which had entered, and of which I kept an accurate account in obtaining the absorption of the rest of the gas; for on opening the vessel again, it soon became entirely filled with water. Hence I may venture to affirm, from the diversified trials, as well as from several others which I shall pass over, that water which has been deprived of its air by distillation, is capable of dissolving and liquifying at the ordinary temperature, rather less than one fourth of its bulk of phosphorated hydrogen gas, and that by this proportion it is completely saturated.

This solution, prepared according to the above directions, and preserved from the contact of air, has always exhibited the following properties: its colour is nearly similar to that of roll brimstone, though in general pale. Its smell is strong and disagreeable, and its taste extremely bitter, though insipid and nauseous.

On examining this solution in the dark, it did not appear in the least luminous, a proof that the phosphorus had intimately combined with the hydrogen,

When distilled in a small retort with the pneumato-chemical apparatus, a little below the temperature of boiling water, and particularly if submitted when recently prepared, it afforded a considerable quantity of phosphorated hydrogen gas, equally pure and combustible as that obtained by heating caustic alkalies, or quick lime with phosphorus, and the addition of a small quantity of water. What remained in the retort after the disengage-
ment

ment of the gas had entirely ceafed, was nothing but pure water, without fmell, tafte, or colour; and in every refpect fimilar to that newly diftilled.

When expofed to the air of the atmofphere, it foon depofited a remarkable quantity of red oxide of phofphorus, and at the fame time emitted fome hydrogen gas, but which was no longer fufceptible of inflammation, except by the contact of an ignited body. By keeping the folution expofed to the air for a length of time, and occafionally increafing or renewing the points in contact by a gentle agitation, it becomes completely decompofed, or in other words, it refolves itfelf entirely into phofphoreous oxide, and pure hydrogen gas.

The tinctures of turnfol and violets do not undergo any change in their colour by contact with the liquid phofphorated hydrogen, a proof that this fluid is neither acid nor alkaline.

The fulphuric and nitric acids, and the muriatic acid either fimple or oxygenated, on being added to this liquor, produced no remarkable effect; neither did the application of potafh, foda, nor ammonia.

The oxydes of mercury and lead were fpeedily reduced, and immediately converted into metallic phofphurets, by their mixture with the folution of phofphorated hydrogen gas.

When poured into the nitrate of filver this folution inftantly produces a black precipitate in great abundance, which does not change its colour, and which before the blow pipe exhibits all the characters of metallic phofphurets.

When brought into contact with the nitric folution of mercury, it alfo produced a very confiderable precipitate in great abundance, which is at firft black, but foon becomes white and cryftallized, in proportion as it paffes from the ftate of phofphuret to that of the mercurial phofphate, by abforbing oxygen either from the nitric acid in which it takes place, or the atmofpheric air with which it is in contact.

The folution of lead by the nitric acid is likewife decompofed by the hydro-phofphorated liquor, but with lefs activity than the folutions of filver and mercury. In this decompofition fome phofphuret of lead is alfo produced, which in time becomes converted into phofphate.

The fulphate of copper alfo, after a certain time, exhibits a confiderable quantity of black precipitate, when a portion of phofphorated hydrogen gas is added to the folution. This precipitate, like that obtained by the decompofition of the nitrate of filver, retains its black colour; which feems to fhew, that it cannot without great difficulty be converted into phofphate.

The fulphate of iron did not appear to be affected by this liquid.

Nitrate of arfenic, when poured into this fluid, does not undergo any perceptible decompofition, till the expiration of feveral days; but at length a precipitate is generated of a beautiful yellow colour, in the form of fmall grains, which is capable of remaining expofed to the air for a length of time, without undergoing any change. This precipitate is the arfenical phofphuret.

CONCLUSIONS.

CONCLUSIONS.

The following are the new properties which I conceive I have been the firſt to diſcover in the phoſphorated hydrogen gas.

1. This gas is capable of uniting with diſtilled water in the proportion of about one-fourth of its bulk, when the ſolution is effected at about ten degrees of the French thermometer *.

2. It communicates to the water in which it is diſſolved a ſtrong and diſagreeable ſmell, as well as a bitter taſte ; which may at ſome future period be uſed with ſucceſs in the treatment of various diſeaſes, either from the facility with which this preparation is de-compoſed, or on account of the effect produced by the phoſphorus it contains, in the formation of animal ſubſtances.

3. When water, purified of its air, is uſed for the purpoſe of diſſolving this gas, and if, when thus diſſolved, it is carefully preſerved in cloſely ſtopped veſſels, it may be kept for a long time without undergoing decompoſition ; ſo that by heating the ſolution, the whole of the phoſphorated hydrogen gas which it contains may be diſengaged in the gaſeous ſtate.

4. When water has been by this means deprived of the phoſphorated gas which it had diſſolved, it again returns to its original ſtate of purity ; whence it follows that its new properties are derived from the preſence of this gas alone.

Laſtly, this ſolution is capable of ſpeedily reducing ſeveral metallic oxydes, whether alone, or diſſolved by acids, and of forming with them, by double elective attraction, water and metallic phoſphurets : which combinations have only hitherto been obtained in the dry way only ; that is to ſay, by heating metals with phoſphorus, or rather by the decompoſition of phoſphoric glaſs, or metallic phoſphates, by means of metals and carbon.

III.

Deſcription of the underground inclined Plane, executed at Walkden Moor in Lancaſhire, by his Grace the Duke of Bridgewater. By the Rev. FRANCIS EGERTON †.

I BEG leave to preſent to the Society an account of the under-ground inclined plane, which the Duke of Bridgewater has lately made at Walkden-Moor, between Worſley and Bolton, in Lancaſhire. To this account I have ſubjoined two plans, with a table of reference to each.

* It is probable that at the temperature of zero, a greater quantity might be diſſolved by the water; but for want of a ſufficient quantity of phoſphorus, I was not able to aſcertain this fact.

† Extracted from the Tranſactions of the Society for the Encouragement of Arts, &c. for the year 1800. The letter is addreſſed to their ſecretary, and the Society has voted their annual gold medal to his Grace.

At Worſley the Duke of Bridgewater's navigation begins; it goes weſt to Leigh, and eaſt to Mancheſter, where it locks up into the Rochdale canal. In its way to Mancheſter, it turns out, in a weſtern direction, near Longford Bridge, to meet the Grand Trunk Canal, above Preſton Brook; and from thence it goes north-weſt to Runcorn, where it locks down into the Merſey, in the tide-way to Liverpool.

To this navigation above-ground, which, in all its directions, is extended through a length of * forty miles, upon one level, without tunnel or lock, except the locks at the extremities. At Worſley, an underground navigation is joined, which goes to the different mines of coal under Walkden-Moor; from which mines, by theſe navigations above-ground and under-ground, Mancheſter and various other places are ſupplied with that valuable article.

The canals of this under-ground navigation lie upon two levels, or ſtories.

The lower is upon the ſame level with the open navigation, which it joins at Worſley; and conſiſts, in the different lines which it purſues to the different ſeams of coal, of near twelve miles of tunnelling.

The higher is thirty-five yards and a half perpendicular height above the level of the lower, and varies from thirty-eight to ſixty-one perpendicular yards below the ſurface of the earth, and conſiſts of near ſix miles of tunnelling.

The tunnelling of each level is ten feet four inches wide, and eight feet ſix inches deep; and the depth of water, three feet ſeven inches.

Before a communication was made by an inclined plane, the coals were diſcharged by hand from the boats on the higher level, and were let down the pits in tubs by an engine and a break-wheel into thoſe upon the lower. To convey the boats themſelves from the canals of the higher level into that of the lower, was the intent of making this under-ground inclined plane. By the help of this machinery, the whole buſineſs is now done at once, without diſcharging or damaging the coal, and at one fourth of the expence: for the boats of the higher level are bodily let down the inclined plane, and are floated from the foot of it through nearly three miles, in a ſtrait line, of the lower level canal, into the open navigation at Worſley: and, whereas they were before obliged to be drawn up to the ſurface of the earth at great inconvenience and expence, to be repaired at a work-ſhop on Walkden-Moor, they now come of themſelves, in their courſe of buſineſs, to be repaired at the great dock-yard at Worſley.

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* *Forty miles upon one level.*] Adding to theſe forty miles, nearly twelve miles of the Duke of Bridgewater's underground navigable canal, which lie upon his lower main level, and including eighteen miles of the Grand Trunk Canal betwixt the loweſt lock between Middlewich and Preſton-Brook, there are ſeventy miles of navigable canal, without a lock, upon one level, eighty-two feet above low-water mark; whereby a communication is obtained between London, Liverpool, Briſtol, and Hull. At this loweſt lock the Grand Trunk Navigation locks down, to be upon a level with the Duke of Bridgewater's.

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The place where the inclined plane is constructed, is adapted in a singular way for the purpose.   There is a bed of white rock, or grit, eight yards twelve inches deep, which dips one in four, lying exactly in the direction most convenient for the communication between the two levels; which bed of rock is hollowed into a tunnel, driven upon the rise of the metals, by blasting with gunpowder, and working it down with wedges and hammers.   In this tunnel, formed through a rock reaching from the lower to the higher level, the inclined plane is fixed; and, by its being in the heart of a rock, the whole workmanship can be pinned, secured, and compacted together at the top, bottom, and sides, most effectually:—an advantage which no inclined plane above-ground can have, and which renders this a singular production, no where perhaps to be imitated.

The run of the inclined plane is one hundred and fifty-one yards, besides eighteen yards, the length of the locks, at the north or upper end: and the fall is one in four, corresponding with the dip of the rock.

Of these one hundred and fifty-one yards, about ninety-four yards are formed into a double waggon-way, in order to let two boats, namely, the empty and the loaded boat, pass up and down; and are divided by a brick wall, supporting the roof, in which are openings for a person to escape out of the way of the boats; which double-waggon way joins in one, about fifty-seven yards from the lower level.

The whole width of the double waggon-way is nineteen feet; and of the single waggon-way, after the junction, ten feet.

These waggon-ways are supplied with iron rails, or gullies, laid on sleepers, down the whole run; and the height of the roof, above the iron rails, is eight feet.

At the top of the inclined plane there is a double lock, or rather two locks, side by side, formed in the heart of the same rock, which deliver the loaded boats from the higher level down the inclined plane, and receive the empty boats from the lower.   The length of that part of the tunnel in which these are formed, is eighteen yards; the width or diameter, twenty feet six inches; and the height of the roof, at the north end and above the locks, at *d d*, *Plate XXI. Fig.* 1. twenty-one feet, to admit the break-wheel.

The bottom, or south end of the inclined plane, is six feet nine inches under the surface of the water, where the loaded boat floats off the carriage upon the canal of the lower level.

The depth of the locks, under water, at the north end, is four feet six inches; at the south end it is eight feet.

The wall between the locks is nine inches above the surface of the level water; its breadth is three feet.

The diameter of the horizontal main shaft, upon which the rope works to let the loaded boats down, and to draw the empty boats up, is four feet eleven inches, and its circumference is fifteen feet five inches.   The main-rope is two inches and a half in diameter, and seven inches and a half in circumference.   It is lapped round with a small cord of about an inch in circumference, for the length of about one hundred and five yards, to prevent its wearing, which it does chiefly when it drags upon the bottom, when at work,

at the place where the waggon-ways unite; and, for the fame purpofe, rollers of eight inches diameter are fixed at intervals down the run of the inclined plane. Moreover, a hollow caft-iron roller of eight inches and a half diameter is fixed acrofs the weft lock, parallel to the upper weft lock-gate, and near the north end of the lock, but half a yard higher than the gate, in order to bear up the rope, and to prevent it from fwagging.

A hold-faft rope is faftened to the main-rope, to ftay each boat upon its waggon, as they go up or down. It is marked *k k*, in *Fig.* 2, *Plate XXI.* and its ufes are more particularly detailed in the table of reference, at *k k*, to that plate.

Upon this horizontal main-fhaft is a break-wheel above mentioned, which regulates the motion of the loaded boat going down the inclined plane.

The number of iron teeth, or cogs, in the fpur-wheel, which is faftened to the fide of the break-wheel, is three hundred and feventy-two; and the little nut-wheel, *No.* 3, *Fig.* 4. which fets it in motion, contains eleven teeth, or cogs. The nut-wheel is fupported by two uprights from the pillar to the roof, and works between them. Two winches or handles, *No.* 4 4, *Fig.* 4. on its axis, put the main-fhaft, *d d*, *Fig.* 2. or *No.* 1, *Fig.* 4. in motion. The power of both united enables a man, who ufes a force equal to forty pounds weight, to fet forward two tons upon the waggon-road: and this force, multiplied at the winches or handles, may be ufed to fet forward the loaded boat out of one lock, and to bring the empty boat into the other. The boats being thus put in motion, the little nut-wheel is difengaged from the main-fhaft, by a flide drawing the little nut fideways, fo as to difengage the teeth, or cogs, from the cogs of the fpur-wheel. The weight of four tons going down bring up about one.

The fpur-wheel, however, which is faftened to the break-wheel, *No.* 2, *Fig.* 4. is feldom ufed, as it is occafionally only put in motion to regulate the ftretch of the ropes when new, and to draw the light boat into the lock, when, at any time, it may happen to be over-weighted with materials, fuch as mortar, props, flabs, &c. for the ufe of the higher level collieries, and will not move of itfelf, upon a balance, out of the lower level.

The length of the carriage, or cradle, is thirty feet; its width is feven feet four inches. It moves upon four folid caft-iron rollers, which run upon caft-iron plates; on one fide of each of which there are iron crefts, which ftand two inches higher than the plates, and prevent the carriage from running off the road.

The weight of neat coal, contained in the loaded boat, is about twelve tons: the boat weighs about four tons; and the carriage, or cradle, in which the boat is placed, when conveyed down the inclined plane, is about five tons:—in all about twenty-one tons.

At this inclined plane thirty loaded boats are now let down, with eafe, in about eight hours; that is to fay, four boats are let down in a little more than an hour. The boats ufed in thefe collieries are of different fizes and dimenfions; fome will carry feven, fome eight and a half, fome twelve tons.

The weight of neat coal, independently of the weight of the carriage and boats which is let down the inclined plane, in twelve-ton boats, in eight hours, will confequently be three

hundred and sixty tons. The weight of the carriage, suppose five tons, let down in the same time, will be one hundred and fifty tons; and the weight of the boat, suppose four tons, thirty times down, in eight hours, will be one hundred and twenty tons:—in all six hundred and thirty tons down in eight hours.

The weight of the carriage thirty times up, and thirty boats up, in eight hours, will be

Carriage,   at 5 tons,   30 times up = 150 tons
Boat,   at 4 tons,   30 times up = 120 tons
                                                   _____

                                In all   270 tons
                                up in eight hours.

So that there will be 630 tons down
                                270 tons up
                                          _____

             In all   900 tons moved at the inclined plane, in 8 hours, exclusive of an
                              indeterminate quantity of materials occasionally brought up for
                              the use of the higher level collieries.

The various feeders which are loosened by opening the coals in the higher level collieries, as well as three sufficient reservoirs, which may occasionally be resorted to, and used in a dry season, keep the higher level always to its height, and afford a constant supply of water to fill the locks, for the purpose of working the inclined plane.

This inclined plane was begun in September, 1795; it was finished, and in use, in October, 1797.

Of this, as of most of his other great works, the Duke of Bridgewater was himself the planner and contriver:—to project greatly, and to execute completely, are the perfection of genius.

The singularity of the place in which it is constructed; the original boldness of the design; the ingenuity and mechanism displayed in planning and executing it; the dispatch with which it has been finished; the simplicity, beauty, and harmony of its parts, tending to one united whole; and, above all, the perfection to which it is proved to have been brought, now that it is practically in use; render it equally astonishing with any other of the stupendous works which have been so ably planned, and so successfully executed, by the first projector and patriotic father of inland navigation.

I have the honour to be, &c.

FRANCIS H. EGERTON.

Bridgewater-House, March 5, 1800.

Reference

Reference to Plate XXI. being a Plan and Section of his Grace the Duke of Bridgewater's *Under-ground Inclined Plane, at Walkden-Moor Colliery, near Worsley, in Lancashire.*

Figure 2.

a to *b*, Dip of the metals and waggon-road on the under-ground inclined plane. From *b*, on the lower level, to the mouth of the tunnel, is three miles.

A, The east lock.

B, The west lock.

C, Represents a section of the lock; the dotted line shows the horizontal depth, and the black line under it, the slope upon which the waggon-wheels run to receive the loaded boat, or to bring the empty boat into the lock.

d d, The main-shaft, four feet eleven inches diameter, upon which the ropes work to wind the boats up and down; and here also the break-wheel is fastened on, together with a spur-wheel, and a nut-wheel. See *Fig. 4. No. 1.*

e, A passage betwixt the higher level and the locks.

f f, A loaded boat going down, and an empty boat going up the under-ground inclined plane.

G, A brick wall from the sole to the top of the inclined plane, in order to give additional support to the roof.

h h h h, Openings through the brick wall *G*, into which a person may step out of the way of the boats, at the time they are passing up and down.

i, A bell, which is rung by the rope dotted to *b*, upon the lower level, at the bottom of the under-ground inclined plane, to give notice when the empty boat is upon the waggon, or cradle, and when the men below are ready, that the loaded boat may be let down by the men above.

k k, Holdfast-ropes fastened to the main-ropes. and hooked on to a ring at the south end of each boat, as it goes up or down, in order to stay the boats upon the waggon or cradle, that they may not swag, or slip off. These holdfast-ropes are spliced on to the end of the main-ropes, and run above and between the two bridle-ropes when they are fastened to the iron uprights, which are upon each side of the waggons, or cradles: and they run over the north end of the boat, to be hooked on to the south end.

l l, The bridle-ropes fastened to the main-ropes at *O*, and secured to two iron uprights upon each side of the waggon, or cradle.

O O, The places where the main-ropes, the bridle-ropes, and the holdfast-ropes, are fastened all together.

No. 1. An open space driven into the side of the lock *A*, to which a pit is sunk from the higher level, in order to convey the water out of the locks down to the lower level, and also to force a current of fresh air into the lower level collieries.

No. 2.

No. 2. A paddle to let the water out of the lock *A*, into the pit *No.* 1.

No. 3. A paddle to let the water out of the lock *B*, through a culvert, reprefented by dotted lines, under the lock *A*, into the pit *No.* 1.

No. 7, 7. Paddles in the lock-gates, to let the water out of the higher level into the locks.

No. 8, 8. The two north lock-gates, one to each lock, which turn upon the heels of the gates, and fwing round when they are opened or fhut.

No. 10, 10. Two ftops or cloughs, one to each lock, which ferve as lock-gates to the fouth end, and are raifed and let down by a windlafs.

S, A ftop, which is ufed occafionally when the lock-gates want repairing.

T, The place where the boats which are to pafs to or from the lower fingle waggon-way are directed, at pleafure, into either part of the double waggon-way, by a moveable iron fleeper or plate at that point, upon which fleeper or plate the wheels of the boat-carriage or cradle run.

Table of Reference to Plate XXI. Fig. 4, *of his Grace the* DUKE OF BRIDGEWATER'S *Under-ground Inclined Plane, at Walkden-Moor Colliery, near Worfley, in Lancafhire.*

1. Main-fhaft, on which the rope laps.

2. Break-wheel, on one fide of which the fpur-wheel is faftened.

3. Nut-wheel, out of geer, but which flides into the fpur-wheel, when ufed to draw the empty boat into the lock occafionally, and which is fupported by two uprights from the pillar to the roof.

4, 4. Winches or handles, to work the nut and fpur-wheel.

5, 5. The main-ropes faftened to the boats, and which are lapped to prevent their wearing.

6. The fpur-wheel, which is faftened on one fide of the break-wheel; and on which break-wheel is a ftrong iron-jointed timber brace, which, according to the preffure given thereto by the man who attends it, will allow the loaded boat to defcend quick or flow, or detain it in its paffage.

7, 7. Paddles in the lock-gates, to let the water out of the higher level into the lock.

8. A hollow caft-iron roller, to prevent the main-ropes from fwagging.

9. Shroud-wheel, to prevent the ropes going over the end of the main-fhaft, flipping off, jerking, or breaking. This ftands three inches above the main-fhaft.

Concerning

IV.

Concerning the Engine for raising Water by the lateral Communication of Motion. In a Letter from the Inventor, Mr. WILLIAM CLOSE.

LETTER III.

To Mr. NICHOLSON.

S I R,   *Dalton, Jan.* 12, 1801.

SINCE the date of my second letter on raising water by the lateral communication of motion, I have occasionally turned my thoughts to the subject, and made some improvement in the plan of the apparatus for producing a series of elevations, which I shall communicate in the present letter.

The height to which the lateral communication of motion will cause the pressure of the atmosphere to raise water in one column, depends upon the height of the water in the reservoir above the conical tube. When the height of the charge is five feet, the straitest diameter of the conical tube equal to six inches, and its other dimensions adapted to the pressure; the extent of the column may be estimated at eleven feet. Under similar circumstances, when water is to be raised to the height of twenty-three feet, the following method may be pursued.

Raise the water four times by the pressure of the atmosphere, through four vertical tubes, each rising to the height of seven feet and an half above the brim of the cistern from which it is to receive water, as is represented in Fig. 3, Pl. XIII. When the charge of the reservoir is five feet, it will be of no use to make the tubes much longer than seven feet and an half, as the ascent through the space above will be slow, and the extension of the elevation in this way will seldom compensate for the loss of time.

Fig. 1, in Pl. XXI. represents the first vessel above the reservoir; C part of the water tube, and E part of the air pipe which leads to the conical tube.

Let the air pipe E be made four times the width that would be required for the elevation of one column, and let the valve represented by *m* be adopted to its top.

The air pipe represented by G should be made wide for this vessel; in all those above it may be omitted.

Into the top of the vessel D insert three small air pipes O P Q, and carry each of them to a different vessel on the top of a water tube. The length of each of these above the cistern from which it is to raise a column of water, must rather exceed the length of the lowest rarefying tube above the lowest reservoir. They will require no valves.

When the rest of the apparatus is completed upon the principles before laid down, the lateral communication of motion will rarefy the air equally in all its parts, and the water will ascend through all the vertical tubes, into and nearly fill the vessels upon their tops.

But

But before it quite reaches the tops of the tubes O P Q, it will begin to defcend in a full ftream through the loweft rarefying tube E, which is the fhorteft: the lateral action now ceafing to rarefy the air, the water will rife no higher, but will remain at the fame elevation above each of the cifterns, until the external valves fhall open. After this the valve *m* will clofe the end of its tube, and prevent the diminution of the jet; the air from without will enter the loweft veffel D, through the tube G; part of it will afcend from this veffel through the empty branches O P Q of the rarefying tube into the veffels above; and at the fame time the raifed water will flow out of all the veffels into different cifterns.

If all the veffels are connected together by their air pipes, inftead of each being connected to the loweft, the fame equality of action will take place. In both cafes, as the elevation of the water in each of the tubes muft correfpond with the degree of rarefaction in the air before it can begin to afcend, it is evident that the tubes will all be full at the fame time, although they may contain columns of unequal height, when the air begins to rarefy: the imperfection of one or two of the valves to the water tubes can therefore be attended with little inconvenience.

If the air pipes were feparately connected with the conical tube, fome of the veffels might be filled with water before others: this inequality, indeed, might be removed by bringing all the tubes into one; but in either cafe, if the external valves were to remain too long clofe, the raifed water would defcend through the air pipes, and wafte rapidly from feveral cifterns at once. The arrangement that has been recommended will be free from this inconvenience, however long the valves may remain fhut.

By proportioning the widths of the water tubes and the capacities of the veffels upon their tops, it will be eafy to raife a little more water into the firft ciftern, than will be delivered into the fecond; and a little more into the fecond, than will be carried into the third; and fo on, in order to prevent any of the lower cifterns from becoming empty of raifed water. The wafte thus incurred will be very inconfiderable. If, however, more water fhould be raifed into any of the higher cifterns than thofe below can long afford, it it will be proper to make part of it defcend through tubes for the purpofe, and replenifh the lower ones.

As the jet will bear the admiffion of fome air without diminution, it is not neceffary that the valve *m* fhould perfectly clofe the end of the air tube: if it did, it would not be eafy to open. When a cock is ufed inftead of this valve, it muft be fixed in the tube at fome depth below the furface of the water in the refervoir, and a weight hung upon an arm or lever proceeding from it, to keep open the paffage through the tube. This weight will alfo ferve to counterbalance the empty veffel which contains the fyphon, and opens the valves. A cock can be attended with little inconvenience in this fituation: if, after long continued ufe, a little water fhould be admitted, it will caufe no perceptible diminution of power.

A fecond and third feries of elevations may be caufed by connecting more air pipes with the conical tube; but as the introduction of fo many pipes into the throat would diminifh

its

its width, and greatly retard the expenditure, it will be beſt to introduce them all through holes made in the circumference of the tube, and to take off their ends even with its inſide. When water is thus raiſed in an additional number of columns, the water tubes ſhould be made ſtill ſhorter than was recommended before, and to prevent the water which is raiſed by the ſecond ſeries from being waſted, the rarefying tubes ſhould be carried up to ſuch height above each veſſel, that the column may counterbalance the active preſſure of the atmoſphere before it arrives at the top. Recourſe may be had to this method when ſeveral conical tubes are employed, as is repreſented in Fig. 3, Pl. XIII. In this laſt caſe, however, it will be unneceſſary to make any addition to the loweſt veſſel, as the expenditure can only take place from the loweſt ciſtern.

In all the arrangements that have been hitherto deſcribed, the ſtream through the conical tube is ſuffered to run one half of its time without producing any uſeful effect. It will, however, be of very great advantage to keep the lateral action conſtantly in work in rarefying the air in different parts of the apparatus. The raiſed water may then be let out of one ſet of veſſels while it is riſing into another ſet, and the water that flows out of one deſcending veſſel, and has opened the valves of the firſt ſet, may, by being carried into a ſmall ciſtern a ſecond time, and delivered from thence by a ſyphon into another deſcending veſſel, be made to open the valves of the ſecond ſet.

As it may ſometimes happen that all the valves may be half open, and much air be thus admitted into the ſtream, it is likely that cocks fixed in the rarefying tubes, and placed in water, would on this occaſion be preferable to valves, particularly when the rarefying tubes can be filled with water, for then no air can deſcend into the jet. I think, however, that this can only be neceſſary when ſeveral air pipes are connected with the conical tube, and the air would enter the ſtream on all ſides.

In this manner by the aſſiſtance of the lateral action of a ſtream of water through one conical tube, and the preſſure of the atmoſphere, an uſeful quantity of water may be raiſed to a conſiderable height.

<div align="center">I am, SIR,</div>

<div align="center">Your humble ſervant,</div>

<div align="right">WILLIAM CLOSE.</div>

V. *On*

V.

On the Power of penetrating into Space by Telescopes; with a comparative Determination of the Extent of that Power in natural Vision, and in Telescopes of various Sizes and Constructions; illustrated by select Observations. By WILLIAM HERSCHEL, *LL. D. F. R. S.* [*]

I T will not be difficult to shew that the power of penetrating into space by telescopes is very different from magnifying power, and that, in the construction of instruments, these two powers ought to be considered separately.

In order to conduct our present inquiry properly, it will be necessary to examine the nature of luminous bodies, and to enter into the method of vision at a distance. Therefore, to prevent the inaccuracy that would unavoidably arise from the use of terms in their common acceptation, I shall have recourse to algebraic symbols, and to such definitions as may be necessary to fix a precise meaning to some expressions which are often used in conversation, without much regard to accuracy.

By luminous bodies I mean, in the following pages, to denote such as throw out light, whatever may be the cause of it: even those that are opaque, when they are in a situation to reflect light, should be understood to be included; as objects of vision they must throw out light, and become intitled to be called luminous. However, those that shine by their own light may be called self-luminous, when there is an occasion to distinguish them.

The question will arise, whether luminous bodies scatter light in all directions equally; but, till we are more intimately acquainted with the powers which emit and reflect light, we shall probably remain ignorant on this head. I should remark, that what I mean to say, relates only to the physical points into which we may conceive the surfaces of luminous bodies to be divided; for, when we take any given luminous body in its whole construction, such as the sun or the moon, the question will assume another form, as will appear hereafter.

That light, flame, and luminous gases are penetrable to the rays of light, we know from experience; [†] it follows therefore, that every part of the sun's disk cannot appear equally luminous to an observer in a given situation, on account of the unequal depth of its lumi-

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[*] Philosophical Transactions, 1800, p. 49.

[†] In order to put this to a proof, I placed four candles behind a screen, at $\frac{3}{4}$ of an inch distance from each other, so that their flames might range exactly in a line. The first of the candles was placed at the same distance from the screen, and just opposite a narrow slit, two-thirds of an inch long, and $\frac{1}{4}$ broad. On the other side of the screen I fixed up a book, at such a distance from the slit that, when the first of the candles was lighted, the letters might not be sufficiently illuminated to become legible. Then, lighting successively the second, third, and fourth candles, I found the letters gradually more illuminated, so that at last I could read them with great facility; and, by the arrangement of the screen and candles, the light of the second, third, and fourth, could not reach the book, without penetrating the flames of those that were placed before them.

nous

nous atmofphere in different places *. This regards only bodies that are felf-luminous. But the greateft inequalities in the brightnefs of luminous bodies in general, will undoubtedly be owing to their natural texture; which may be extremely various, with regard to their power of throwing out light more or lefs copioufly.

Brightnefs, I afcribe to bodies that throw out light; and thofe that throw out moft are the brighteft.

It will now be neceffary to eftablifh certain expreffions for brightnefs in different circumftances.

In the firft place, let us fuppofe a luminous furface throwing out light, and let the whole quantity of light thrown out by it be called *L*.

Now, fince every part of this furface throws out light, let us fuppofe it divided into a number of luminous phyfical points, denoted by *N*.

If the copioufnefs of the emiffion of light from every phyfical point of the luminous furface were equal, it might in general be denoted by *c*; but, as that is moft probably never the cafe, I make *C* ftand for the mean copioufnefs of light thrown out from all the phyfical points of a luminous object. This may be found in the following manner: let *c* exprefs the copioufnefs of emitting light, of any number of phyfical points that agree in this refpect; and let the number of thefe points be *n*. Let the copioufnefs of emiffion of another number of points be *c'*, and their number *n'*. And if, in the fame manner, other degrees of copioufnefs be called c^2, c^3, &c. and their numbers be denoted by n^2, n^3, &c. then will the fum of every fet of points, multiplied by their refpective copioufnefs of emitting light, give us the quantity of light thrown out by the whole luminous body. That is, $L = c\,n + c'\,n' + c^2\,n^2$, &c.; and the mean copioufnefs of emitting light, of each phyfical point,

will be expreffed by $\dfrac{c\,n + c'\,n' + c^2\,n^2,\ \&c.}{N} = C.$

It is evident that the mean power, or copioufnefs of throwing out light, of every phyfical point in the luminous furface, multiplied by the number of points, muft give us the whole power of throwing out light, of the luminous body. That is $C\,N = L$.

I ought now to anfwer an objection that may be made to this theory. Light, as has been ftated, is tranfparent; and, fince the light of a point behind the furface of a flame will pafs through the furface, ought we not to take in its depth, as well as its fuperficial dimenfions? In anfwer to this, I recur to what has been faid with regard to the different powers of throwing out light, of the points of a luminous furface. For, as light muft be finally emitted through the furface, it is but referring all light arifing from the emiffion of points behind the furface, to the furface itfelf, and the account of emitted light will be equally true. And this will alfo explain why it has been ftated as probable, that different parts of the fame luminous furface may throw out different quantities of light.

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* See the Paper on the Nature and Conftruction of the Sun. Phil. Tranf. for 1795, page 46.

Since

Since, therefore, the quantity of light thrown out by any luminous body is truly repre-
fented by CN, and that an object is bright in confequence of light thrown out, we may fay
that brightnefs is truly defined by CN. If however, there fhould at any time be occafion
for diftinction, the brightnefs arifing from the great value of C, may be called the intrinfic
brightnefs; and that arifing from the great value of N, the aggregate brightnefs; but the
abfolute brightnefs, in all cafes, will ftill be defined by CN.

Hitherto we have only confidered luminous objects, and their condition with regard to
throwing out light. We proceed now to find an expreffion for their appearance at any
affigned diftance; and here it will be proper to leave out of the account, every part of CN
which is not applied for the purpofe of vifion. L reprefenting the whole quantity of light
thrown out by CN, we fhall denote that part of it which is ufed in vifion, either by the eye
or by the telefcope, l. This will render the conclufions that may be drawn hereafter more
unexceptionable; for, the quantity of light l being fcattered over a fmall fpace in propor-
tion to L, it may reafonably be looked upon as more uniform in its texture; and no fcruples
about its inequality will take place. The equation of light, in this prefent fenfe, therefore,
is $CN = l$.

Now, fince we know that the denfity of light decreafes in the ratio of the fquares of the
diftances of the luminous objects, the expreffion for its quantity at the diftance of the ob-
ferver D, will be $\dfrac{l}{D^2}$.

In natural vifion, the quantity l undergoes a confiderable change, by the opening and
contracting of the pupil of the eye. If we call the aperture of the iris a, we find that in
different perfons it differs confiderably. Its changes are not eafily to be afcertained; but
we fhall not be much out in ftating its variations to be chiefly between 1 and 2 tenths of an
inch. Perhaps this may be fuppofed under-rated; for the powers of vifion, in a room
completely darkened, will exert themfelves in a very extraordinary manner. In fome ex-
periments on light, made at Bath, in the year 1780, I have often remarked, that after ftay-
ing fome time in a room fitted up for thefe experiments, where on entering I could not
perceive any one object, I was no longer at a lofs, in half an hour's time, to find every
thing I wanted. It is however probable that the opening of the iris is not the only caufe of
feeing better after remaining long in the dark; but that the tranquillity of the retina, which
is not difturbed by foreign objects of vifion, may render it fit to receive impreffions fuch as
otherwife would have been too faint to be perceived. This feems to be fupported by telef-
copic vifion; for it has often happened to me, in a fine winter's evening, when, at mid-
night, and in the abfence of the moon, I have taken fweeps of the heavens, of four, five, or
fix hours duration, that the fenfibility of the eye, in confequence of the exclufion of light
from furrounding objects, by means of a black hood which I wear upon thefe occafions, has
been very great; and it is evident, that the opening of the iris would have been of no fer-
vice in thefe cafes, on account of the diameter of the optic pencil, which, in the 20 feet
telefcope,

telefcope, at the time of fweeping, was no more than ,12 inch. The effect of this increafed fenfibility was fuch, that if a ftar of the 3d magnitude came towards the field of view, I found it neceffary to withdraw the eye before its entrance, in order not to injure the delicacy of vifion acquired by long continuance in the dark. The tranfit of large ftars, unlefs where none of the 6th or 7th magnitude could be had, have generally been declined in my fweeps, even with the 20 feet telefcope. And I remember, that after a confiderable fweep with the 40 feet inftrument, the appearance of Sirius announced itfelf, at a great diftance, like the dawn of the morning, and came on by degrees, increafing in brightnefs, till this brilliant ftar at laft entered the field of view of the telefcope, with all the fplendour of the rifing fun, and forced me to take the eye from that beautiful fight. Such ftriking effects are a fufficient proof of the great fenfibility of the eye, acquired by keeping it from the light.

On taking notice, in the beginning of fweeps, of the time that paffed, I found that the eye, coming from the light, required near 20′, before it could be fufficiently repofed to admit a view of very delicate objects in the telefcope; and that the obfervation of a tranfit of a ftar of the 2d or 3d magnitude, would diforder the eye again, fo as to require nearly the fame time for the re-eftablifhment of its tranquillity.

The difficulty of afcertaining the greateft opening of the eye, arifes from the impoffibility of meafuring it at the time of its extreme dilatation, which can only happen when every thing is completely dark; but, if the variation of *a* is not eafily to be afcertained, we have, on the other hand, no difficulty to determine the quantity of light admitted through a telefcope; which muft depend upon the diameter of the object-glafs, or mirror; for, its aperture *A* may at all times be had by meafurement.

It follows, therefore, that the expreffion $\dfrac{a^2 l}{D^2}$ will always be accurate for the quantity of light admitted by the eye; and that $\dfrac{A^2 l}{D^2}$ will be fufficiently fo for the telefcope. For it muft be remembered, that the aperture of the eye is alfo concerned in viewing with telefcopes; and that, confequently, whenever the pencil of light tranfmitted to the eye by optical inftruments exceeds the aperture of the pupil, much light muft be loft. In that cafe, the expreffion $A^2 l$ will fail; and therefore, in general, if *m* be the magnifying power, $\dfrac{A}{m}$ ought not to exceed *a*.

As I have defined the brightnefs of an object to the eye of an obferver at a diftance, to be expreffed by $\dfrac{a^2 l}{D^2}$, it will be neceffary to anfwer fome objections that may be made to this theory. Optical writers have proved, that an object is equally bright at all diftances. It may, therefore, be maintained againft me, that fince a wall illuminated by the fun will appear equally bright, at whatfoever diftance the obferver be placed that views it, the fun alfo,

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at the diſtance of Saturn, or ſtill farther from us, muſt be as bright as it is in its preſent ſituation. Nay, it may be urged, that in a teleſcope, the different diſtance of ſtars can be of no account with regard to their brightneſs, and that we muſt conſequently be able to ſee ſtars which are many thouſands of times farther than Sirius from us ; in ſhort, that a ſtar muſt be infinitely diſtant not to be ſeen any longer.

Now, objections ſuch as theſe, which ſeem to be the immediate conſequence of what has been demonſtrated by mathematicians, and which yet apparently contradict what I aſſert in this paper, deſerve to be thoroughly anſwered.

It may be remembered, that I have diſtinguiſhed brightneſs into three different ſorts *. Two of theſe, which have been diſcriminated by *intrinſic* and *abſolute* brightneſs, are, in common language, left without diſtinction. In order to ſhew that they are ſo, I might bring a variety of examples from common converſation ; but, taking this for granted, it may be ſhewn that all the objections I have brought againſt my theory have their foundation in this ambiguity.

The demonſtrations of opticians, with regard to what I call *intrinſic* brightneſs, will not oppoſe what I affirm of *abſolute* brightneſs ; and I ſhall have nothing farther to do than to ſhew that what mathematicians have ſaid, muſt be underſtood to refer entirely to the intrinſic brightneſs, or illumination of the picture of objects on the retina of the eye ; from which it will clearly follow, that their doctrine and mine are perfectly reconcilable ; and that they can be at variance only when the ambiguity of the word brightneſs is overlooked, and objections, ſuch as I have made, are raiſed, where the word brightneſs is uſed as *abſo-lute*, when we ſhould have kept it to the only meaning it can bear in the mathematicians' theorem.

The firſt objection I have mentioned is, that the ſun, to an obſerver on Saturn, muſt be as bright as it is here on earth. Now by this cannot be meant, that an inhabitant ſtanding on the planet Saturn, and looking at the ſun, ſhould *abſolutely* receive as much light from it as one on earth receives when he ſees it ; for this would be contrary to the well known decreaſe of light at various diſtances. The objection, therefore, can only go to aſſert, that the picture of the ſun, on the retina of the Saturnian obſerver, is as *intenſely* illuminated as that on the retina of the terreſtrial aſtronomer. To this I perfectly agree. But let thoſe who would go farther, and ſay that therefore the ſun is *abſolutely* as bright to the one as to the other, remember that the ſun on Saturn appears to be a hundred times leſs than on the earth ; and that conſequently, though it may there be *intrinſically* as bright, it muſt here be *abſolutely* † an hundred times brighter.

The next objection I have to conſider, relates to the fixed ſtars. What has been ſhewn in the preceding paragraph, with regard to the ſun, is ſo intirely applicable to the ſtars, that it will be very eaſy to place this point alſo in its proper light. As I have aſſented to the demonſtration of opticians with regard to the brightneſs of the ſun, when ſeen at the

* See page 497.   † See the definition of *abſolute* brightneſs, page 497.

diſtance

diftance of Saturn, provided the meaning of this word be kept to the *intrinfic* illumination of the picture on the retina of an obferver, I can have no hefitation to allow that the fame will hold good with a ftar placed at any affignable diftance. But I muft repeat, that the light we can receive from ftars is truly expreffed by $\frac{a^2 l}{D^2}$; and that therefore their abfolute brightnefs muft vary in the inverfe ratio of the fquares of their diftances. Hence I am authorifed to conclude, and obfervation abundantly confirms it, that ftars cannot be feen by the naked eye, when they are more than feven or eight times farther from us than Sirius; and that they become, comparatively fpeaking, very foon invifible with our beft inftruments. It will be fhewn hereafter, that the vifibility of ftars depends on the penetrating power of telefcopes, which, I muft repeat, falls indeed very fhort of fhewing ftars that are many thoufands of times farther from us than Sirius; much lefs can we ever hope to fee ftars that are all but infinitely diftant.

If now it be admitted that the expreffions we have laid down are fuch as agree with well known facts, we may proceed to vifion at a diftance; and firft with refpect to the naked eye.

Here the power of penetrating into fpace, is not only confined by nature, but is moreover occafionally limited by the failure in brightnefs of luminous objects. Let us fee whether aftronomical obfervations, affifted by mathematical reafoning, can give us fome idea of the general extent of natural vifion. Among the reflecting luminous objects, our penetrating powers are fufficiently afcertained. From the moon we may ftep to Venus, to Mercury, to Mars, to Jupiter, to Saturn, and laft of all to the Georgian planet. An object feen by reflected light at a greater diftance than this, it has never been allowed us to perceive; and it is indeed much to be admired, that we fhould fee borrowed illumination to the amazing diftance of more than 18 hundred millions of miles; efpecially when that light, in coming from the fun to the planet, has to pafs through an equal fpace, before it can be reflected, whereby it muft be fo enfeebled as to be above 368 times lefs intenfe on that planet than it is with us, and when probably not more than one-third part of that light can be thrown back from its difk *.

The range of natural vifion with felf-luminous objects, is incomparably more extended, but lefs accurately to be afcertained. From our brighteft luminary, the fun, we pafs immediately to very diftant objects; for, Sirius, Arcturus, and the reft of the ftars of the firft magnitude, are probably thofe that come next; and what their diftance may be, it is well known, can only be calculated imperfectly from the doctrine of parallaxes, which places the neareft of them at leaft 412530 times farther from us than the fun.

* According to Mr. Bouguer, the furface of the moon abforbs about two-thirds of the light it receives from the fun. See *Traité d'Optique*, page 122.

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In

In order to take a second step forwards, we must enter into some preliminary considerations, which cannot but be attended with considerable uncertainty. The general supposition, that stars, at least those which seem to be promiscuously scattered, are probably one with another of a certain magnitude, being admitted, it has already been shewn in a former paper *, that after a certain number of stars of the first magnitude have been arranged about the sun, a farther distant set will come in for the second place. The situation of these may be taken to be, one with another, at about double the distance of the former from us.

By directing our view to them, and thus penetrating one step farther into space, these stars of the second magnitude furnish us with an experiment that shews what phænomena will take place, when we receive the illumination of two very remote objects, equally bright in themselves, whereof one is at double the distance of the other. The expression for the brightness of such objects, at all distances, and with any aperture of the iris, according to our foregoing notation, will be $\frac{a^2 l}{D^2}$; and a method of reducing this to an experimental investigation will be as follows :

Let us admit that α Cygni, β Tauri, and others, are stars of the second magnitude, such as are here to be considered. We know, that in looking at them and the former, the aperture of the iris will probably undergo no change; since the difference in brightness, between Sirius, Arcturus, α Cygni, and β Tauri, does not seem to affect the eye so as to require any alteration in the dimensions of the iris; a, therefore becomes a given quantity, and may be left out. Admitting also, that the latter of these stars are probably at double the distance of the former, we have D^2 in one case four times that of the other; and the two expressions for the brightness of the stars, will be l for those of the first magnitude, and $\frac{1}{4} l$ for those of the second.

The quantities being thus prepared, what I mean to suggest by an experiment is, that since sensations, by their nature, will not admit of being halved or quartered, we come thus to know by inspection what phænomenon will be produced by the fourth part of the light of a star of the first magnitude. In this sense, I think we must take it for granted, that a certain idea of brightness, attached to the stars which are generally denominated to be of the second magnitude, may be added to our experimental knowledge; for by this means, we are informed what we are to understand by the expressions $\frac{a^2 l}{\odot^2}$, $\frac{a^2 l}{\overline{\text{Sirius}|^2}}$, $\frac{a^2 l}{\overline{\beta\,\text{Tauri}|^2}}$

We cannot wonder at the immense difference between the brightness of the sun and that of Sirius; since the two first expressions, when properly resolved, give us a ratio of brightness of more than 170 thousand millions to one; whereas the two latter, as has been shewn, give only a ratio of four to one.

* Phil. Transf. for the year 1796, page 166, 167, 168.

† The names of the objects $\odot$, Sirius, β Tauri, are here used to express their distance from us.

What

What has been said will carry us, with very little addition, to the end of our unassisted power of vision to penetrate into space. We can have no other guide to lead us a third step than the same beforementioned hypothesis; in consequence of which, however, it must be acknowledged to be sufficiently probable, that the stars of the third magnitude may be placed about three times as far from us as those of the first. It has been seen, by my remarks on the comparative brightness of the stars, that I place no reliance on the classification of them into magnitudes *; but, in the present instance, where the question is not to ascertain the precise brightness of any one star, it is quite sufficient to know that the number of the stars of the first three different magnitudes, or different brightnesses, answers, in a general way, sufficiently well to a supposed equally distant arrangement of a first, second, and third set of stars about the sun. Our third step forwards into space, may therefore very properly be said to fall on the pole-star, on γ Cygni, ε Bootis, and all those of the same order.

As the difference, between these and the stars of the preceding order, is much less striking than that between the stars of the first and second magnitude, we also find that the expressions $\dfrac{a^2\, l}{\beta\,\text{Tauri})^2}$, and $\dfrac{a^2\, l}{\text{Polaris})^2}$, are not in the high ratio of 4 to 1, but only as 9 to 4, or $2\frac{1}{4}$ to 1.

Without tracing the brightness of the stars through any farther steps, I shall only remark, that the diminution of the ratios of brightness of the stars of the 4th, 5th, 6th, and 7th magnitude, seems to answer to their mathematical expressions, as well as, from the first steps we have taken, can possibly be imagined. The calculated ratio, for instance, of the brightness of a star of the 6th magnitude, to that of one of the 7th, is but little more than $1\frac{1}{3}$ to 1; but will we find by experience, that the eye can very conveniently perceive it. At the same time, the faintness of the stars of the 7th magnitude, which require the finest nights, and the best common eyes to be perceived, gives us little room to believe that we can penetrate much farther into space, with objects of no greater brightness than stars.

But, since it may be justly observed, that in the foregoing estimation of the proportional distance of the stars, a considerable uncertainty must remain, we ought to make a proper allowance for it; and, in order to see to what extent this should go, we must make use of the experimental sensations of the ratios of brightness we have now acquired, in going step by step forward: for, numerical ratios of brightness, and sensations of them, as has been noticed before, are very different things. And since, from the foregoing considerations, it may be concluded, that as far as the 6th, 7th, or 8th magnitude, there ought to be a visible general difference between stars of one order and that of the next following, I think, from the faintness of the stars of the 7th magnitude, we are authorized to conclude, that no star, eight, nine, or at most ten times as far from us as Sirius, can possibly be perceived by the natural eye.

* Phil. Transf. for the year 1796, page 168, 169.

The

The boundaries of vision, however, are not confined to single stars. Where the light of these falls short, the united lustre of sidereal systems will still be perceived. In clear nights, for instance, we may see a whitish patch in the sword-handle of Perseus*, which contains small stars of various sizes, as may be ascertained by a telescope of a moderate power of penetrating into space. We easily see the united lustre of them, though the light of no one of the single stars could have affected the unassisted eye.

Considerably beyond the distance of the former must be the cluster discovered by Mr. Messier, in 1764; north following H. Geminorum. It contains stars much smaller than those of the former cluster; and a telescope should have a considerable penetrating power, to ascertain their brightness properly, such as my common 10-feet reflector. The night should be clear, in order to see it well with the naked eye, and it will then appear in the shape of a small nebula.

Still farther from us must be the nebula between η and ζ Herculis, discovered by Dr. Halley, in 1714. The stars of it are so small that it has been called a nebula†; and has been regarded as such, till my instruments of high penetrating powers were applied to it. It requires a very clear night, and the absence of the moon, to see it with the natural eye.

Perhaps, among the farthest objects that can make an impression on the eye, when not assisted by telescopes, may be reckoned the nebula in the girdle of Andromeda, discovered by Simon Marius, in 1612. It is however not difficult to perceive it, in a clear night, on account of its great extent.

From the powers of penetrating into space by natural vision, we proceed now to that of telescopes.

It has been shewn, that brightness, or light, is to the naked eye truly represented by $\frac{c^2\ l}{D^2}$; in a telescope, therefore, the light admitted will be expressed by $\frac{A^2\ l}{D^2}$. Hence it would follow, that the artificial power of penetrating into space should be to the natural one as A to a. But this proportion must be corrected by the practical deficiency in light reflected by mirrors, or transmitted through glasses; and it will in a great measure depend on the circumstances of the workmanship, materials, and construction of the telescope, how much loss of light there will be sustained.

In order to come to some determination on this subject, I made many experiments with plain mirrors, polished like my large ones, and of the same composition of metal. The method I pursued was that proposed by Mr. Bouguer, in his *Traité d'Optique*, page 16,

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* See the catalogue of a second thousand of new nebulæ and clusters of stars, VI. 33, 34. Phil. Transf. Vol. LXXIX. page 251.

† In the *Connoissance des Temps* for 1783, No. 13, it is described as a nebula without stars.

fig.

fig. 3.; but I brought the mirror, during the trial, as close to the line connecting the two objects as possible, in order to render the reflected rays nearly perpendicular.

The result was, that out of 100 thousand incident rays, 67262 were returned; and therefore, if a double reflection takes place, only 45242 will be returned.

Before this light can reach the eye, it will suffer some loss in passing through the eye glass; and the amount of this I ascertained, by taking a highly polished plain glass, of nearly the usual thickness of optical glasses of small focal lengths. Then, by the method of the same author, page 21, fig. 5. I found, that out of 100 thousand incident rays, 94825 were transmitted through the glass. Hence, if two lenses be used, 89918; and, with three lenses, 85265 rays will be transmitted to the eye.

Then, by compounding, we shall have, in a telescope of my construction with one reflection, 63796 rays, out of 100 thousand, come to the eye. In the Newtonian form, with a single eye lens, 42901; and, with a double eye glass 40681 will remain for vision.

There must always remain a considerable uncertainty in the quantities here assigned; as a newly polished mirror, or one in high preservation, will give more light than another that has not those advantages. The quality of metal also will make some difference; but, if it should appear by experiments, that the metals or glasses in use will yield more or less light than here assigned, it is to be understood that the corrections must be made accordingly.

We proceed now to find a proper expression for the power of penetrating into space, that we may be enabled to compare its effects, in different telescopes, with that of the natural eye.

Since then the brightness of luminous objects is inversely as the squares of the distances, it follows, that the penetrating power must be as the square roots of the light received by the eye.

In natural vision, therefore, this power is truly expressed by $\sqrt{\overline{a^2\,l}}$; and, since we have now also obtained a proper correction x, we must apply it to the incident light with telescopes.

In the Newtonian and other constructions where two specula are used, there will also be some loss of light on account of the interposition of the small speculum; therefore, putting b for its diameter, we have $\overline{A^2 - b^2}$ for the real incident light. This being corrected as above, will give the general expression $\sqrt{x\,l \times \overline{A^2 - b^2}}$ for the same power in telescopes. But here we are to take notice, that in refractors, and in telescopes with one reflection, b will be $= o$, and therefore is to be left out.

Then, if we put natural light $l = 1$, and divide by a, we have the general form $\dfrac{\sqrt{x \cdot \overline{A^2 - b^2}}}{a}$ for the penetrating power of all sorts of telescopes, compared to that of the natural eye as a standard, according to any supposed aperture of the iris, and proportion of light returned by reflection, or transmitted by refraction.

In the following inveftigation we fhall fuppofe $a = 2$ tenths of an inch, as being perhaps nearly the general opening of the iris, in ftar-light nights, when the eye has been fome moderate time in the dark. The value of the corrections for lofs of light will ftand as has been given before.

(To be concluded in our next.)

VI.

Some Obfervations on the Head of the Ornithorhynchus paradoxus. *By* EVERARD HOME, *Efq. F. R. S.**

THE fpecimens of this extraordinary animal which have been fent to Europe, have been deprived of the internal parts, and the fkins are moftly dried, and but badly preferved. Such imperfect fpecimens have raifed the curiofity of the naturalift, and excited the ardor of the anatomift, without fatisfying their enquiries.

It was natural, under thefe circumftances, to referve any obfervations which had been made upon this newly difcovered quadruped, till the entire animal fhould be brought home preferved in fpirit, and enable us to examine the ftructure of its different organs; but, finding that Profeffor Blumenbach has been led to believe that it was an animal without teeth, an opinion which muft have arifen from the imperfect ftate of the fpecimen he examined, it appeared highly proper to do away the miftake, and lay before this learned Society, fuch obfervations refpecting the head of this extraordinary animal, as I have been enabled to make.

My opportunities of examining the *Ornithorhynchus* were procured through Sir Jofeph Banks; who permitted me to have drawings made from the fkin of one of a very large fize, and which, from having been preferved in fpirit, was more perfect than any of the dried fpecimens.

Any general defcription of the beak of this animal, which is its moft confpicuous peculiarity, becomes unneceffary, as the accompanying drawings will give a fufficiently correct idea of the outward appearances, to anfwer the prefent purpofe.

It was not permitted to examine the head anatomically; but a fmaller dried fpecimen, received from Sir Jofeph Banks, furnifhed me with the following obfervations.

The beak of the *Ornithorhynchus*, when it is curforily examined, appears fo ftrongly to refemble that of the duck, as to lead to the belief of its being calculated for exactly the fame purpofes; it will however be found to differ materially from it, in a variety of circumftances.

The beak is found, upon examination, not to be the animal's mouth, but a part added to the mouth, and projecting beyond it.

* Philofophical Tranfactions 1800.

The

The cavity of the mouth is situated as in other quadrupeds, and has two grinding teeth on each side, both in the upper and lower jaw; but, instead of incisor teeth, the nasal and palate bones are continued forwards, lengthening the anterior nostrils, and forming the upper part of the beak; and the two portions of the lower jaw, instead of terminating at the symphisis, where they join, become two thin plates, and are continued forwards, forming the under portion of the beak.

This structure differs materially from the bill of the duck, and indeed from the bills of all birds, since in them, the cavities of the nostrils do not extend beyond the root of the bill; and, in their lower portions, which correspond to the under jaw of quadrupeds, the edges are hard, to answer the purpose of teeth, and the middle space is hollow, to receive the tongue. But, in this animal, the two thin plates of bone are in the centre; and the parts which surround them are composed of skin and membrane, in which a muscular structure probably is inclosed.

The teeth have no fangs which sink into the jaw, as in most quadrupeds, but are imbedded in the gum; and have only lateral alveolar processes, from the outer and inner edges of the jaw, to secure them in their places, but no tranverse ones between the two teeth.

The tongue is extremely short, not half an inch long; and the moveable portion not more than a quarter of an inch; the papillæ on its surface are long, and of a conical form. When the tongue is drawn in, it can be brought intirely into the mouth; and, when extended, can be projected about a quarter of an inch into the beak.

The organ of smell, in this animal, differs, in some particulars, from that of quadrupeds in general, as well as of birds. The external openings of this organ are placed nearly at the end of the beak, there being only the lip beyond them; while the turbinated bones are in the same relative situation to the other parts of the skull as in quadrupeds; by which means, there are two cavities the whole length of the beak, superadded to the organ of smell.

The turbinated bones in each nostril are two in number, and are distinct from each other. That next the beak is the longest, has a more variegated surface than in the duck, and has the long axis in the direction of the nostril; the posterior one is short, projects farther into the nostril, and the ridges are in a tranverse direction.

The posterior nostrils do not open directly under the turbinated bones, as in the duck, but about an inch farther back, and are extremely small; the cavities of the nose, in this animal, are therefore uncommonly extensive; they reach from the end of the beak nearly to the occiput.

The beak itself is formed by the projecting bones already mentioned, covered with a smooth black skin, which extends some way beyond the bones, both in front and laterally, forming a moveable lip. This lip is so strong, that, when dried or hardened in spirit, it seems to be rigid; but, when moistened, is very pliant, and, as has been already mentioned, has probably a muscular structure. The under portion of the beak has a lip

equally

equally broad with the upper : this has a ferrated edge ; but the ferræ are confined to the foft part, not extending to the membrane covering the bone, and are not met with in the upper one. The extent of the lips beyond the bones, is diftinctly marked in the drawings.

There is a very curious tranverfe fold of the external black fmooth fkin, by which the beak is covered, projecting all round, exactly at that part where the beak has its origin. Its apparent ufe feems to be to prevent the beak being pufhed further into the foft mud, in which its prey may lie concealed, than up to this part, which is fo broad that it muft completely ftop its progrefs.

The nerves that fupply the beak, in their general courfe, fize, and number, feem very clofely to correfpond with thofe of the bill of the duck.

The cavity of the fkull bears a greater general refemblance to that of the duck than of quadrupeds : there is a very uncommon peculiarity in it, which is, that there is a bony falx of fome breadth, but no bony tentorium. This is met with in no quadruped that I know of : it is found in a fmall degree in fome birds, as the fpoon-bill, and the parrot ; but not at all fo as to refemble the falx in this animal.

The orifice of the eye lids is uncommonly fmall, for the fize of the animal ; but the eye itfelf was not in a ftate to be examined.

The external opening of the ear was fo fmall as not readily to be perceived : it is fimply an orifice ; but the meatus enlarges confiderably beyond the fide of the opening, and paffes fome way under the fkin before it reaches the organ, which in this fpecimen had been deftroyed. In the duck, the orifice leading to the ear is very large, when compared with the opening in this animal.

When we confider the peculiarities in the ftructure of the nofe of this animal, which lives in water, it is natural to conclude the organ is fitted to fmell in water, and the external noftrils are fo placed, to enable it to difcover its prey by the fmell; for that purpofe, the animal can apply its nofe, with great eafe, to the fmall receffes in which its prey may be concealed.

The ftructure of the beak is not fuch as enables it to take a firm hold ; but, when the marginal lips are brought together, the animal will have a confiderable power of fuction, and in that way may draw its prey into its mouth.

EXPLANATION OF THE FIGURES.

PLATE XXII.

Fig. 1. A view of the beak, to fhow the fituation of the openings of the external noftrils, marked *a a.*

Fig. 2. Another view of the beak, expofing the under portion.

Fig. 3. A lateral view, to fhow the opening of the lips, and the fituation of the eye and ear. *a.* The eye *b.* The ear.

Fig.

Fig. 4. A view of the upper jaw and palate, to fhew the teeth in their fituation.

Fig. 5. A fimilar view of the under jaw.

Fig. 6. The bones which form the beak delineated, and the foft furrounding parts only marked in outline.

Fig. 7. A fimilar view of the bones forming the lower portion of the beak.

VII.

Inflances of fufpended Animation in Vegetables. By Mr. JOHN GOUGH.

To Mr. NICHOLSON.

S I R,

Kendal, Jan. 20, 1801.

MR. Baker and Sig. Spalanzani have obferved and defcribed various aquatic ani-malculæ which fuffer apparent death when deprived of water, but refume their figure and all their natural functions upon being reftored, at a future period, to their native element. The latter gentleman has alfo extended his enquiries, in hopes of finding fimilar examples in the vegetable kingdom, but with little fuccefs; one or two fpecies of tremella being all the plants he difcovered anfwering his expectations.

Notwithftanding the great induftry of this celebrated naturalift, a plant more perfect in its ftructure than the gelatinous fubftances compofing the genus tremella, appears to have efcaped his notice; which poffeffes in a high degree the faculty of dying apparently, and reviving again, in order to accommodate itfelf to the viciffitudes of its fituation.

The plant upon which nature, or more properly, the author of nature, has beftowed this remarkable property, is the lemna minor, or common duck's meat. Some remarks which I made accidentally on a pond covered with duck's meat, in the beginning of July, 1797, induced me to imagine that this little plant could hardly preferve its exiftence during a long fit of dry weather; unlefs it refembles the wheel animal, in fuffering a kind of temporary death when deprived of water; and afterwards experiencing what may be called a refur-rection to a fecond life, upon receiving a frefh fupply of its native fluid.

In order to difcover in what degree the lemna poffeffes a power of accommodating itfelf to the inconveniences of its fituation, the following experiments were made. A quantity of the plant was expofed for four or five hours to the fun, in which time it became per-fectly dry; a number of leaves which had undergone the preceding procefs, and appeared dead and withered, were placed, at the end of two days, in a jar of frefh water, where they revived immediately, and continued apparently in a healthy ftate for three weeks; when

when an end was put to the experiment, from a conviction, that putrefaction would have deſtroyed the plants in a much ſhorter time, had the indications of life exhibited by them proved deluſive.

The foregoing fact appeared to me, at the time of its diſcovery, a ſurpriſing inſtance of ſuſpended animation in a vegetable; but a ſimilar trial, made at a ſubſequent period, afforded a more extraordinary proof of the exiſtence of an accommodating principle in the ſame plant. A number of leaves, which were dried with thoſe uſed in the laſt experiment, had been accidentally preſerved in a ſmall box, from the beginning of July, 1797, to the end of March, 1800. Being convinced by trial that the plants were ſtill living, notwith-ſtanding their long continuance in what may be called a torpid ſtate, I placed ſeveral of them in a glaſs jar, which was furniſhed with a ſmall ſyphon, for the purpoſe of changing the water from time to time. The plants, treated in this manner, not only revived after remaining in a ſtate of apparent death for more than two years, but retained ſufficient vigour to produce complete parts of fructification in Auguſt, which is the proper flowering ſeaſon of this ſpecies of lemna.

Various ſpecies of conferva and tremella might be pointed out as endued with the pro-perty under conſideration; but I ſhall only mention one circumſtance of the kind at preſent. When ponds dry up in the ſummer, the ſediment left behind forms a paper-like ſubſtance on their bottoms: if a piece of this ſubſtance be put into water, it turns green in a few minutes, on account of the different confervæ, which enter into the compoſition of this natural paper, and revive upon their being ſaturated with their proper fluid.

The principle of accommodation that forms the ſubject of the preſent eſſay, is not con-fined to aquatic vegetables; for in the courſe of my examination of the ſubject, I found it in the vegetating germs of the feſtuca vivipara; ſeveral of which produced perfect plants after being kept dry ſome of them four, and others five weeks. The knowledge of this circumſtance, and the information I received that barley does not loſe the power of ſprout-ing by being malted, determined me to enquire more carefully into the nature of ſeeds in this reſpect, and I fixed upon peaſe for the experiment. With this view I permitted a number of them, which had been moiſtened for the purpoſe, to germinate for three days, and then dried them again, by the application of a gentle heat: on ſoaking them afreſh in water, and expoſing them to the air, the proceſs of germination recommenced, not by the production of new parts, for the former ſprouts revived, and advanced in growth as if no interruption had been previouſly offered to them. A repetition of the laſt experiment, made with the ſame peaſe, gave a ſimilar reſult; but a third attempt failed, becauſe the ſeed lobes parted, their mutual connection being nearly broken before the preſent trial, by the enlargement of the germs, and the treatment they had received.

The preceding experiments ſeem to ſuggeſt and authorize the following concluſions.

1ſt. The analogy which has been ſhewn in many caſes to connect vegetables with animals, becomes more extenſive the farther we carry our reſearches; of the truth of which the preſent eſſay affords an example.

4

2d.

2d. This analogy appears to be a neceffary provifion in the inftance under confideration, which enables a certain defcription of organic beings belonging to both kingdoms, to accommodate themfelves to the viciffitudes of a fituation variable in its nature, and to preferve not only the life of the individual, but perhaps the exiftence of the fpecies, by fubmitting to a temporary death, or more properly a compleat fufpenfion of animation.

The feeds of land plants, which germinate on the furface of the ground, being expofed to the inconveniences of the weather, in common with aquatic plants growing in fhallow ponds, are provided with the fame principle of accommodation to infure their fafety. The exiftence of this falutary property is confirmed by experiment; but our knowledge of the nature of vegetables is as yet too limited to difcover the fingularity of conftitution from which it refults; we fee the wifdom of the contrivance, but cannot inveftigate the caufe.

4th. The peculiarity of conftitution, which diftinguifhes viviparous plants appears to be this: germs of this defcription begin to fprout when their feed lobes have imbibed a quantity of humidity too fmall to fuffice in common cafes; for which reafon they frequently vegetate on the ftem of the parent plant.

This opinion is corrobated by the following facts:

ift. All feeds germinate when placed in the air, having their lobes faturated with water: on the contrary, they begin to rot when the quantity abforbed by them is too little.

2d. The proportion of water required to produce faturation, in equal weights of different feeds, is different.

3d. Some plants, fuch as the polygonium viviparum, are only viviparous in wet feafons.

<div style="text-align:center">

I am, SIR,

Your's, &c.

JOHN GOUGH.

</div>

<div style="text-align:center">

VIII.

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Extract of a Letter from Doctor G. M. to Dr. William Babington, dated Freiberg, Dec. 17, 1800. *On the State of Galvanifm and other fcientific Purfuits in Germany. Communicated by Dr.* BABINGTON.

MY DEAR DOCTOR,

I HAVE long been waiting for an opportunity of making you fome return for the very valuable and interefting intelligence in your laft, and now feize the earlieft occafion of giving you fome account of the galvanic labours in Germany; not but that I think it

<div style="text-align:right">poffible</div>

poffible that you have already anticipated them in England : they are, however, of fuch an important nature, that I think it advifeable to give them to you on chance.   The principal galvanic difcoverer here is a young man, called Ritter, at Jena, in Saxony : about two years fince he publifhed the refult of his almoft innumerable experiments, in which he eftablifhed all its laws, and anticipated almoft all the newer experiments.   Unfortunately the book was written very obfcurely, and was ftill more obfcured by the language of the newer philofophy ; and the author having neither enemies who were interefted in bringing him into difcredit, nor friends who were defirous of drawing him out of obfcurity, his work remained little known and lefs noticed ; he, therefore, gave up in a great meafure the fubject, and applied himfelf to other purfuits, till a few months fince the general intereft beftowed upon the fubject reanimated his zeal.   Having fatisfied himfelf that by paffing the galvanic influence by means of gold wires through water, the oxygen gas alone was given off at the extremity of the wire connected with the zinc end, and the hydrogen gas from that communicating with the filver end of the pile, he reafoned thus.   If each particle of water be compofed of a particle of hydrogen and another of oxygen, how can the cor- refponding gafeous particles firft make their appearance at the extremities of the re- fpective wires, placed at one inch diftance ? can it be fuppofed that the particle of water is at one and the fame time at the extremity of each wire ?—this fuggefted to him to inter- pofe fome fubftance between the extremities of the wires, which was at the fame time ca- pable of conducting the galvanic influence, and of remaining perfectly unaltered by it. Such a fubftance prefented itfelf in highly concentrated fulphuric acid : in a tube of the ad- joining figure 6, plate XXI. he poured a portion of the acid, the upper parts he filled carefully with water, in which, by means of cork, he introduced two gold wires within a fhort diftance of the furface of the acid.   On fuffering the galvanic influence to pafs through it, the water in the leg *a* being connected with the zinc afforded oxygen gas alone, while that in the leg *b* communicating with the filver, gave hydrogen gas ; and this evolution of gas, as in all other cafes, continues fo long as the galvanic chain remains uninterrupted.   To obviate the objec- tions which might be made, that perhaps one or other of the gafes made its way through the acid, he contrived the following experiment.   He filled two tubes *a* and *b*, fig. 5, half with acid, and the remainder with water ; in each two gold wires were introduced, in fuch a manner that the extremities of the upper were furrounded with water, while the lower remained immerfed in acid, and were connected at *z*.   The effect was precifely the fame : fo long as the tubes ftood in the chain, and the wire *a* was connected with the zinc, the water in that tube furnifhed oxygen gas alone, and the water in *b* hydrogen gas ; but no fooner was the manner of connection reverfed, than the reverfe took place.   Laftly, to re- move the objection that thofe two proceffes are dependant on, and infeparable from, one another, he placed a fingle tube filled in the above manner in the chain.   As long as the wire immerfed in water was connected with the zinc, oxygen gas alone prefented itfelf ; but when it was turned towards the filver, hydrogen gas ; thus it is proved, that water under certain circumftances, may be wholly converted into oxygen gas, and under others, into hydrogen

<center>3</center>

<div align="right">gas,</div>

gas: the rationale of this phænomenon is as yet in obscurity. One philosopher accounts for it thus; that water + light gives oxygen gas, and water + heat hydrogen gas; when these two gases are decomposed, the ponderable part presents itself once more under the form of water, and the fire escapes. Others propose the following; that oxygen gas is water + positive electricity; and hydrogen gas, water + negative electricity. This seems countenanced by numerous observations, among others by some already made in England, viz. that the electrometer is affected negatively on being presented to the zinc extremity of the galvanic battery, and positively at the silver end. I suppose I need not tell you that the atomistic system is almost entirely exploded in Germany, and the dynamic substituted in its stead; consequently, in place of the term *matter*, the phrase *spacefilling* is employed. Thus, according to the newest sect of natural philosophy, water is the only spacefilling in nature that gravitates, and when united to electricity, furnishes the principal gases as seen above; but when to water, magnetism is superinduced, their cohesion is the consequence, and the different solids are the products: the varieties in these result from the predominance of carbon or azote; the former being water + the north pole of the magnet, and the latter water + the south pole. Water itself is perfect indifference, being neither electric nor magnetic, yet the fruitful parent of all!—This theory, certainly very simple, and possibly very sublime, will probably appear as extraordinary to you, who are sunk in the quagmire of matter, as it did to me. You are probably well acquainted with the progress the Parisian chemists have made in the decomposition of the alkalis and alkaline earths: their compound nature has ever been the opinion of Mr. Werner, from the incontrovertible facts which geognostic observations furnish; there can also be no doubt but the silex, argil, &c. are equally compounded. I have lately repeated Guyton's experiments with the alkaline solutions of silex and argil: when these are well concentrated and mixed in equal proportion, a firm, gelatinous, opulescent, mass results in a very few minutes; this is perfectly insoluble in water, yet soluble in acids, whether concentrated or diluted, nay, even in distilled vinegar, and yet consists of both silex and argil: here, therefore, the properties of the silex must be considerably altered. This must render all analysis with alkalis suspicious, and shows on what fallacious grounds the proud dominion of chemistry rests, which she has exercised so long in such an arbitrary and overbearing manner in the mineral kingdom. * * * * *

IX.

On the Chemical Effects of the Pile of Volta. By a Correspondent.

LETTER II.

To Mr. NICHOLSON.

SIR,

I HOPE I shall not trespass too much on your time by insisting a little further on the difficulty, which I took the liberty of submitting to you respecting the decomposition of water by the Pile of Volta, and which you honoured by inserting in your truly valuable Journal. I am not at all disposed to believe that chemical effects, dependant on each other, should always take place close to each other; although I contend that we should be able to ascertain the cause which produces a deviation from local proximity.

That some distance of time intervenes between chemical effects, some experiments, in which I have been engaged for a considerable time, have fully convinced me. I have long thought, that as Nature produces some of her most curious and perfect combinations by very slow and regular processes, that as she destroys her finest productions when she operates at high temperatures in volcanic countries, where every thing alike assumes the forms of cinders, ashes and lava, that the usual modes of operating chemically are defective in as far as we wish, by the application of great degrees of heat and sudden strong attractions by mixture, to produce immediate results in new combinations and new substances. I have succeeded in reversing many acknowledged attractions, by only giving time for the proper arrangement of the particles. I shall mention one decomposition which requires a very considerable time for its production. Why, if the attractions tend one way, they do not instantly arrange themselves, I cannot at present determine. Having found that a solution of sulphuret of pot-ash attracts oxygen from the atmosphere, I thought it might be employed for the decomposition of the carbonic acid. With this view, I inclosed in a bottle a quantity of carbonate of lime, and a strong solution of sulphuret of pot-ash. I shook them well together, but I did not observe any change after a considerable time. The bottle, however, remained in my room closely stopped for six months, when I observed that the sides of it were covered as high as the solution reached with a black powder, which had the properties of charcoal. I could produce many other instances to prove that combinations will take place, as it were spontaneously, if time be allowed for proper arrangement. Organized substances in particular afford very striking instances. Phosphorus, though so strongly attached to oxygen in animal substances, is nevertheless frequently separated by a slow decomposition.

In the experiment of the vegetations of a metal by means of zinc, which you produce as a parallel fact to the decomposition of water from the pile, I find some modes of reasoning which

which feem fufficient to account for the phænomenon. The laws of cryftallization will apply to explain why the lead affumes a regular figure, as other metals, for inftance tin, affume their own forms. You may alfo have remarked that, when a metal is diffolving in an acid, the folution defcends through the folvent (as happens in the oxidation of copper by wires of that metal in water, and connected with the pile) according to the increafed fpecific gravity of the newly formed compound. It is alfo known, that there are different degrees of faturation between metals and acids, which may take place as the heavier fluid defcends; for you will obferve that the vegetations generally take place downwards. I have obfervcd that, when zinc is placed in a faturated folution of lead or tin, a very con-fufed mafs of precipitated metal forms very foon around it, and that, in order to produce fine cryftallizations, it is neceffary to add a large proportion of water : this feems to fhew that the defcent of the new metallic folution has fome fhare in producing the effect. But in thefe experiments continuity is preferved, and the fame effects are produced in every cryftallization of any falts.

It appears to me therefore that the feparation of the two gafes from the connection with the pile of Volta, is an anomalous effect in chemiftry, as yet unexplained according to the doctrine of the decompofition of water. It has been a long time known that pofitive and negative electricity have different properties; it is now clearly fhewn that they produce different chemical effects. I do not fee how, in ftrict philofophy, we are warranted in faying more of the experiment with the pile, than that one kind of electricity and water produce one kind of gas, and the other another gas. Is it not an affumption in this ex-periment to fay, that the bafes of the gafes are the component parts of water? I could ftart other difficulties even where there appears to be a clearer proof of the decompofition of water, but, fearful left I fhould have already too much impofed upon your kindnefs, I haften to fubfcribe myfelf,

Your obedient humble Servant,

AN EXPERIMENTALIST.

January 20, 1801.

X.

Analyfis of the Mellite, or in German, Honigflein. By CIT. VAUQUELIN [*].

THE analyfes which Meffrs. Abich and Lampadius have given of this interefting foffil are well known. The firft obtained from one hundred parts, 16 carbonate of alumine, 4 carbone, 3 oxyde of iron, 40 carbonic acid, 28 water of cryftallization, having a fmell of bitter almonds, and 5.5 of Naptha.

[*] Annales de Chimie XXXVI. 203.

The

The ſecond had for his reſult 86.4 of charcoal, 3.5 petroleum, 2 ſilex, and 3 of water of cryſtallization. An enormous difference *.

Mr. Abich propoſes on account of the incombuſtibility of the honigſtein, that it ſhould be removed from the claſs of combuſtibles, and placed in that of the alumines.

But Mr. Klaproth, whoſe labours are intitled to the higheſt confidence, wrote me ſeveral months ago, that he has diſcovered that this pretended ſtone is compoſed of a peculiar vegetable acid united to alumine.

M. Abildgnaard, to whom I am indebted for many intereſting minerals of Norway, ſent me ſome decades ago, by favor of M. Mantey, a ſmall quantity of honigſtein, of which he had deſigned part for analyſis : and I haſtened to comply with his wiſh in that reſpect.

Deſcription of the Honigſtein.

This ſubſtance has a light yellow colour, which has cauſed it to obtain the name of mellite, or honey ſtone. It is uſually cryſtallized in an octahedron, of which the angles are ſometimes replaced by faces, owing to laws of diminution, of which the laws have not yet been limited.

Its ſpecific gravity is not confiderable. It is according to M. Abich 1. 666. It is found in Thuringia in ſtrata of foſſil bituminous wood.

I. *Chemical Character of Honigſtein.*

When expoſed to fire with the contact of air it becomes white, and burns without becoming ſenſibly charred ; it leaves a white matter, which produces a ſlight efferveſcence with acids. It has no ſenſible taſte ; though if it be kept for ſome time on the tongue, it occaſions a ſlight impreſſion of acidity.

II. ANALYSIS.

I took two grammes of the honigſtein reduced to powder, and mixed them with four grammes of ſaturated carbonate of pot-aſh diſſolved in a ſufficient quantity of water. As ſoon as the mixture was made, an efferveſcence of confiderable activity followed, without the aſſiſtance of foreign heat ; but to accelerate, and more effectually to complete the decompoſition of this ſubſtance, I ſlightly heated it on a ſand bath.

The filtered liquor after cooling was of a browniſh colour, and left on the filtering paper a brown matter, which when dried in the ſun weighed very nearly 0.8 of a gramme.

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* If M. Lampadius operated upon the ſame ſubſtance as Mr. Abich and myſelf have analyzed, it is impoſſible he could have had 86.4 of carbone ; for in forty parts of carbonic acid, and four of carbone, obtained by Mr. Abich, there is not the matter to form eighty-ſix of carbone : and as from my analyſis it appears to me that there is not more than 55 per cent. of real acid in the honigſtein, it is evident that it cannot afford eighty-ſix of charcoal. It muſt therefore neceſſarily follow, either that Mr. Lampadius did operate upon another ſubſtance, or that he did not heat it ſufficiently to decompoſe the acid, if he did in fact operate upon the honigſtein.

This.

III.

This 0.8 of a gramme of brown matter calcined in a crucible became white, and then weighed only 0.33 gramme. When mixed with diluted fulphuric acid, it produced a flight effervefcence. The mixture was afterwards evaporated to drynefs.

I expected from the information of Mr. Klaproth, that by the addition of water, nearly the whole of this matter would have been diffolved; but the contrary happened, for the greateft part remained in the form of a white powder.

The fluid being evaporated to the point, at which there remained at moft not more than three or four grammes, I added one drop of fulphate of pot-afh, and obtained, by leaving it to fpontaneous evaporation, about 0.1 gramme of alum, mixed with a fmall quantity of fulphate of lime.

I afterwards examined what might be the nature of the fubftance, which after the treatment with fulphuric acid remained infoluble in water.

For this purpofe I boiled it in a folution of carbonate of pot-afh; then filtered, wafhed, and examined it as follows:

1. The muriatic acid, diluted with two parts of water, attacked it with a flight effervefcence; but the folution did not become clear; on the contrary it remained fomewhat milky.

2. This filtered liquor afforded with ammonia a precipitate refembling in its tranfparency that which alumine affords by the fame means; but it was not entirely foluble in pot-afh.

But the portion diffolved by the pot-afh was the moft abundant, and prefented all the characters of alumine: for when combined with the fulphuric acid it afforded alum. The caufe why this portion did not remain combined with the fulphuric acid was therefore, moft probably, that it had been too much heated towards the end of the deficcation.

The fluid from which the ammonia had feparated the alumine laft mentioned, afforded alfo flight precipitates by the carbonate of pot-afh, and the oxalate of ammonia; which proves that it contained a fmall portion of lime.

The portion of matter which was not diffolved by the pot-afh, weighed at moft 0.1 gramme, and appeared to me to be filex.

The honigftein therefore contains a fmall portion of lime and of filex.

After having afcertained the matter which compofed my refidue, I directed my operations to the liquid which muft have contained the acid of mellite combined with pot-afh; and with the expectation that it would yield its bafis to the mineral acids, I added to a portion of the liquid fome drops of nitric acid, which produced a very flight effervefcence, and difengaged a very fmall portion of brown flocculent matter. Some hours afterwards, that which I fufpected came to pafs; the acid of the mellite cryftallized in the form of fmall fhort prifms with brilliant faces.

3·   Finding

Finding that this method would ſucceed for ſeparating the whole of this acid from the pot-aſh, I ſlightly heated the whole of the fluid, and added nitric acid till there was an exceſs perceptible to the taſte. I then filtered to ſeparate the brown flocculent matter, and obtain the acid in a ſtate of greater purity; and in effect I obtained about 1.34 grammes, which was white enough, though ſtill of a yellow tinge. The following are the properties which preſented themſelves on its mixture with different ſubſtances.

1. This acid has brilliant facets, conſiderable hardneſs, ſlight acid taſte, accompanied with bitterneſs, which may ariſe from ſome portions of bitumen ſtill attached to it, and to which it owes its yellow colour.

2. A portion of this acid expoſed to the flame of the blow pipe, gave at firſt a few ſcintillations like thoſe of ſalt-petre; after which it ſwelled up, and left a ſubſtance that ſoon penetrated the charcoal.

3. When heated in a covered crucible of platina it ſwelled up at firſt, afterwards became charred, without affording an oily flame, and left a light coal which is very alkaline *. This acid was therefore combined with a certain quantity of the pot-aſh, notwithſtanding the exceſs of nitric acid which had been added to its ſolution. The ſame effect alſo takes place with the tartareous and oxalic acids, which by ſimilar treatment are made to paſs to the ſtate of acidules.

4. This acid is ſparingly ſoluble; but I have not exactly determined the quantity of water which it requires.

5. Several grammes of the ſame acid, diſſolved in water, were mixed, 1. with a ſolution of lime: in which a white flocculent precipitate was immediately formed, that ſoon fell to the bottom of the fluid. 2. With a ſolution of ſulphate of lime, in which a light granulated and cryſtallized precipitate was found, that left the water ſtill in a certain degree tranſparent; but which was increaſed and rendered flocculent by the addition of one drop of ammonia †. 3. With the ſolution of muriate of barytes it affords little precipitate the firſt moment, but ſoon afterwards a ſhower of cryſtals in needles. 4. With the ſolution of ſilver it gives a white ſilky precipitate, which ſhines like a ſolution of ſoap: and ſometime afterwards it ſubſides in the form of powder. 5. With the nitric ſolution of lead it affords a white pulverulent very heavy precipitate. 6. With that of mercury a white precipitate, which a ſingle dop of ammonia turns black.

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* This acid muſt not be confounded with the acidulous tartrite of pot-aſh by this property. For the latter ſwells up much more, and emits at the time of its decompoſition a denſe fume, of a peculiar and very diſtinguiſhable odor.

† The acidulous tartrite of pot-aſh does not immediately produce a precipitate in the ſolution of ſulphate of lime; but twenty-four hours afterwards there is found in the mixture cryſtals with very brilliant facets, compoſed of lime and tartareous acid. Though cryſtallized, this tartrite of lime does not reſemble the combination formed with the acid of honigſtein, and the ſame baſe. It differs from it in ſwelling up in the fire, whereas that is decompoſed without ſwelling, in which reſpect it has analogy with the oxalate of lime.

From

From the result of these experiments we see, that the acid of the honigstein has many properties analogous to those of the acid of sorrel, and by the comparison I have made, I have perceived no difference but the following: 1. The precipitate which it causes in the solution of sulphate of lime is less speedily manifested, and is crystalline, instead of being pulverulent, like that which is formed by the acidulous oxalate of pot-ash. 2. It appears less acid to the taste than the acidulous oxalate of pot-ash; but this may depend upon my not having added enough of nitric acid to the combination with pot-ash, to deprive it of a sufficient quantity of the alkali; and 3. It swells up rather more by heat than the acidulous oxalate of pot-ash.

In other respects the sublimed salt, the great quantity of carbonic acid, and the little coal which the mellite affords by distillation, are facts that seem to unite in proving the identity of the two acids: for the habitude of the salt of sorrel by fire is the same *.

The octahedral figure of the honigstein appears also to have analogy with that of the oxalic acid, which is a rectangular prism terminated by pyramids with four faces. It is only necessary to examine the inclination of these faces in order to obtain a certainty.

Nevertheless, as I have had no more than about 1.34 gramme of this acid at my disposal, I have not been able to subject it to all the trials which would have been required to demonstrate in the most absolute manner its identity in all respects with the oxalic acid. For though they have presented similar phænomena in all the comparative trials I have made, it is possible that by other that might yet be made, there might be found a single difference which would destroy the similitude.

It is therefore chiefly with a view to engage the chemists of Germany, where this substance is more common, to recommence the analysis of honigstein, and to compare its acid in every possible respect with those of sorrel that I publish this notice.

If my opinion should be confirmed by new experiments, we shall then possess the oxalic acid in all the three kingdoms; namely, in the state of acidulous oxalate of pot-ash in several kinds of vegetables; in that of oxalate of lime in the human urinary calculi; and lastly, in the state of oxalate of alumine in the bowels of the earth among bituminous wood. But in whatever part it is found, it appears to owe its origin to vegetable substances.

Note, Since the above notice was drawn up, I have thought of mixing the acid of honigstein combined with a small quantity of pot-ash, with a solution of pure sulphate of alumine, and a very abundant flocculent precipitate was immediately afforded. On the

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* The acid of sorrel, or oxalic acid, is that which affords by distillation the largest proportion of carbonic acid and water, because it contains more oxigen than any other known vegetable acid.

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other hand, I put into the ſolution of the ſame ſalt the acidulous oxalate of pot-aſh; but no precipitation was ſeen. Now theſe different effects muſt afford doubts ſufficiently ſtrong reſpecting the identity of the acid of honigſtein with that of ſorrel: and I muſt confeſs, that I ſhall ſuſpend my judgment in this matter, notwithſtanding the diſpoſition I had at firſt to believe, that theſe two acids are of the ſame nature. Before we decide, we muſt therefore wait for new proofs derived from a larger ſcale of experiments, in order to throw a more deciſive light on the ſubject.

Cit. Hauy has carefully examined this ſubſtance. Some cryſtals of it were preſented to him by M. Abildgaard, and he procured ſome others from the collection made in Germany by Cit. Launoy. He has diſcovered, that the mellite has a very obvious double refraction, whence there ariſes a diſtinct character between this mineral and amber, of which the refraction is ſimple. He has obſerved likewiſe, that the cryſtals of mellite being inſulated, acquire with much facility the reſinous electricity, but he was not able when they were not inſulated, to excite in ſome of them more than a weak and varying electricity; ſo that it was neceſſary to bring quickly together the cryſtal and ſmall copper needle that is uſed in theſe kind of experiments, in order that the latter may be more ſenſibly attracted. What ſeveral learned men have advanced, that this mellite is not rendered electrical by friction, is not exactly true: it may then acquire an electricity of the ſame nature, as that of amber, but it would be conſiderably weaker if the cryſtal be not inſulated.

According to the obſervation of the ſame naturaliſt, the primitive form of the mellite determined according to the poſition of the natural junctures, is that of a rectangular octahedron, in which the incidence of the face of one pyramid with the face of the other, is about $93\frac{1}{2}°$. This octahedron is ſometimes truncated by means of a decreaſe, by an arrangement on all its ſolid angles, in which caſe the two facets which are placed on its ſummits are often curve lined. When that the diminution only acts on the four lateral angles a dodecahedron is produced, which conſiderably reſembles the rhomboidal figure. Suppoſing this reſemblance perfect, this would be a fourth origin of that dodecahedron, which exiſts as primitive in granite and ſulphurated zinc, and has in other ſubſtances, where it becomes a ſecondary form, ſometimes as nucleus a cube, and at others a regular octahedron.

Bulletin des Sciences, No. 43. An. 9.

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Method of ascertaining the Inclination of the Magnetic Needle. By Cit. Coulomb.

THOUGH the instrument which is usually employed to measure the inclination of the magnetic needle is very simple in its construction, it is nevertheless liable to great errors, which in general arise from the almost absolute impossibility of placing the needle in all the positions it can take in equilibrium with regard to the effect of gravitation, that is to say, so that its centre of gravity may always exactly agree with the point on which it turns. When the dimensions are considerable, a new inconvenience arises from a degree of flexure which, though scarcely sensible, is nevertheless productive of very great effects from the slightest displacement of the centre of gravity producing a combination of the power of gravitation with that of magnetism.

To obviate these difficulties, Cit. Coulomb, instead of endeavouring to ascertain immediately, as has been hitherto done, the direction of the magnetic needle in the vertical plane which passes through the magnetic pole, conceives the force of this pole to be decomposed or resolved into two others in the same plane, the one acting in a horizontal, and the other in a vertical direction. He determines separately the intensity of each of these last forces, and the result gives the direction in which the magnetic force acts, and which a needle governed singly by this force would take.

Cit. Coulomb has proved, in the Memoirs of the Academy of Sciences for the year 1789, that the magnetic needle suspended by its centre of gravity is incessantly brought back to its true direction by a constant force at the same place and time. It thence follows that by observing the number of oscillations made in a given time by a needle horizontally suspended, the ratio of the horizontal component part of the magnetic power with gravity may be obtained. As to the vertical component part, it is measured by determining with care the weight necessary to be added to the southern part of the magnetic needle, to maintain it in a perfectly horizontal position. That being done, if A and B represent the respective measures of the horizontal and vertical component parts of the magnetic power $\frac{B}{A}$ will be the tangent of the angle made by their result with the horizontal force, and, consequently, it will be the inclination of the magnetic needle.

In the experiments made by Cit. Coulomb the needle had the form of a right angled parallelipipedon, very thin in proportion to its breadth, and always suspended, so that its breadth was kept in a vertical plane. Let P represent the weight of the needle, l the half of its length, λ the length of a pendulum that performs its oscillations in the same time as the needle when it obeys the magnetic power in an horizontal plane. Cit. Coulomb then

gives the formulæ $\dfrac{P\,l^2}{3\,\lambda}$ to calculate the momentum of the magnetic force referred to the arm of a lever of one millimeter in length. The length of the needle was 427 millimetres, its breadth 13, and its weight 88753 milligrammes. It was fufpended horizontally by a thread of filk in a box well clofed, and it made 30 ofcillations in 286 feconds, and by applying thefe data to the preceding formula, Cit. Coulomb finds that the logarithm of the momentum of the horizontal magnetic force is 4,1740.

Cit. Coulomb having placed his needle in a clip, having knife edges, which refted on two cylinders of glafs, in the manner of the beam of a balance, endeavoured firft to bring it to an equilibrium in an horizontal fituation coinciding with the magnetic meridian, by placing the edges in a proper manner, and when they were fufficiently near the point where the equilibrium took place, he completed it by the addition of fmall weights. He then reverfed the poles of the needle by the magnetic touch, but without altering the pofition of the clip, and again bringing it to an equilibrium in this new ftate, the fum of the momenta of the additional weights placed in thefe two operations gave him the double of the momentum of the vertical component parts of the magnetic force, valued at $\dfrac{74467}{2}$. The refult of this force, and of the horizontal force, is inclined 68° 9′.

In repeating thefe operations three times, Cit. Coulomb obtained fucceffively 68° 9′; 68° 13′; and 68° 11′. Though the differences of thefe refults are very trifling, he does think they are to be entirely attributed to errors in the obfervation; for he is affured that they do not amount to fo much. It is poffible that the needle is fubject to variations in the vertical fimilar to thofe which are known to take place in the horizontal plane.

Bulletin de la Soc. Phil. No. 31. An. 8.

New Production of Ammonia.

Mr. Lampadius, at Freyberg, has obferved that when crude, or which is better, purified acidulous tartrite of pot-afh, is heated till no more fumes or flame appear, and water is then added to it, ammoniac is produced. This fact is beft obferved when heated mafs is ftill warm (about 120 R.)

The experiment may be performed with the fame tartrite repeatedly, or at leaft as long as any carbonaceous matter remain, by merely heating it, and then wetting it with a few drops of water. Carbon mechanically joined to pot-afh does not produce this effect; but the acidulous oxalate of pot-afh, treated in the fame manner, affords a fimilar refult.

Crell's Annals of Chemiftry, No. 8, 1800.

Upon

Upon trial of the above before fome philofophical gentleman at my houfe, the fumes which arofe upon the addition of the water did not indeed fmell of ammonia, but they formed a white cloud round the ftopper of a bottle which was wetted with ftrong muriatic acid. The indication was not fo manifeft as to remove all doubts in the minds of the affiftants ; but Mr. Accum has fince informed me that he has found it much ftronger and more conftant when paper, wetted with muriatic acid, was ufed.

Difcovery of a new Alkali.

Dr. Hahneman [*], at Altona, in Germany, has difcovered a new fpecies of alkali, which he diftinguifhes from the others by the name of *pneum-alkali*, on account of the fingular property it poffeffes, of increafing in bulk to twenty times its fize when heated. This alkali differs greatly from any of the others, by the following properties.

Its cryftals are hexaedral prifms, terminated by two inclined faces, one of which appears to be trihedral, the other pentahedral:

It is not volatile by heat.

It neither attracts moifture, nor efflorefces by expofure to the air.

Two parts of diftilled water diffolve one of this alkali at † 300° F. Five hundred parts of water diffolve one hundred and forty parts at 60° F. At a lower temperature water acts upon it very flightly : it is, therefore, feparated from its former folution by mere cold : it may thus likewife be feparated when combined with any of the other alkalies.

It effervefces but flightly with concentrated acids; but it is not yet determined if the difengaged aeriform fluid is carbonic acid, or any other gafeous fluid.

The fulphate of pneum-alkali is totally infoluble in ardent fpirit. Water diffolves only a fmall quantity.

The nitric, muriatic, phofphoric, and acetous acid, readily combine with it, and the neutral falts refulting from thefe combinations are totally foluble in water and ardent fpirit. The phofphate and acetite of pneum-alkali are in confiderable quantity even foluble by cold in fpirit of wine.

The phofphate of pneum-alkali has a bitter tafte.

The muriate cryftallifes in plumofe cryftals.

All the preceding combinations of this alkali with acids (the phofphate excepted) are decompofable by mere heat alone. They all part with their acids, and leave the pneum-alkali behind in a pure ftate. The inventor of this alkali employed for this purpofe a red heat. If a lefs heat will do, I do not know.

[*] Scherer's Chem. Journal, III.   † Quere !

The

The nitrate of pneum-alkali liquifies in its own water of cryſtallization at 300° Fr. It firſt bubbles much, nitrogen gas is evolved, and the ſalt is long decompoſed before the maſs is ignited. The nitrate of pneum-alkali does not detonate on ignited coal, it does not decrepitate, nor does it become phoſphoreſcent, nor does it emit ſparks. Its decompoſition by heat is affected gradually and quietly.

The union of pneum-alkali with carbonic acid is extremely difficult. It is beſt effected by decompoſing a ſaline combination of this alkali with an exceſs of acid, by means of any of the other carbonated alkalies: the firſt precipitate which is obtained by this means, is the *pure pneum-alkali* in a cryſtalline form ; but ſoon afterwards the carbonate appears in the ſtate of a light, white, pulverulent, earthy ſubſtance. The carbonate of pneum-alkali readily parts with its carbonic acid at the common temperature of our climate, and becomes converted into pure pneum-alkali.

It ſeparates earths and metallic ſubſtances from their ſaline combinations.

Mercury, when agitated with the aqueous ſolution of this alkali, does not loſe its luſtre.

It precipitates the oxigenated muriate of quickſilver of a carmine red, the nitrate of mercury black, the nitrate of ſilver white, &c. &c.

It decompoſes muriate of ammoniac at 100° F ; but water, impregnated with ammoniac gas, ſeparates the pneum-alkali from the neutral combinations by mere cold.

A ſaturated ſolution of this alkali unites with fat oils by mere agitation, and forms a ſaponaceous compound, ſoluble in ſpirit and in water. Ardent ſpirit has no effect upon it. It changes blue vegetable tinctures green, and poſſeſſes ſome of the properties common to the other alkalies, but again many others, widely different, which entitle it to the notice of the philoſophical chemiſt.

This alkali is ſold at C. G. Hilſcher in Leipzic, in bottles containing one ounce, at one Frederic d'or.

Flour from the Bread Fruit.

Citizen Van Noorden, phyſician at Rotterdam, has written to the (French) Society of Arts, that a ſurgeon, who lately arrived from Surinam, informs him, that the bread fruit tree has ſucceeded ſo well in that colony, that there are conſiderable plantations, which are productive beyond all expectation. They make bread from it, which is as good as that afforded by wheat. For this purpoſe the fruit is ſliced, then dried in the ſun, and afterwards pounded ; and the flour, when made into paſte with water, riſes as well as the flour of wheat, and may be kept for a conſiderable time. The expectation which has

3   juſtly

juftly been entertained of introducing this valuable tree into the weftern colonies, and even into Europe, will give additional intereft to this account of the new application of the bread fruit at Surinam.

Bulletin de la Societé Philom. No. 44, *An.* 9.

Extract from a Memoir of Cit. Thénard, on the feveral Degrees of Oxigenation of the Oxide of Antimony, and on its Combinations with fulphurated Hydrogen.

Cit. Thénard divides his memoir into four paragraphs.

In the firft he relates the principal experiments that have been made fince Geoffroy to the prefent time.

In the fecond he treats of the feveral oxides of antimony, and fhews that this metal is capable of being combined at leaft in fix different proportions with oxygen; that when oxided to the minimum, it is firft black, then chefnut brown, orange, yellow, and white; and at the maximum alfo white;—that diaphoretic antimony is a combination of this laft with pot-afh, and not a pure oxide, as has hitherto been imagined : that the fecond, white oxide which is leaft oxided, comprehends the fublimed oxide of antimony, that which enters with the compofition of the emetic tartar, and alfo of the butter of antimony, which, confequently, ought to be erafed from the lift of oxigenated muriates, where it is placed; that all thefe oxides, when heated in a well clofed crucible, are reduced, and the more eafily the lefs they are oxided; and thus produce the oxides of a yellow, orange, and chefnut brown, and the black oxide, which is alfo obtained with more facility by precipitating the folutions of antimony by iron, and poffeffes the remarkable property of being pyrophoric.

In the third part, the author gives the analyfis of kermes and the golden fulphur, and fhews that the alterations to which thefe bodies are fubject from the action of air and light are owing to the decompofition of thefe fluids : that in the kermes the oxide is in the ftate of the brown oxide, and in the golden fulphur in the ftate of the orange oxide ; that the caufe of different colours of the kermes, which are obtained, arifes from the different coloured oxides which thefe kermes refpectively contain. He afterwards gives analyfes of the fulphuric acid, the fulphate of barytes, and of fulphurated hydrogen, together with the fpecific gravity of this laft, and then proceeds to fhew the action of the alkaline bafes on the fulphuret of antimony, and fhews that the kermes is held in folution by the fulphurated hydro-fulphuret of the bafe, formed by the decompofition of water; that accordingly as this fulphurated hydro-fulphuret has, or has not, the property of being more foluble with heat than without, the depofition by cooling is found to take place on the contrary; and, laftly,

laftly, that the liquor when cold throws down, by means of the acids, the golden tartar, and not kermes, becaufe the brown oxide becomes more oxided by the oxygen of the water and paffes to the ftate of the orange coloured oxide.

In the third paragraph Cit. Thenard makes a recapitulation of his experiments.

Bulletin des Sciences, An. 8. No. 31.

On the Structure of the upper Pyreneans, by CIT. RAYMOND. *Communicated to the National Inftitute of France.*

Pallas in Afia, Sauffure, Deluc, and Dolomieu in Europe, have obferved, that in large chains of mountains, there is ufually in the center a more elevated chain of granite, accompanied on each fide by a collateral fchiftus chain, and ftill lower by another, which is calcareous.

The Pyreneans appear to accord with this law. Their higheft points are certainly calcareous, and this circumftance has embarraffed obfervers.

Cit. Raymond has afcertained, that the refpective difpofition of the five orders of mountains, exifts no lefs in thefe than in the other chains, but with this difference, that the calcareous chain on the Spanifh fide is the moft elevated of the five, and that in returning on the fide towards France the fouthern fchiftus chain is found, then the granite, or middle, and the northern fchiftus and calcareous chains, gradually diminifhing in height, which is the caufe why the geological axis of the Pyrenees, or the granite, is not the fame as the geographical axis, or that which determines the courfe of the waters.

In order to demonftrate the accuracy of his obfervations he has drawn on the chart five lines, correfponding with the five orders of the mountains, each of which is found to pafs through the fummits or maffes of the fubftance, which forms the character of that order which the line indicates.

The granitic axis paffes through the fummits of Néouvielle, Pic long, Bergons, and Monné, the fchiftofe chain, and the northern Gneifs, through the Pic du Midi, and the fouthern through thofe of Troumoufe, Pic mené, Vignemâle, and the Pic du Midi de Pau.

The calcareous ridges on the fide of France are thofe of Campan and of Sarrancolin, fo much celebrated for the marble they produce, and thofe on the fide of Spain form Mont Perdu, Marboré, and le Pic blanc, which are among the moft elevated fummits of thefe mountains.

Abridged in the Bulletin de la Soc. Philom. No. 41, An. 8.

Letter from Mr. DAVY, Superintendant of the Pneumatic Inftitution, containing Notices concerning Galvanifm.

To Mr. NICHOLSON.

S I R,

In purfuing my enquiries concerning the produ&tion of galvanic influence during metallic oxidation, I have found that many of the difficultly oxidable metals may be made to a&t as Voltean combinations, by being conne&ted in pairs in the common order with fluids capable of oxydating one of the alternate metals.

Ten filver plates attached to thin gold wires, and arranged in glaffes containing diluted nitric acid, produced when their agency was applied in the ufual mode, a ftrong cauftic fenfation on the tongue, and effe&ted, though feebly, the ufual changes in water.

Twenty pieces of copper in conta&t with filver wires, when conne&ted with weak folutions of nitrate of mercury a&ted powerfully, and that for a great length of time, *i. e.* till almoft all the mercury was precipitated on the copper. The influence produced fenfible fhocks. When it was paffed through water by means of gold wires, oxygen was given out at the place of the copper, and hydrogen at the place of the filver. Whereas in the combination with filver and gold, the oxygen was produced at the place of the filver, and the hydrogen at that of the gold.

The agency of galvanifm upon inorganic bodies appears to be fimilar under all its different modes of excitement. I have found that the gafes evolved from water by the a&tion of feries in which the oxidating fluid media are acids, or metallic folutions, do not differ in kind or properties from thofe produced by that of combinations in which the fluid media are conftituted by common water.

I have lately made many experiments on the fingle oxidating circles of Afh, and on the influence of thofe circles on galvanic animal irritation. Thefe experiments will at fome time be made public; they go far towards proving not only that the circles of Afh are governed by the fame laws as the pile of Volta; but likewife that there exifts in living matter galvanic a&tion independent of all influence generated by metallic oxidation. I have produced the phænomena of tafte and mufcular irritation by means of metals, in cafes when they were apparently incapable of undergoing chemical change.

I am, SIR,

with refpe&t your's,

Dowry Square, Hotwells,
   Jan. 23, 1801.

HUMPHRY DAVY.

A new

A new edition of Parkinfon's Chemical Pocket Book, with improvements, is on the point of being publifhed.

Mr. William Henry, of Manchefter, has in the prefs, and in confiderable forwardnefs, a fmall work, intended, partly, to facilitate the acquirement of chemical knowledge to perfons entering on the ftudy, without the aid of an inftructor ; and, partly, as a portable companion, for the ufe of more advanced ftudents. The firft part will contain directions refpecting the beft mode of ftudying chemiftry ; and, alfo, an arranged feries of experiments, neceffary to be performed, by thofe who intend to become acquainted, by actual obfervation, with the chemical properties and habitudes of bodies. More minute directions will be given for conducting thefe experiments with fuccefs, than are to be found in other elementary books. The fecond part will comprize fummary inftructions for the analyfis of mineral waters, and of mineral bodies in general : and the third part will point out fome of the ufeful applications of chemical agents, in detecting adulterations, in difcovering poifons, &c. The work will form one fmall pocket volume ; and the author thinks proper to obferve, that it will not at all interfere with the excellent little manual, lately publifhed by Mr. Parkinfon, the plan and objects of which are perfectly different.

⁎ J. W. L. requefts the Correfpondents of this Journal to point out *a method of obtaining aluminous earth perfectly free from any heterogeneous mixture,* and obferves, that the methods pointed out by fyftematical writers are infufficient for that purpofe. As this difficulty will no doubt be new to many chemifts, it would be a defirable communication if he were to ftate the imperfections which his practice may have led him to detect.

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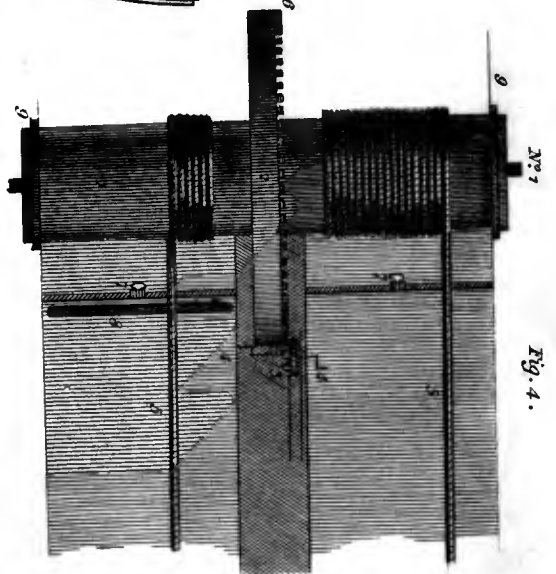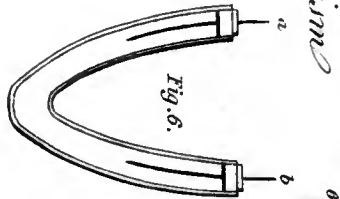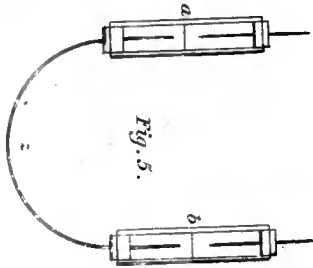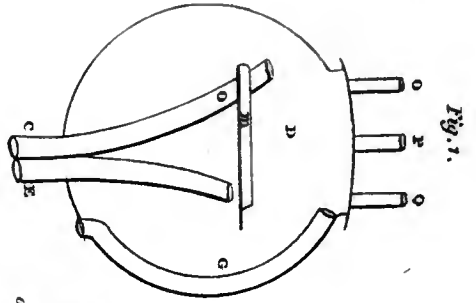
Surface of the Water in the higher Level

Horizontal Line of the upper Level

Line of Inclination of the underground Plane

Surface of the Water at the Lower Level

Fig. 3.

GROUND PLAN

Fig. 2.

Fig. 1.

Galvanism

Fig. 5.

Fig. 6.

Fig. 4.

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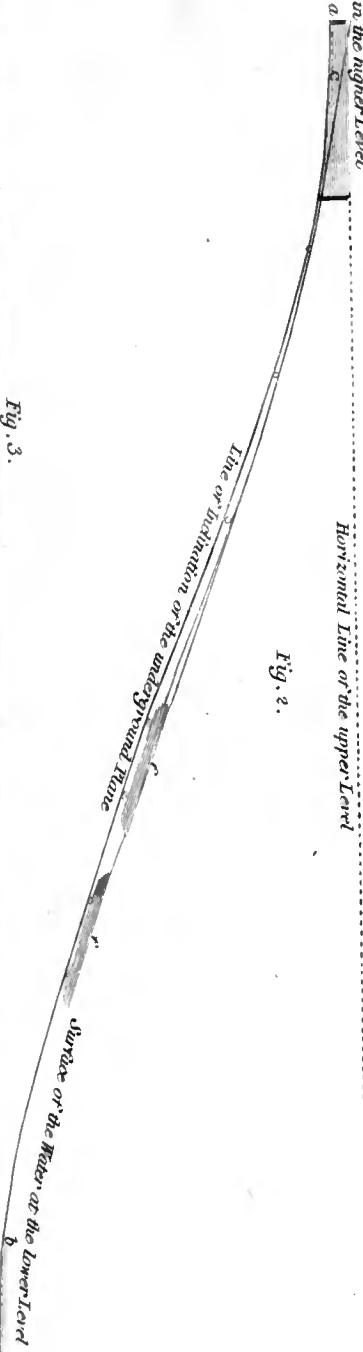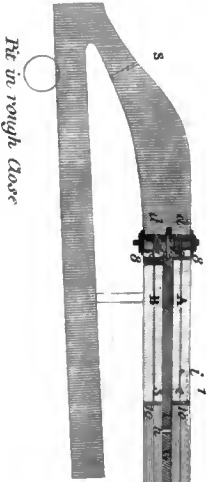
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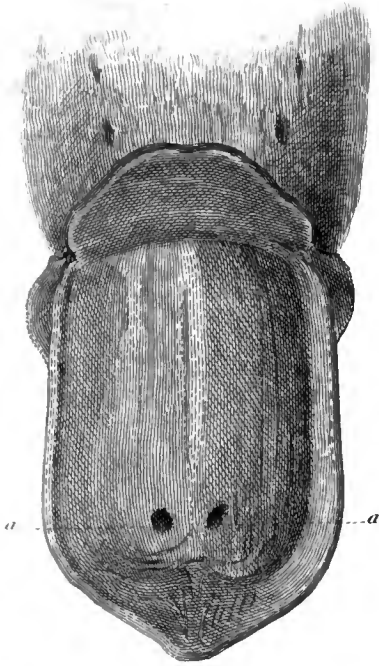
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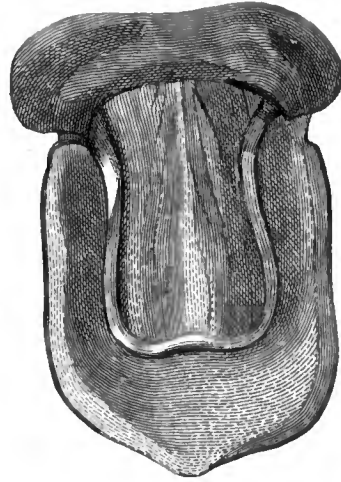
Fig. 4.

Fig. 3.

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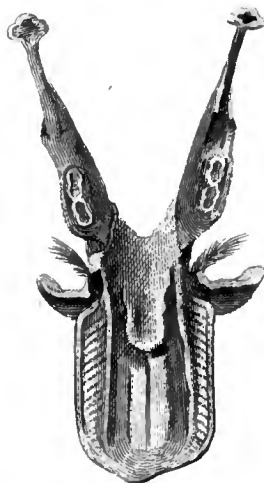
Fig. 7.

Blulow Sculp.

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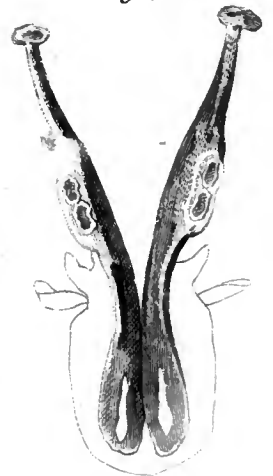
THE ARTS.

ARTICLE I.

Experiments to determine whether or not Fluids be Conductors of Caloric. By Thomas Thomson, *M. D. Lecturer on Chemistry in Edinburgh. Communicated by the Author.*

IT is rather furprifing that the experiments publifhed by Count Rumford, in order to prove that fluids are non-conductors of caloric, fhould have been in the poffeffion of men of fcience for more than two years, without any attempt either to confirm or refute them. Thefe experiments were conducted with fuch admirable fimplicity and ingenuity, by a man of fuch acknowledged talents and candour, and of fuch well-merited celebrity, that they might have been expected to excite peculiar attention. The fubject, too, is of the higheft importance; not only becaufe it is calculated to throw additional light on the nature of caloric, but becaufe it is intimately connected with fome of the moft important operations in nature. It is impoffible for us to inveftigate fpontaneous evaporation, rain, winds, the changes of temperature in the atmofphere, and fome of the moft important phenomena of chemiftry, at leaft with any chance of fuccefs, till we have fettled the previous queftion, whether fluids be conductors of caloric or not. Perhaps it will be faid, that this point has been completely eftablifhed by Count Rumford. This is the very thing which I pro-

pose to examine. But it will be proper, in the first place, in order to prevent verbal disputes, to explain what I mean by *the conducting power* of bodies. Count Rumford has neglected to do this; and I am not absolutely certain that he affixes the same meaning to the phrase that I do.

Dr. Herschel appears to me to have demonstrated, that caloric is emitted from the sun in rays, and that it moves with the same velocity as light, or at the rate of nearly 200,000 miles in a second. It follows, from his experiments, and from those of Scheele and Pictet, that caloric moves through air without any sensible diminution of its velocity. It has been demonstrated, too, that it is capable of moving through glass of a certain temperature, without any sensible diminution of its celerity. Now, whenever caloric passes through a body with undiminished celerity, we may say that it is *transmitted* through the body. *Air*, then, and *glass*, have the property of *transmitting* caloric through them. When caloric is transmitted through a body, as its velocity is too great to be measured, it appears to pass through the body instantaneously. Thus M. Pictet found that caloric moved instantaneously through 69 feet of air.

When the end of an iron rod, 20 inches long, is put into the fire, while a thermometer is applied to the other extremity, it is four minutes before the thermometer begins to rise, and it takes 15 minutes before it rises 10°. Caloric, then, does not pass through a rod of iron instantaneously; consequently, it is not *transmitted* through the iron, but moves through it in a different manner. Its velocity is prodigiously diminished, since instead of moving at the rate of 200,000 miles in a second, it moves only at the rate of 20 inches in 4'. Now, whenever caloric passes in this manner through bodies, with its velocity prodigiously diminished, it is said to to be *conducted* through them. It appears, then, that caloric is *transmitted* through bodies, and *conducted* through them, in quite a different manner.

If we take an iron rod, and another of baked clay, of precisely the same size and shape, and, putting one end of each into the fire, apply thermometers to the other extremity, we shall find that the thermometer applied to the iron rod rises sooner than that applied to the clay rod. Caloric is then conducted with more rapidity through iron than through baked clay. Consequently, iron is a *better conductor* of caloric than baked clay. When a body does not allow caloric to pass through it at all, it is called a *non-conductor*.

It is not difficult to form some notion of the manner in which caloric is conducted through bodies. As its motion is prodigiously retarded, it is clear that it does not move without restraint. It must be detained for some time by the particles of the conducting body, and, consequently, must be attracted by them: hence it follows, that there is an *attraction* or *affinity* between caloric and every conductor of caloric. Now it is in consequence of this affinity that the caloric is conducted through the body.

Let

Let M be a body (a mafs of iron for inftance) compofed of an indefinite number of particles, arranged in the ftrata 1, 2, 3, 4, 5, 6, 7, &c. Let caloric be communicated to it in the direction X. The firft ftratum of particles 1 combines with a certain dofe of this caloric, and forms a compound, which we fhall call A. This compound cannot be decompofed by the fecond ftratum, becaufe all the ftrata, before the application of the heat, were at the fame temperature; confequently, the affinity of all for caloric muft have been equal. Now it would be abfurd to fuppofe a compound deftroyed by an affinity not greater than that which produced it. If, therefore, only one dofe of caloric combined with ftratum 1, no caloric could pafs beyond that ftratum. But the compound A has ftill an affinity for caloric; it therefore combines with another dofe of it, and forms a new compound, which we fhall call B. Now it is a general law, to which I know not a fingle exception, that when a body combines with different dofes of another body fucceffively, the firft dofe is retained by a ftronger affinity than the fecond, the fecond by a ftronger than the third, and fo on. Thus iron, by combining with 25 per cent. of oxygen, forms the green oxide of iron: by combining with 48 per cent. it forms the brown oxide. Here, then, are two dofes of oxygen which combine fucceffively with the iron; the firft is 28 per cent. the fecond 23 per cent. Now the fecond dofe is not retained by an affinity nearly fo great as the firft dofe. For many fubftances are capable of abftracting the fecond dofe, and, confequently, of converting brown oxide of iron into green oxide which have no action on the green oxide: but all the bodies capable of decompofing the green oxide are capable alfo of decompofing the brown oxide.

Let us apply this general law to the compound B, into which ftratum 1 has entered. This ftratum is now combined with two dofes of caloric, the firft of which is retained by a ftronger affinity than the fecond. Stratum 2, therefore, though incapable of decompofing the compound A, has a ftronger affinity for caloric than compound A has for the fecond dofe of caloric; it therefore feizes upon this fecond dofe, combines with it, and forms the compound A. Here, now, are two ftrata of particles, combined each with a dofe of caloric, and, confequently, conftituting the compound A. The third ftratum is unable to decompofe the fecond, for the fame reafon that the fecond was unable to decompofe the firft. Stratum 1 again combines with a dofe of caloric, and forms compound B: ftratum 2 is unable to decompofe this compound, becaufe being already compound A, its affinity for caloric cannot be greater than that of compound A. Caloric, then, cannot pafs farther through the body; but ftratum 1 combines with a new dofe of caloric, and forms a compound, which we fhall call C. The affinity of this third dofe being inferior to that of the fecond, ftratum 2 decompofes compound C of ftratum 1, and forms itfelf compound B. This compound is decompofed by ftratum 3, which now forms compound A. Stratum 1 again forms compound C, which is again decompofed by ftratum 2, which ftratum forms a

new

new compound B. Compound C is a third time formed by stratum 1. Three strata are now heated. Stratum 1 is combined with three doses, stratum 2 with two doses, stratum 3 with one dose. The heat can pass no farther, for stratum 4 cannot decompose compound A, nor compound A compound B, nor compound B compound C; but stratum 1 combines with a fourth dose of caloric, and forms a new compound, which we shall call D. This new compound is decomposed by stratum 2, which forms compound C. Stratum 2 is decomposed by stratum 3, which forms compound B, and stratum 3 by stratum 4, which forms compound A. Stratum 1 again forms compound D, which is again decomposed by stratum 2. This, in its turn, is decomposed by stratum 3, which forms compound B. Stratum 1 again forms compound D, which is again decomposed by stratum 2, in order to form compound C. Compound D is again formed by stratum 1, and is not now decomposed. Here are four strata combined with caloric. Stratum 1 with four doses, stratum 2 with three doses, stratum 3 with two doses, and stratum 4 with one dose. In this manner may the heating progress go on till any number of strata whatever are combined with caloric.

Thus we see that caloric is conducted through bodies merely by repeated chemical combinations and decompositions. Hence the reason of the slowness of its progress. We see, too, that the temperature of the strata must diminish in a kind of arithmetical ratio according to their distance from the source of caloric; and this, in fact, holds in every instance. When one end of an iron rod is held in the fire, the temperature gradually diminishes as we approach the other extremity. We see, too, that the distance to which a body can conduct caloric must in all cases depend upon the degree of heat to which the first stratum of its particles can be subjected, before it changes its state. If it can bear a very great heat, the body will conduct to a very considerable distance; whereas, if if it can bear but little heat, the body will conduct but a very short way. Thus brick will conduct much farther than wood, because when the temperature is raised above a certain degree, the wood catches fire, and is dissipated; whereas, the brick undergoes no change. The distance, then, to which a body conducts caloric, is not a criterion of the goodness of its conducting power, so much as of the degree of heat to which it can be subjected without a change of state, unless the heat be far inferior to what is capable of producing such a change. It appears, too, from this account, that the conducting power of a body is, in all cases, inversely as its affinity for caloric. Consequently, the best conductors are those bodies which have the weakest affinity for caloric.

It can scarcely be doubted that caloric is conducted through all *solid* bodies in the manner above described. All solid bodies hitherto examined are conductors of caloric. Fluids, too, were universally considered as conductors, till Count Rumford drew the contrary conclusion from his very ingenious experiments. If his conclusions be just, this constitutes a very curious and important distinction between solid and fluid bodies. But the more important the conclusion is, the more rigidly ought we to examine the premises before we adopt it.

3. Count

Count Rumford has proved, by a fet of very ingenious and happily-contrived experiments, that when heat is applied to a fluid, the fluid is heated in confequence of every individual particle going in its turn to the fource of heat, combining with caloric, and then giving place to other particles, which combine with caloric in their turn. It is evident, then, that folid and fluid bodies are not heated in the fame manner. The firft fet of bodies can only be heated by their conducting power, in the fecond fet every individual particle goes directly to the fource of heat. Though fluids, then, when placed upon the fire, acquire heat very fpeedily, we are not entitled to conclude, from this circumftance, that they are conductors of caloric. This point can only be determined by direct experiments.

The experiments which Count Rumford * made for this purpofe may be divided into two fets. The firft fet was made in order to fhow that when the internal motions of fluids are impeded, they are not heated fo foon as when their internal motions are unimpeded. This, I think, he has very completely eftablifhed. But as fluids are not heated by their conducting power, it does not follow, from thefe experiments, that fluids are non-conductors. They demonftrate merely that, if fluids be conductors, their conducting power is much inferior to the power by which they are heated. Count Rumford appears to have feen this himfelf, and to have looked upon thefe experiments rather as auxiliaries then as fatisfactory proofs.

The fecond fet of experiments was made in order to prove that caloric cannot pafs *downwards* through fluids. Now, as caloric can pafs downwards through all conducting bodies, and muft indeed, from the very nature of the thing, pafs in every direction through them, it follows that, if it cannot move downwards through fluids, they are, in fact, *nonconductors*. Thefe experiments, then, are perfectly in point. Let us confider them a little.

To fee whether caloric moves downwards, through water, he fixed a cake of ice in the bottom of a glafs jar, covered $\frac{1}{4}$ inch thick with cold water. Over this was poured gently a confiderable quantity of boiling water. Now, if water were a non-conductor, no caloric would pafs through the cold water, and confequently none of the ice would be melted. The melting of the ice, then, was to determine whether water be a conductor or not. In two hours about half of the ice was melted. This, one would think, at firft fight, a decifive proof that water is a conductor. But the Count has fallen upon a very ingenious method of accounting for the melting of the ice, " without being under the neceffity," as he tells us, " of renouncing his theory that fluids are non-conductors."

It is well known that the fpecific gravity of water at 40° is a maximum: if it be either heated above 40°, or cooled down below 40°, its denfity diminifhes. Therefore, whenever a particle of water arrives at the temperature of 40°, it will fink to the bottom of the veffel. Now, as the water next the ice was at 32°, it is evident that whenever any part of the hot water was cooled down to 40°, it would fink, difplace the water at 32°, come into

* Count Rumford's experiments are detailed in our Journal; for which fee the Indexes.---N.

contact

contact with the ice, and, of courfe, melt it. The Count's ingenuity, never without refources, enabled him to prove completely that the ice employed in his experiment was actually melted in that manner. For when he covered the ice partially with flips of wood, that part which was fhaded by the wood was not melted; and when he covered the whole of the ice with a thin plate of tin, having a circular hole in the middle, only the part exactly under the hole was melted. From thefe facts it certainly may be concluded that the ice was melted by defcending currents of water.

But the point to be proved is not whether there were defcending currents, but whether water be a conductor or not. Now, if water be a non-conductor, I afk how the hot water was cooled down to 40°? Not at the furface; for the Count himfelf tells us, that there the temperature was never under 108°; not by the fides of the veffel; for the defcending current in one experiment was exactly in the axis; and it follows irrefiftibly from the experiment with the flips of wood, that thefe defcending currents fell equally upon every part of the furface of the ice; which would have been impoffible, if thefe currents had been cooled by the fide of the veffel. The hot water, then, muft have been cooled down to 40° by the cold water below it; confequently, it muft have imparted caloric to this cold water. If fo, one particle of water is capable of abforbing caloric from another; that is, water is a *conductor of caloric*. After the hot water had ftood an hour over the ice, its temperature was as follows:

At the furface of the ice　-　-　-　-	40°
1 inch above the ice　-　-　-　-　-	80
2 inches　-　-　-　-　-　-　-	118
3 inches　-　-　-　-　-　-　-	128
4 inches　-　-　-　-　-　-　-	130
7 inches　-　-　-　-　-　-　-	131

How is it poffible to account for this gradual diminution of heat as we approach the ice, if water be a non-conductor? The water, it may be faid, gives out caloric at its furface, falls down and arranges itfelf according to its fpecific gravity. If fo, how comes it, that there is only one degree of difference between the temperature at 4 and at 7 inches above the ice? Thus it appears that the Count's experiment, inftead of demonftrating that water is a non-conductor, rather favours the fuppofition that it is a conductor.

The Count drew, as a corollary, from this experiment, that water at 41° will melt as much ice in a given time as boiling water, when both ftand over the ice. He found this actually to hold; or rather he concluded from his experiments, affifted, however, by a little calculation, that water at 41° will melt more ice in a given time than boiling water. It would not be difficult, perhaps, to find fome flaws in the calculation; but granting the truth of the fact, I do not perceive how it contributes in the fmalleft degree to prove that water is a non-conductor. For water at 41° (being denfer than water at 32°) melts the ice, not by its conducting power; but by actually travelling to it particle after particle, and giving out its caloric: whereas, boiling water can only act by its conducting power,

3

till

till fome part of it be cooled down to 40°. And as water is confeſſedly a very bad con-
ductor, it is a very confiderable time before much of it is cooled down fo low. Ac-
cordingly we find that after 20′ the boiling water at the diſtance of only one inch above the
ice was 130°, and of half an inch 46°. Water at 41°, then, has an advantage over boiling
water, and this advantage is the greater the worfe a conductor of caloric the water is.

I fhall not be fo particular in my examination of Count Rumford's experiments made in
order to prove that *mercury* and *oil* are non-conductors of caloric; becaufe they are not
fufceptible of fufficient accuracy to decide the point. The iron cylinder would retain its
heat too fhort a time to melt much ice, even if thefe fluids had been excellent conductors:
and I really do not fee how it was poffible to detect a very fmall quantity of water, even if
fome of the ice had been melted. But if the Count had made ufe of a thermometer inſtead
of ice, it would have rifen feveral degrees, as I have found by making the experiment,
even though the hot iron had been at a greater diftance from the bulb, than his was from
the nipple of ice.

It appears, then, that Count Rumford's experiments are not decifive, or if any conclufion
can be drawn from them, it is rather favourable to the fuppofition that fluids are *conductors
of caloric.* It occured to me, that by varying Count Rumford's experiments a little, the
point in queſtion might be decided with certainty. I took a tubulated glaſs receiver M,
Fig. 2, Pl. XXIII. into which there paſſed the thermometer B. This thermometer was fo
placed, that its bulb was exactly in the axis of the veffel. The mouth at which it entered
was clofed with cement. The degrees were marked upon the tube itſelf, and were all on
that part of it which was without the receiver. Into this veffel I poured the fluid, whofe
conducting power I wifhed to examine, till it ſtood at a certain height above the thermo-
meter B, fuppofe at D. I then poured cautiously over it (fuppofe to the height E) another
fluid of inferior fpecific gravity, and heated to a certain temperature. Now, if the ther-
mometer B did not rife, it would follow that the fluid was a non-conductor; if it did rife,
that it was a conductor. A variety of precautions were neceffary to prevent being mifled;
but thefe will appear from the experiments themfelves, which I fhall now detail.

The firſt fluid made choice of was mercury. I filled the veffel M with mercury till it
ſtood 0.2 inch above the bulb of the thermometer B. Over this was poured cold water,
till it ſtood ½ inch above the furface of the mercury. I then cautiously poured in boiling
water till it ſtood about two inches above the furface of the mercury. The thermometer A
was fufpended in the water, fo that its bulb approached very near the furface of the mer-
cury. There was alfo a third thermometer C, whofe bulb, which was oblong, refted
againſt the bottom of the veffel. The temperature of the mercury and of the external air
was 42°. That of the hot water, as foon as it could be obferved after pouring it in (which
took up fome time), was 174°. The refult of the experiment was as follows:

EXPERIMENT

EXPERIMENT I.

Thermometer B rose to 80° during the pouring in of the water.

Time.	Therm. A.	Therm. B.	Therm. C.	Water 1 inch below the surface.
In 2′	126°	85°	67°	174°
5	111	90	83	163
10	106	93½	88	152
12		95		
15	104	96	91	144
21	102	96	91¾	133
25	100	96	91¾	128
28	98	95	91¾	122
35	97	93	89½	116
40	95	91	88	100
45	93	90	87	106
50	91	88	85	102
55	88	86	84	99
60	86	85	82	96
65	84	83½	81	92
70	83	82	79	90
75	80	80	78	88
80	79	78	76	85
85	77½	77	75	83
90	76	76	73	80
95	74	74	72	76

Here the thermometer B rose in 15 minutes 16°; and in 21′ the thermometer C rose 24.75°: all this time the thermometer A was gradually falling, and approaching nearer and nearer the temperature of B, and after 95′ the two were precisely of the same temperature. The maximum of B was 96°, of C 91.75°. B first acquired its maximum, then C: B began to fall in 25′, C in 28′. Now all these circumstances are precisely what ought to happen if mercury be a conductor of caloric. The experiment, therefore, seems to prove that mercury actually conducts caloric: for we see that caloric passes downwards through mercury, which could not happen if mercury were a non-conductor.

Though this experiment seemed decisive, I wished to repeat it in a different manner, in order, if possible, to determine the internal motions of the hot liquid, that I might ascertain whether or not these motions had any influence on the rise of the thermometers. Count Rumford's ingenious method readily presented itself. Accordingly I poured over the mercury a saturated solution of carbonate of pot-ash in water, till it stood ½ inch above its surface. Then boiling carbonate of pot-ash was cautiously poured over all, till it stood

two

two inches above the furface of the mercury. As this folution had not been filtered, there were floating through it a number of opaque particles, nearly of the fame fpecific gravity with the folution. By the motion of thefe I expected to be able to judge of the currents in the hot liquid. The temperature of the mercury and of the external air was $41\frac{1}{2}°$. That of the carbonate, when firft tried after being poured in, 174°. The refult of the experiment was as follows:

EXPERIMENT II.

Thermometer B rofe during the pouring in of the carbonate to 75°.

Time.	Therm. A.	Therm. B.	Therm. C.
In 1′	118°	78	55°
2	109	80	67
6	103	85	78
10	100	88	84
15	98	90	85
20	96	90	87
25	94	90	86
30	92	89	86
35	90	87	85
40	87	85	83
45	85	84	82
50	84	83	80
55	82	80	$78\frac{1}{4}$
60	80	$79\frac{1}{2}$	77
65	78	77	$75\frac{1}{2}$
70	$76\frac{1}{2}$	76	74
75	75	$74\frac{1}{2}$	$73\frac{1}{2}$
80	73	73	$70\frac{1}{2}$
85	72	72	70
90	70	70	$68\frac{1}{3}$
95	69	69	67
100	68	$67\frac{1}{2}$	66
105	$66\frac{1}{2}$	66	65
110	65	65	64
115	$64\frac{1}{2}$	64	63
120	63	63	62

Here the refult is almoft the fame as in experiment firft. It is therefore a confirmation of it, and makes the conclufion drawn from it, that mercury conducts caloric, more to be depended on.

Soon

Soon after pouring in the hot carbonate, I obferved the opaque particles formerly mentioned, moving pretty rapidly upwards and downwards, indicating Count Rumford's currents. Thefe motions did not depend upon the fides of the veffel, for particles were afcending and defcending in every part of the liquid. They afcended with great rapidity till they came within a certain diftance of the furface; then they defcended again as rapidly as they had afcended, till they came within a certain diftance of the mercury; here they became ftationary for a moment, and then afcended again with as much velocity as at firft. Sometimes a few particles afcended to the very furface, and then defcended again with very great rapidity. But they conftantly ftopped, and began to move upwards when they came to a certain diftance from the mercury; and they all ftopped at the fame diftance from the mercury. At firft this diftance was about half an inch, but it became gradually greater as the procefs of cooling went on, and at laft the particles did not defcend lower than the middle of the liquid. If any particle happened to go beyond this boundary, and to approach a little nearer the mercury, it foon became ftationary, and did not rife any more. The rapidity of the motions of thefe particles gradually diminifhed, as well as the fpace through which they moved: after 40′ I could not obferve them moving at all; though, when I marked the fituation of a particle, and then obferved it fome time after, it became evident that it had changed its place.

Thefe motions are curious, and give us a good deal of information about the procefs of cooling in the hot liquid. I pretend not to explain their rapidity; but, waving that difficulty, it feems pretty clear that the particles near the furface giving out their heat, became fpecifically heavier, and tumbled down towards the mercury. They did not reach the mercury, however, becaufe at fome diftance from it they came to a part of the liquid colder than themfelves. If thefe defcending particles happened, in confequence of a momentum accidentally greater than ufual, to penetrate into this cold ftratum, they loft their excefs of caloric before they could rife again, and, of courfe, continued ever after ftationary in the cold ftratum. Thefe defcending currents neceffarily produced afcending currents; but thefe afcending particles could fcarcely reach the furface, unlefs their momentum happened to be unufually great, becaufe the water being hotteft juft below the furface, they foon came to particles fpecifically lighter than themfelves. As the cooling advanced, the cold ftratum, of courfe, increafed, and the currents moved more flowly, becaufe the water at the furface, approaching to the temperature of the air, was cooled more flowly. We fee, then, that the defcending currents do not reach the mercury; confequently, they cannot affect its heating except indirectly; far lefs can they affect the temperature of the thermometer.

Wifhing, now, to try a hot liquid of a different kind, I poured lintfeed oil, at 230°, over the mercury, till it ftood an inch and a half above its furface. The temperature of the mercury, and of the external air, was 41½°; that of the oil, as foon as it could be afcertained, after pouring it in, was 180°. The refult was follows:

EXPERIMENT

EXPERIMENT III.

Thermometer B rose during the pouring to 50°.

Time.	Therm. A.	Therm. B.	Therm. C.	
2′		56°	50°	
5	91°	60	56	
10	82	63	60½	
15	77	6+	63	
20	74	64½	62¾	
25	70	64½	62¾	
30	67¼	63½	62	
35	67½	63	61½	
40	64	62	60¾	
45	62½	61	59¾	
50	61	60	58¾	
55	59½	58½	57¾	
60	58½	58	57	
65	57	57	56	Oil
70	56	56	55	58

Here the general result is precisely the same as in the former experiments. The only remarkable difference is the small rise of the thermometer during the pouring in of the hot oil. During that operation it rose only 8½°, whereas in the two former experiments its rise was nearly 30°.

These three experiments were varied different ways, but the results were so nearly the same, that it would be both useless and tiresome to relate the whole. They seem to me to put it beyond a doubt that mercury is a *conductor of caloric.*

I wished now to try whether water also be a conductor. For this purpose I poured water at 40° (the temperature of the air) into the vessel M, till it stood 0.8 inch above the bulb of the thermometer B. Over this I poured a thin stratum of lintseed oil. Then lintseed oil, heated to 220°, was poured cautiously over all, till it stood 1.15 inches above the surface of the water. The temperature of the oil, as soon as it could be ascertained after it had been poured in, was 160°. The result of the experiment was as follows:

EXPERIMENT

EXPERIMENT IV.

Thermometer B rose during the pouring 0°.

Time.	Therm. A.	Therm. B.	Therm. C.
In 5′		41°	
10	79°	42	
15	70	43	42½
20	68	44	43
25	65	45	43
30	63	46	43
35	61	46½	43
40	59	46½	43
45	57	46½	43½

We fee from this experiment that water is a conductor of caloric; but a much worfe conductor than mercury. The thermometer B did not rife during the pouring in of the oil, partly becaufe it was at a confiderable diftance from the furface, and partly becaufe water conducts worfe than mercury. But it did rife afterwards 6½°. It was 5′ before it began to rife, and 35′ before it reached its maximum. The refult of this experiment was fo curious, that I wifhed to repeat it with fome variation. I put all the three thermometers in the water: thermometer A 0.2 inch below the furface, thermometer B 0.7 inch below the furface, and thermometer C 1.725 inches below the furface. The cold water was 2.2 inches deep; its temperature was 42° (that of the air). A ftratum of cold lintfeed oil was poured over the water, then lintfeed oil, at 220°, was poured over all, till it ftood 2.125 inches above the furface of the water. The refult was as follows:

EXPERIMENT V.

Time.	Therm. A.	Therm. B.	Therm. C.
In 2′	58°	45°	
5	59	46	42
10	60	49	42.75
15	60	50	43
20	60	51	43.5
25	59	51½	44
30	59	52	44.5
35	58	52	45
40	57	52	45.5
45	56½	52	46
50	56	52	46.5
55	55½	51½	46.75

Time.

Time.	Therm. A.	Therm. B.	Therm. C.
In 60′	55°	$51\frac{1}{2}$°	47°
65	54	$51\frac{1}{4}$	47.25
70	$53\frac{1}{2}$	51	47.5
75	53	51	48
80	53	51	48
85	$52\frac{1}{2}$	$50\frac{1}{2}$	48
90	$52\frac{1}{4}$	$50\frac{1}{4}$	48.25
95	52	$50\frac{1}{2}$	48.25
100	$51\frac{1}{2}$	50	48.25
105	51	50	48.5
110	51	$49\frac{1}{2}$	48.5
115	51	$49\frac{1}{4}$	48.5
120	$50\frac{1}{2}$	49	48.25
125	50	$48\frac{3}{4}$	48.25
130	50	$48\frac{3}{4}$	48.25
223	47	46	46

Here the thermometer A rose 18°   and reached its maximum in   11′

B   10   31

C   6.5   106.

This experiment seems to me to prove, in a very clear manner, that water is a conductor. But before we draw any such conclusion from it, or from those before detailed, it will be proper to enquire whether the rising of the thermometers can be accounted for in any other way. If it cannot, then water and mercury must be considered as conductors. Now, it may be said, that the thermometers rose in consequence of the action of *transmitted* caloric; or that caloric was *conducted* to them by means of the glass vessel in which the experiment was made; or that *currents* of the hot liquid made their way to them; or, lastly, that the cold liquid was thrown into such *agitation* by the pouring in of the hot liquid over it, that currents of it heated directly by the contact of the hot liquid made their way to the thermometers. If none of these suppositions will account for the rise of the thermometers, we must conclude from the experiments that mercury and water are conductors of caloric.

1. If the thermometers had risen in consequence of caloric transmitted to them through the cold liquids, they ought to have begun to rise at the same instant; or, rather, the lowest, which was the most delicate, ought to have begun to rise first. But the contrary is evident from all the experiments. In the last experiment, for instance, the thermometer B did not begin to rise till some time after the oil had been poured in; and thermometer C not till more than 6′ after I had begun to observe the thermometers, and, consequently, not till at least 12′ after beginning to pour in the oil. Thermometer B reached its maximum in

3^{1},

31′, C in 106 ; a length of time absolutely incompatible with every idea respecting transmitted caloric. It is not possible, then, that the thermometers could have risen in consequence of the action of transmitted caloric.

2. If the caloric had been conducted to the thermometers by the vessel, C, which was always nearest the vessel, ought to have risen first, or at least, as its situation was precisely the same, it ought to have risen the same number of degrees in experiments 3d and 4th. In these the hot oil was the same; it ought therefore to have communicated the same quantity of caloric to the vessel: but in experiment 3d, C rose both sooner and higher than in experiment 4th. This is easily explained, on the supposition that water is a worse conductor than mercury; but it is inexplicable, on the supposition that the heat was communicated by the vessel. The following experiment proves clearly that this supposition is without foundation.

To determine whether the cold liquid was thrown into waves, or whether currents were formed in it either by pouring the hot liquid over it, or during the process of heating, I took a solution of carbonate of pot-ash, with amber floating through it, prepared in the way described by Count Rumford, and poured it into the vessel M till it stood 0.3 inch above the thermometer B. Over this was poured cold water to the height of half an inch. The two surfaces kept perfectly distinct, as was evident from the amber, some of which was floating at the surface of the carbonate. Over all I poured boiling water, tinged blue with the juice of red cabbage. The blue colour hung down in clouds through rather more than the half of the pure water; but none of it approached the carbonate. The temperature of the carbonate was 40°. (that of the air). Thermometer B rose during the pouring to 73°. Thermometers A and C placed as in experiment 1st. The result of this experiment was as follows:

EXPERIMENT VI.

Time.	Therm. A.	Therm. B.	Therm. C.
In 4′	105°	75°	43°
10	102	78	46
15	98	79	$47\frac{1}{2}$
20	96	80	50
25	93	80	53
30	90	79	$53\frac{1}{2}$
35	88	78	55
40	86	77	56
45	83	$76\frac{1}{2}$	57
50	81	76	58
55	$79\frac{1}{2}$	75	$58\frac{1}{2}$
60	78	74	$58\frac{3}{4}$

Time.

Time.	Therm. A.	Therm. B.	Therm. C.
In 65′	76°	73°	59°
70	74	72	59
75	72	70	59
80	71½	69	59
127	62	62	56
130	61¾	61¾	56
135	61	60½	56

During the whole of this experiment the amber continued perfectly motionless; consequently there were no currents. But there must have been currents, if the heat had been communicated by the sides of the vessel; for in that case the particles of the liquid next the sides must have received heat: this would have caused them to ascend, and there would have been descending currents in the interior parts of the liquid. It is not possible, then, to suppose that the thermometers rose in consequence of caloric conducted to them by the sides of the vessel.

3. If the thermometers had risen in consequence of currents of the hot liquid striking against them, these currents would have been visible in several of the experiments. If the smallest particle of oil, for instance, had made its way through the water, it would have been detected by its whiteness. We are absolutely certain that no such currents existed in the 6th experiment; for had the smallest atom of the water, tinged with red cabbage, made its way through the carbonate, it would have been instantly detected by a change of colour from blue to green. But no such change of colour took place: nay, more than six hours after the experiment, no green colour could be perceived; but it appeared instantly after a very slight agitation. Besides, how could oil or water makes its way downwards through mercury? This supposition, then, must also be given up.

4. If the cold liquid had been so much agitated by pouring the hot liquid over it, that currents of it, heated directly by the hot liquid, made their way to the thermometers, these currents must have been visible in the 6th experiment, which was made on purpose to detect them. Now, as the amber remained perfectly motionless, even during the pouring in of the hot liquid, and never moved after, it follows that no such currents existed. Consequently, the fourth supposition cannot be admitted.

Upon the whole, then, I consider these experiments as proving, beyond a doubt, that water and mercury are conductors of caloric. I have repeated them frequently, in order to guard as much as possible against mistakes: but I trust that others, who are interested in this subject, will take the trouble to repeat them. I may, perhaps, have overlooked something, notwithstanding all my caution. If I have, the experiments are so easily made, that it cannot fail to be very soon discovered. Whatever may be the issue of this investigation, the greatest part of the merit belongs exclusively to Count Rumford. My experiments were made merely in consequence of his, and I availed myself of the contrivances which he himself had suggested.

To see whether sulphuric acid be a conductor of caloric, I made the following experiment:

EXPERIMENT

3

EXPERIMENT VII.

Into the veffel M was poured concentrated fulphuric acid, till it ftood 0.6 inch above the thermometer B. The temperature was 43° (that of the air). Over this was poured cautioufly a faturated aqueous folution of fulphate of pot-afh with excefs of acid, at 43°, till it ftood 0.4 inch above the acid. The two liquids remained perfectly diftinct, as was evident from the difference of their colours and confiftence; for the acid was a little coloured. The furface where they joined reflected light very ftrongly. The thermometer A was fufpended, fo that its whole bulb was in the fulphate; thermometer C was 1.366 inches under the furface of the acid; the acid began flowly to combine with the water above it, and this combination gradually evolved heat. I expected that this heat would make its way downwards, and raife the thermometers. The refult was as follows:

Time.	Therm. A.	Therm. B.	Therm. C.
In 3′		49°	
5		50	
10		54	
15		56	47°
20	82°	58	$47\frac{1}{4}$
25	82	60	49
30	82	$60\frac{1}{4}$	50
35	82	61	51
40	81	$61\frac{1}{2}$	$51\frac{3}{4}$
45	80	62	$52\frac{1}{4}$
50	$79\frac{1}{2}$	$62\frac{1}{2}$	$52\frac{3}{4}$
55	79	63	$53\frac{1}{2}$
60	$78\frac{1}{2}$	63	54
65	$77\frac{1}{2}$	63	$54\frac{1}{2}$
70	77	$63\frac{1}{2}$	55
75	$76\frac{1}{2}$	$63\frac{1}{2}$	$55\frac{1}{4}$
80	76	63	$55\frac{3}{4}$
85	75	63	56
90	74	63	$56\frac{1}{4}$
95	$73\frac{1}{2}$	$62\frac{1}{2}$	$56\frac{1}{4}$
100	73	$62\frac{1}{2}$	$56\frac{1}{4}$
105	$72\frac{1}{2}$	$62\frac{1}{2}$	56°
110	72	62	$56\frac{1}{4}$
115	$71\frac{1}{2}$	62	$56\frac{1}{4}$
120	71	$61\frac{3}{4}$	$56\frac{1}{4}$
125	70	$61\frac{3}{4}$	56
130	70	$61\frac{1}{2}$	56
230	62	58	53

Two days after this experiment was made, the surfaces of the acid and sulphate were still as distinct as ever; but the stratum of sulphate was nearly double its original thickness, owing evidently to its having combined with a considerable portion of the acid. This experiment, I think, proves that sulphuric acid is also a conductor of caloric. Indeed, it is more than probable that all fluids are conductors.

II.

Description of a Steam Engine on the Principle of Savary, operating by a separate Condenser; with other essential Improvements. By Mr. John Nancarrow [*].

PLATE XXIII. Fig. 3. A. The receiver, which may be made either of wood or iron.

BBBB. Wooden or cast-iron pipes for conveying the water to the receiver, and from thence to the penstock.

C. The penstock or cistern.

D. The water-wheel.

E. The boiler, which may be either iron or copper.

F. The hot-well for supplying the boiler with water.

GG. Two cisterns under the level of the water, in which the small bores BB, and the condenser are contained.

HHH. The surface of the water with which the steam-engine and water-wheel are supplied.

aa. The steam-pipe, through which the steam is conveyed from the boiler to the receiver.

b. The feeding-pipe, for supplying the boiler with hot water.

ccccc. The condensing apparatus.

dd. The pipe which conveys the hot water from the condenser to the hot-well.

eee. Valves for admitting and excluding the water.

ff. The injection pipe, and *g* the injection cock.

h. The condenser.

It does not appear necessary to say any thing here on the manner in which this machine performs its operations without manual assistance, as the method of opening the cocks by which the steam is admitted into the receiver and condensed, has been already well described by several writers. But it will be necessary to remark that the receiver, penstock, and all the pipes, must be previously filled [†] before any water can be delivered on the

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[*] From a learned paper on Mills, in the American Transactions, IV. page 355. I have not been able to extract the theoretical parts of the paper, because the plates and references are too imperfect.

[†] Or the air blown out by steam; which may perhaps be less convenient than the method in the text.—N.

wheel, and when the steam in the boiler has acquired a sufficient strength, the valve at *c* is opened, and the steam immediately rushes from the boiler at E into the receiver A, the water descends through the tubes A and B, and ascends through the valve *e* and the other pipe or tube B into the penstock C. This part of the operation being performed and the valve *c* shut, that at *a* is suddenly opened, through which the steam rushes down the condensing pipe *c*, and in its passage meets with a jet of cold water from the injection cock *g*, by which it is condensed. A vacuum being made by this means in the receiver, the water is driven up to fill it a second time through the valves *ee*, by the pressure of the external air, when the steam-valve at *c* is again opened, and the operation repeated for any length of time the machine is required to work.

There are many advantages which a steam-engine on this construction possesses, beyond any thing of the kind hitherto invented; a few of which I shall beg leave to enumerate.

1. It is subject to little or no friction.

2. It may be erected at a small expence when compared with any other sort of steam-engine.

3. It has every advantage which may be attributed to Boulton and Watt's engines, by condensing out of the receiver, either in the penstock or at the level of the water.

4. Another very great advantage is, that the water in the upper part of the pipe [*] adjoining the receiver, acquires a heat by its being in frequent contact with the steam, very nearly equal to that of boiling water; hence the receiver is always kept uniformly hot as in the case of Boulton and Watt's engines.

5. A very small stream of water is sufficient to supply this engine, (even where there is no fall) for all the water raised by it is returned into the reservoir HHH.

From the foregoing reasons it manifestly appears that no kind of steam-engine is so well adapted to give rotatory motion to machinery of every kind as this. Its form is simple, and the materials of which it is composed are cheap; the power is more than equal to any other machine of the kind, because there is no deduction to be made for friction, except on account of turning the cocks, which is but trifling.

Its great utility is therefore evident in supplying water for every kind of work performed by a water-wheel, such as grist-mills, saw-mills, blast-furnaces, forges, &c.

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[*] Not being thrown out by a side aperture, as in Plate XVII. Vol. I. of our Journal, but merely raised and depressed in the pipe AB.---N.

III.

New Application of the Syphon to raise Water above the Surface of the Reservoir. In a Letter from Mr. WILLIAM CLOSE.

To Mr. NICHOLSON.

SIR,

THE syphon will raise a stream of water through an extensive space in every situation where a little descent can be procured, but while the operation continues, no water can be taken directly out of the stream above the lowest part of the tube. When, however, the two open ends of a syphon are closed, a quantity of water may be let out of the highest part, and its place supplied by introducing a like quantity of no use: all the avenues for the purpose being then closed, and the stream suffered to flow through the tube; the useless water will be displaced, and a fresh quantity may be soon after drawn off. This mode of exchange may be useful in furnishing a supply for washing, and some other purposes, but there are several domestic uses for which the water drawn off will not be thought sufficiently pure. A method of taking water out of the syphon at any height within the limits of the elevation, without retarding the stream, or introducing another quantity, has often appeared to me very desirable, and some months ago, I made a number of experiments to determine the practicability of the project.

My hopes of success were founded upon the following observations:

When water flows through a syphon that has an elbow or dilatation containing air, the stream moves much slower, and is smaller on entering into the external air, than when the part is completely filled with water. When the current moves with a certain velocity, its lateral action extracts the air, and carries it down the descending column. As the part fills with water, the stream becomes fuller, and its velocity encreases.

Among a variety of projects for extracting the air out of a vessel fixed to the side of a syphon, and filling it with water, by the assistance of the lateral action of the stream, the following seemed the most likely:

If the connection between the vessel and syphon be made by two tubes, the end of one turned against the current, and the end of the other turned from it, the stream after striking the end of the first tube, will be contracted until it has passed by the end of the second. After this it will begin to expand; the air will be drawn out of the vessel through the tube turned from the current, and water will enter by the other, until the vessel is filled; the stream beyond the contraction occasioned by the two tubes will then receive a supply through the vessel.

The result of an experiment founded upon this theory was very satisfactory. In place, however, of a particular account of the first experiment, I shall give a detail of some others, in which a still more easy expedient was adopted, to produce the same effect:

Two

Two fmall demi-cylindrical tubes when conftructed were joined together in fuch a manner, that one piece ferved for the flat fides of both. They were foldered into the fide of a fyphon at the middle of the turn, or that part which the water paffes through at its higheft elevation. The flat partition between them was thin, and projected a little into the infide of the fyphon, but their femi-circular fides had no projection at all. When the branches of the fyphon were held horizontally, the pofition of the two fmall tubes was vertical, and the plane of the partition was parallel to a line drawn between the branches. The fmall cylinder formed by both tubes was then encircled with thread to a proper thicknefs, and inferted into the neck of an ounce glafs vial.

Every part of this apparatus being perfectly air tight, the fyphon was filled with water, by holding it in nearly an horizontal pofition below the furface of water in a capacious veffel : both of its apertures were then clofed by the thumbs of the operator, and the branches were fet in a vertical pofition : the end of one branch being immerfed in water, its aperture was opened, and the efflux through the tube was permitted immediately after, by opening the aperture of the other branch.

When the ftream had a defcent in the tube equal to one inch below the furface of the water in the veffel, bubbles of air were ejected, and in a fhort time the bottle was filled with water to a level with the upper fides of the two fmall tubes; a continued circulation was then vifible. The water entered the bottle through the tube on that fide of the partition which was ftruck by the ftream, and paffed out through the tube on the other fide, with a velocity proportionate to the active weight of the defcending column, which was encreafed or diminifhed at pleafure, by lowering or raifing the fyphon. When the effective fpace of defcent was lefs than one inch, no air was extracted out of the bottle.

In this experiment the length of each branch of the fyphon was feven inches : the diameter of the tube was all throughout equal to about half an inch.

The length of one branch of the fyphon being encreafed to forty-five inches, and that of the other to forty-three, the ounce bottle was changed for one of eight ounces, and the fyphon was filled with water in an inverted vertical pofition. After its two apertures were refpectively clofed with corks, it was laid horizontally, and the bottle was detached from it, to let out a little water which had been forced into it, and alfo with an intent to leffen the compreffion of the air it contained. After replacing the bottle, the fyphon was fet into its proper vertical pofition, with its fhorter branch plunged in water, and the efflux was permitted by drawing the corks. When the fhorter column was equal to forty-one inches and an half, it was requifite to fettle the fyphon a few inches immediately at the commencement of its operation, to prevent the longer column from wafting fo much as to let the fhorter preponderate. When the ftream begun to extract the air, the fyphon was raifed to its former elevation, or even higher, and water continued to enter the bottle, and air to be carried down the defcending column, until the fhorter branch could no longer receive a fupply of water. The active length of the defcending column was then no longer than two inches and an half.

It.

It was not neceſſary to ſettle the ſyphon when only an ounce bottle was uſed.

When the ſyphon was in its original ſhort ſtate, the partition between the two ſmall pipes was taken ſmoothly off with the inſide of the tube. No water would enter the bottle, although the active part of the deſcending column was of conſiderable length.

From theſe facts and obſervations I deduce the arrangement repreſented in Fig. 1, Plate XXIV. for extracting a quantity of water out of a ſyphon at any elevation, and ſupplying its place with air.

Into any part, except the top ſide, of a vertical ſyphon S Y, inſert two ſmall pipes, and let their apertures in the inſide of the tube be divided by a projecting piece about a quarter of an inch thick; wherever the pipes are inſerted, the piece muſt be placed in ſuch a poſition, that the current will ſtrike againſt one of its flat ſides. The pipe which opens on that ſide of the obſtacle or dam ſtruck by the ſtream, may be called the water pipe, and that on the other ſide the air pipe. Inſert their other ends into a veſſel A W. The air pipe oppoſite to A muſt riſe to near the top of this veſſel, but the water pipe W need not riſe above the place of its inſertion. A cock perfectly air tight muſt be fixed in each pipe between the veſſel and ſyphon: the veſſel A W muſt have a tube in its lower part for letting out water. This tube muſt have a cock fixed in it, or a valve covered with leather to cloſe its lower end. To haſten the delivery of the water in this veſſel, the external air may be admitted in ſuch manner as is moſt convenient.

The communication between the veſſel and ſyphon being interſected by turning the cocks in the pipes A W, and the branches of the ſyphon cloſed at their lower ends, the tube may be filled with water through an aperture in the top. After this aperture is cloſed, and a ſtream of water let into the ciſtern C for ſupplying the ſyphon, the ends of the branches may be opened, and a continued ſtream will flow through the tube.

When it is required to fill the veſſel A W with water, exclude the external air, and open the pipes between it, and the ſyphon. The veſſel will ſoon be filled, and the water may be let out by opening the tube for that purpoſe, after the ſmall pipes A W are again cloſed by turning their cocks.

The water may be let out of the veſſel without attendance, by a quantity of water paſſing through four veſſels placed in the following order one below another, and each provided with a ſyphon.

1. The higheſt, an immoveable veſſel filled in a given time. 2. A deſcending veſſel, ſuſpended from a lever or wheel, which turns the cocks in the tubes oppoſite A W in its axis. This veſſel muſt have a tube open at both ends, fixed in the middle of its bottom. 3. A deſcending veſſel to open the valve for letting water out of the veſſel A W. It muſt be ſuſpended upon the valve by a cord or wire paſſing through the tube in the middle of the ſecond veſſel. 4. The loweſt, a veſſel of the ſame width with the ſecond. The brim of it muſt be connected to the outſide circumference of the bottom of the ſecond by wires or chains.

In

In this arrangement the firſt veſſel will empty itſelf into the ſecond, which will cloſe the cocks in the pipes oppoſite A and W, before air is admitted into the veſſel A W. The third will be filled from the ſecond, and the water in the veſſel A W will be let out. Again, the third will deliver its contents in the fourth or loweſt, which will keep the cocks in the ſmall pipes oppoſite A and W cloſe, until after the third veſſel is empty, has riſen up, and the external air can no longer enter the veſſel A W. The fourth being then emptied by its ſyphon, the pipes between the veſſel A W and ſyphon S Y will open.

The diameter of the ſecond veſſel ſhould be ſomething leſs than either that above or below it. The fourth ſhould be filled before the ſecond is empty; the third will deſcend the laſt and riſe the firſt: the ſecond and fourth will riſe together immediately after the third. If the ſecond and fourth were to riſe before the third, the ſyphon would directly receive a quantity of external air, and its operations would ceaſe. It will therefore require much caution to manage the cocks and valves, if another veſſel, ſimilar to A W, is to be filled while this laſt is emptied, and emptied while it is filled.

The veſſel A W ſhould not be large, and in order to overcome the buoyance of the extracted air, it is adviceable to make the length of the deſcending branch of the ſyphon exceed the length of the aſcending one as much as circumſtances will admit, and to let the loweſt part of it be made of a conical divergent form. The velocity of the ſtream will be thus encreaſed; the veſſel A W will be ſooner filled with water; and the depreſſion of the two columns will be leſs liable to happen from very ſlight imperfections of workmanſhip.

<div align="center">

I am, SIR,

Your humble ſervant,

</div>

Dalton, Feb. 17, 1801.					WILLIAM CLOSE.

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IV.

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On the Power of penetrating into Space by Teleſcopes; with a comparative Determination of the Extent of that Power in natural Viſion, and in Teleſcopes of various Sizes and Conſtructions; illuſtrated by ſelect Obſervations. By *William Herschel,* LL. D. F. R. S.

<div align="center">

(Continued from page 506.)

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We may now proceed to determine the powers of the inſtruments that have been uſed in my aſtronomical obſervations; but, as this ſubject will be beſt explained by a report of the obſervations themſelves, I ſhall ſelect a ſeries of them for that purpoſe, and relate them in the order which will be moſt illuſtrating.

<div align="center">4</div>

Firſt,

First, with regard to the eye, it is certain that its power, like all our other faculties, is limited by nature, and is regulated by the permanent brightnefs of objects; as has been shewn already, when its extent with reflected light was compared to its exertion on felf-luminous objects. It is further limited on borrowed light, by the occafional state of illumination; for, when that becomes defective at any time, the power of the eye will then be contracted into a narrower compafs; an instance of which is the following :

In the year 1776, when I had erected a telefcope of 20 feet focal length, of the Newtonian construction, one of its effects by trial was, that when towards evening, on account of darknefs, the natural eye could not penetrate far into fpace, the telefcope poffeffed that power fufficiently to fhew, by the dial of a distant church steeple, what o'clock it was, notwithstanding the naked eye could no longer fee the steeple itself. Here I only fpeak of the penetrating power; for, though it might require magnifying power to fee the figures on the dial, it could require none to fee the steeple. Now the aperture of the telefcope being 12 inches, and the construction of the Newtonian form, its penetrating power,

when calculated according to the given formula, will be $\dfrac{\sqrt{,429 \times \overline{120^2 - 15^2}}}{2} = 38,99.$

A, b, and *a,* being all expreffed in tenths of an inch *.

From the refult of this computation it appears, that the circumstance of feeing fo well, in the dufk of the evening, may be eafily accounted for, by a power of this telefcope to penetrate 39 times farther into fpace than the natural eye could reach, with objects fo faintly illuminated.

This obfervation completely refutes an objection to telefcopic vifion, that may be drawn from what has alfo been demonstrated by optical writers: namely, that no telefcope can fhew an object brighter than it is to the naked eye. For, in order to reconcile this optical theory with experience, I have only to fay, that the objection is intirely founded on the fame ambiguity of the word brightnefs that has before been detected. It is perfectly true, that the *intrinfic* illumination of the picture on the retina, which is made by a telefcope, cannot exceed that of natural vifion; but the *abfolute* brightnefs of the magnified picture by which telefcopic vifion is performed, muft exceed that of the picture in natural vifion, in the fame ratio in which the area of the magnified picture exceeds that of the natural one ; fuppofing the *intrinfic* brightnefs of both pictures to be the fame. In our prefent instance, the steeple and clock-dial were rendered vifible by the increafed abfolute brightnefs of the object, which in natural vifion was 15 hundred times inferior to what it was in the telefcope. And this establifhes beyond a doubt, that telefcopic vifion is performed by the abfolute brightnefs of objects; for, in the prefent cafe, I find by computation, that the *intrinfic* brightnefs, fo far from being equal in the telefcope to that of natural vifion, was inferior to it in the ratio of three to feven.

* I have given the figures, in all the following equations of the calculated penetrating powers, in order to fhew the conftructions of my inftruments to thofe who may wifh to be acquainted with them.

The

The diſtinction between magnifying power, and a power of penetrating into ſpace, could not but be felt long ago, though its theory has not been inquired into. This undoubtedly gave riſe to the invention of thoſe very uſeful ſhort teleſcopes called nightglaſſes. When the darkneſs of the evening curtails the natural penetrating power, they come in very ſeaſonably, to the relief of mariners that are on the look-out for objects which it is their intereſt to diſcover. Night-glaſſes, ſuch as they are now generally made, will have a power of penetrating ſix or ſeven times farther into ſpace than the natural eye. For, by the conſtruction of the double eye-glaſs, theſe teleſcopes will magnify 7 or 8 times; and the object glaſs being $2\frac{1}{4}$ inches in diameter, the breadth of the optic pencil will be $3\frac{1}{8}$ or $3\frac{4}{7}$ tenths of an inch. As this cannot enter the eye, on a ſuppoſition of an opening of the iris of 2 tenths, we are obliged to increaſe the value of a, in order to make the teleſcope have its proper effect. Now, whether nature will admit of ſuch an enlargement becomes an object of experiment; but, at all events, a cannot be aſſumed leſs than $\frac{A}{m}$. Then, if x be taken as has been determined for three refractions, we ſhall have

$$\frac{\sqrt{,853 \times 25^2}}{a} = 6,46 \text{ or } 7,39.$$

Soon after the diſcovery of the Georgian planet, a very celebrated obſerver of the heavens, who has added conſiderably to our number of teleſcopic comets and nebulæ, expreſſed his wiſh, in a letter to me, to know by what method I had been led to ſuſpect this object not to be a ſtar, like others of the ſame appearance. I have no doubt but that the inſtrument through which this aſtronomer generally looked out for comets, had a penetrating power much more than ſufficient to ſhew the new planet, ſince even the natural eye will reach it. But here we have an inſtance of the great difference in the effect of the two ſorts of powers of teleſcopes; for, on account of the ſmallneſs of the planet, a different ſort of power, namely, that of magnifying, was required; and, about the time of its diſcovery, I had been remarkably attentive to an improvement of this power, as I happened to be then much in want of it for my very cloſe double ſtars [*].

On examining the nebulæ which had been diſcovered by many celebrated authors, and comparing my obſervations with the account of them in the *Connoiſſance des Temps* for 1783, I found that moſt of thoſe which I could not reſolve into ſtars with inſtruments of a ſmall penetrating power, were eaſily reſolved with teleſcopes of a higher power of this ſort; and, that the effect was not owing to the magnifying power I uſed upon theſe occaſions, will fully appear from the obſervations: for, when the cloſeneſs of the ſtars was ſuch as to require a conſiderable degree of magnifying as well as penetrating power, it always appeared plainly, that the inſtrument which had the higheſt penetrating power reſolved them beſt, provided it had as much of the other power as was required for the purpoſe.

[*] Magnifying powers of 460, 625, 932, 1159, 1504, 2010, 2398, 3168, 4294, 5489, 6450, 6652, were uſed upon ı Bootis, γ Leonis, α Lyræ, &c. See Cat. of double ſtars, Phil. Tranſ. Vol. LXXII. page 115, and 147; and Vol. LXXV. page 48.

Sept.

Sept. 20, 1783, I viewed the nebula between Flamſteed's 99th and 105th Piſcium, diſcovered by Mr. Mechain, in 1780.

" It is not viſible in the finder of my 7-feet teleſcope ; but that of my 20-feet ſhews it."

Oct. 28, 1784, I viewed the ſame object with the 7-feet teleſcope.

" It is extremely faint. With a magnifying power of 120, it ſeems to be a collection of " very ſmall ſtars : I ſee many of them."

At the time of theſe obſervations, my 7-feet teleſcope had only a common finder, with an aperture of the object glaſs of about $\frac{3}{4}$ of an inch in diameter, and a ſingle eye-lens ; therefore its penetrating power was $\dfrac{\sqrt{,899 \times \overline{7,5}\,|^2}}{2} = 3,56$. The finder of the 20-feet inſtrument, being achromatic, had an object glaſs 1,17 inch in diameter: its penetrating power, therefore, was $\dfrac{\sqrt{,85 \times \overline{11,7}\,|^2}}{2} = 4,50$.

Now, that one of them ſhewed the nebula and not the other, can only be aſcribed to ſpace-penetrating power, as both inſtruments were equal in magnifying power, and that ſo low as not to require an achromatic object glaſs to render the image ſufficiently diſtinct.

The 7-feet reflector evidently reached the ſtars of the nebula; but its penetrating and magnifying powers are very conſiderable, as will be ſhewn preſently.

July 30, 1783, I viewed the nebula ſouth preceding Flamſteed's 24 Aquarii, diſcovered by Mr. Maraldi, in 1746.

" In the ſmall *ſweeper* *, this nebula appears like a teleſcopic comet."

Oct. 27, 1794. The ſame nebula with a 7-feet reflector.

" I can ſee that it is a cluſter of ſtars, many of them being viſible."

If we compare the penetrating power of the two inſtruments, we find that we have in the firſt $\dfrac{\sqrt{,41 \times \overline{42^2 - 12^2}}}{2} = 12,84$; and in the latter $\dfrac{\sqrt{,43 \times \overline{63^2 - 12^2}}}{2} = 20,25$.

However, the magnifying power was partly concerned in this inſtance ; for, in the *ſweeper* it was not ſufficient to ſeparate the ſtars properly.

March 4, 1783. With a 7-feet reflector, I viewed the nebula near the 5th Serpentis, diſcovered by Mr. Meſſier, in 1764.

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* The ſmall ſweeper is a Newtonian reflector, of two feet focal length ; and, with an aperture of 4,2 inches, has only a magnifying power of 24, and a field of view 2° 12'. Its diſtinctneſs is ſo perfect, that it will ſhew letters at a moderate diſtance, with a magnifying power of 2000 ; and its movements are ſo convenient, that the eye remains at reſt while the inſtrument makes a ſweep from the horizon to the zenith.

A large one of the ſame conſtruction has an aperture of 9,2 inches, with a focal length of 5 feet 3 inches. It is alſo charged low enough for the eye to take in the whole optic pencil ; and its penetrating power, with a double eye glaſs, is $\dfrac{\sqrt{,41 \times \overline{92^2 - 21^2}}}{2} = 28,57.$

" It

" It has feveral ftars in it; they are however fo fmall that I can but juft perceive fome,
" and fufpect others."

May 31, 1783. The fame nebula with a 10-feet reflector; penetrating power

$$\frac{\sqrt{,43 \times \overline{89^2 - 16^2}}}{2} = 28,67.$$

" With a magnifying power of 250, it is all refolved into ftars; they are very clofe,
" and the appearance is beautiful. With 600, perfectly refolved. There is a confide-
" rable ftar not far from the middle; another not far from one fide, but out of the clufter;
" another pretty bright one; and a great number of fmall ones."

Here we have a cafe where the penetrating power of 20 fell fhort, when 29 refolved the
nebula completely. This object requires alfo great magnifying power to fhew the ftars of
it well; but that power had before been tried, in the 7-feet, as far as 460, without fuc-
cefs, and could only give an indication of its being compofed of ftars; whereas the lower
magnifying power of 250, with a greater penetrating power, in the 10-feet inftrument,
refolved the whole nebula into ftars.

May 3, 1783. I viewed the nebula between ϰ and ϱ Ophiuchi, difcovered by Mr.
Meffier, in 1764.

" With a 10-feet reflector, and a magnifying power of 250, I fee feveral ftars in it,
" and make no doubt a higher power, and more light, will refolve it all into ftars. This
" feems to be a good nebula for the purpofe of eftablifhing the connection between nebulæ
" and clufters of ftars in general."

June 18, 1784. The fame nebula viewed with a large Newtonian 20-feet reflector;

penetrating power $\dfrac{\sqrt{,43 \times \overline{188^2 - 21^2}}}{2} = 61,18$; and a magnifying power of 157.

" A very large and very bright clufter of exceffively compreffed ftars. The ftars are but
" juft vifible, and are of unequal magnitudes: the large ftars are red; and the clufter is a
" miniature of that near Flamfteed's 42d Comæ Berenices. R A 17ʰ 6′ 32″; PD 108° 18′."

Here, a penetrating power of 29, with a magnifying power of 250, would barely fhew
a few ftars; when, in the other inftrument, a power 61 of the firft fort, and only 157 of
the latter, fhewed them completely well.

July 4, 1783. I viewed the nebula between Flamfteed's 25 and 26 Sagittarii, difco-
vered by Abraham Ihle, in 1665.

" With a fmall 20-feet Newtonian telefcope, power 200, it is all refolved into ftars,
" that are very fmall and clofe. There muft be fome hundreds of them. With 350, I
" fee the ftars very plainly; but the nebula is too low in this latitude for fuch a power."

(To be concluded in our next.)

SCIENTIFIC NEWS, ACCOUNTS OF BOOKS, &c.

Sketch of Ornithorhyncus Paradoxus. By M. Blumenbach. See our Journal, Vol. IV. page 506.

THIS animal, fee Pl. XXIV. Fig. 4. was prefented to the author by the Right Hon. Sir Jofeph Banks. It abounds in a lake of New Holland, near Botany Bay. The form of its body, with the exception of the head, is nearly fimilar to that of a fmall otter. Its mouth is large, flat, covered with a naked fkin, and is nearly fimilar to the beak of a duck. The edges of its lower-jaw, are furnifhed, as in the abovementioned fowl, with fmall indentation, refembling the teeth of a faw. But notwithftanding the external re- femblance, the head when diffected refembles that of other quadrupeds, but with this re- markable anomaly, that the two intermaxillary bones have an interval between them, which is filled only by cartilages. A great number of nerves, proceeding from the fifth pair, are diftributed to the beak, and afford the animal all the fenfibility neceffary to feek its proper food at the bottom of the waters it inhabits.

This creature, from the form of its body, the fhortnefs of its feet, and the membranes which unite its toes, is in fome degree analogous to the *porcupine ant-eater, myrmecophaga aculeata* of Shaw, Natur. Mifcell. No. 36. *Echidna* Cuv. tab. Zool. But it differs confi- derably from it in the form of its mouth, and the nature of its integuments. It is proper to obferve, that the family of toothlefs animals, of which few fpecies were known in the old world, is found to have feveral reprefentatives in the vaft continent of New Holland: thus alfo the clafs of didelphia may be faid to exift in only few fpecimens in America and the Indies, compared to the various forms under which they are met with in this newly difcovered country.

Bulletin de Sciences, An. 8. *No.* 39.

On a new Variety of Zircon, by Citizen Hauy.

The chryftals of Zircon which have hitherto been found in Ceylon, France, and elfe- where, have been tranfported by water into their different fituations, and we had no indi- cation of their native place, nor of the fubftances which ferved them either for fupport or matrix. The interefting travels which have lately been made by Citizen Lafterie into Sweden and Norway, have given us the knowledge of the primitive fituations of this kind of mineral. Amongft the objects of natural hiftory which he has collected, was a granite found at Fridichfwern in Norway, and compofed of red feld-fpath and amphibolite with brown chryftals, known in that country by the name of Vefuvian, which name has been

given

given by the celebrated Werner to the fubftance which we call *Idocrafis*. Citizen Hauy has difcovered that thefe cryftals effentially differ in ftructure, as well as in their other characteriftics, both from idocrafis and from brown tin grains, with which, at firft fight, they might be confounded; and that they belong to the zircon, of which they prefent a new variety. Their fragments expofed to the flame of a candle, inftantly lofe their colour, and this alfo is the cafe with fragments of zircon. Their primitive fhape, indicated by the direction of the natural junctures, is a rectangular octahedron, (Plate XXIV. Fig. 3,) having the fame angles as thofe of the zircon, and like it, divifible by planes, which proceeding from the fummits, coincide with the bafes of the triangles, which form the faces of the octahedron.

The variety of which he treats, and which is reprefented by Fig. 3, has thirty-fix faces. Its fign is $\frac{D\ D^2\ E^2\ P}{i\ u\ \varkappa\ P}$. Citizen Hauy calls it the *fubtractive zircon*, a denomination which he has adopted in cafes, like the prefent, where one of the exponents which accompany the indicative letters, the letter E for inftance, is lefs by an unity than the fum of the other exponents.

The following are the meafures of the principal angles determined by the help of theoretical calculation. Incidence of *l* upon *l*, 90^d, of P upon *l*, 131^d 25$'$ of $\varkappa$ upon P, 150^d 5$'$, and upon *l*, 142^d 55$'$, of *u* upon *l*, 159^d 17$'$, and upon P 152^d 8$'$.

The length of one of thefe cryftals taken between the fummits of the two pyramids is 18 millimetres, and the thicknefs 8 millimetres. Their colour is brown, mixed with orange; they are tranfparent, and they contain brilliant fpangles, interfperfed through their fubftance, which give the appearance of an aventurine.

Bulletin des Sciences, No. 39, *p.* 16.

Extract of a Letter from CIT. HECTH, *jun. Apothecary at Strafburg, to* CIT. VAUQUELIN.

I lately received a letter from M. Klaproth, in which he informs me, that having purchafed at the fhop of M. Hahneman, a certain quantity of the new kind of fixed alkali, which he pretends to have difcovered, he found, by analyfis, that it was nothing but refined borax.

Thus it is found, that this new fixed alkali, fo pompoufly announced in the Journals by M. Hahneman, under the name of *pneum*, and fold by him for twenty francs the ounce, is reduced by the analyfis of M. Klaproth to its proper value.

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Fig. 1.

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Fig. 4.

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TO THE

FOURTH VOLUME.

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